Conservatism or conservation?: Political behavior responsiveness to experienced environmental threats

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Abstract

How do mass publics respond to threat exposure? From one perspective, crises make specific policy problems salient to impacted individuals, and thus increase political incentives to address realized social and economic threats. However, a growing political psychology literature argues that individuals become more conservative in response to threat and uncertainty, an effect linked to the putative role of conservative ideology in coping with fear, anxiety and uncertainty (Jost et al., 2003). We evaluate threat responsiveness on political behavior by exploiting the relatively random spatial distribution of Californian wildfires. Using an new panel dataset of census block group wildfire exposure and election outcomes, we find that wildfire exposure *increases* pro-environmental voting behavior. By contrast, wildfire induces more conservative voting behavior on such other ideological but non-environmental issues as support for abortion and stronger criminal penalties.

Patterns of social and political response to threat exposure have far-reaching consequences for social mobilization around such varied threats as terrorism and climate change. For example, policy reforms to manage the threat of dangerous, human-caused climate change remain stunted. This policy inaction has been linked, in part, to public lack of concern with (Druckman, 2013; Hughes and Urpelainen, 2015). An optimistic interpretation might hypothesize that public support for climate policy action will naturally accelerate as the impacts of climate change continue to realize. However, the "conservatism-under-threat" hypothesis suggests the opposite by emphasizing a systematic tendency to privilege one particular ideologically position during moments of political crisis (Jost et al., 2003). This is particularly the case in policy domains as climate change where the more conservative policy position (e.g. climate change skepticism) are at odds with optimal risk mitigation strategies.

In this draft working paper, we offer an original panel analysis of political responses to experienced threat. This paper serves as one of the first tests of the conservatism under threat

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hypothesis in a field setting. It also offers a direct evaluation of whether publics who experienced environmental threats translate those experiences into greater support for environmental policy reforms. Specifically. we evaluate whether California residents who experience a wildfire between elections hold more more pro-environment preferences narrowly, and more conservative policy preferences generally. We find that wildfire exposure *increases* pro-environmental voting behavior in subsequent elections, even as threat exposure appears to drive more conservative policy preferences overall. The divergent direction of these results confirm theoretical predictions and make the presence of a systematic unobserved confounder less likely. Nonetheless, we also discuss remaining identification concerns and outline planned next steps for this analysis.

1 Public and political responses to experienced threats

Political psychologists have begun to amass substantial evidence that individuals adopt more conservative preferences under conditions of threat or uncertainty (Wilson, 2013; Jost et al., 2007, 2003; Nail et al., 2009; Bonanno and Jost, 2006; Jost and Napier, 2012). For instance, lab experiments find that liberals adopt more conservative cognitive styles and political preferences under threat (Nail et al., 2009), even after controlling for such general psychological traits as close-mindedness (Jost et al., 2007). Similarly, panel studies of college students have found that shifts towards more politically conservative worldviews are associated with the development of realistic threat perceptions and intergroup anxiety (Matthews et al., 2009). The conservatism-under-threat hypothesis is often explained through an emphasis on the potential cognitive benefits of holding conservative worldviews; in particular, conservatism' emphasis on stability and hierarchy may provide psychological reassurance and comfort for individuals coping with uncertainty and threat (Jost et al., 2003). By interpreting conservatism as a response to cognitive needs, these accounts complement a literature linking order and purity primes to conservative preference shifts (Helzer and Pizarro, 2011).[1]

[1]The conservatism-under-threat hypothesis suggests that threats will induce a one-directional shift towards conservatism irrespective of an individual's prior ideological disposition. Under an alternative two-directional account, the cognitive need to manage uncertainty is equally satisfied by ideological thinking of any variety; from this perspective, individuals will become more ideologically extreme when faced with threat, rather than more conservative. For example, an early expression of this hypothesis is provided by Rokeach (1960) who was responding to an earlier generation of literature on the personality correlates of authoritarianism. This *ideological-extremism-under-threat* hypothesis has been most forcefully advanced by advocates of the evocatively-titled terror management theory (TMT), who emphasize the ways in which mortality primes induce worldview defense, ingroup solidarity, and the salience of symbols associated with an individual's ideology (Greenberg et al., 1990; Arndt et al., 2002). For example, TMT scholars explore the political ramifications of the death of a loved one; while they find that loss strongly induces conservatism in many individuals, they also find evidence that some liberals under threat become more strongly committed to their liberal worldview (Chatard et al., 2010). Other scholars point more generally to the importance of uncertainty in driving in-group identification (Hogg et al., 2007). However, direct tests of the one-directional hypothesis against the two-directional hypotheses have strongly supported the one-directional account (Jost et al., 2007). Terror management theory has also been

This "conservatism-under-threat" hypothesis has far-reaching consequences for social mobilization around such varied threats as nuclear proliferation and climate change because it suggests a systematic tendency to privilege one particular ideologically position during moments of political crisis. Critically, even if conservative ideologies provide individual psychological benefits, there is no guarantee that the conservative policies supported by these preferences will provide society with optimal policy outcomes. If individuals react in systematic ways to uncertainty and threat, this may constrain the range of beliefs and ideas that political elites and mass publics will entertain during crises. However, there are a number of reasons we might be skeptical about this intriguing and politically consequential psychological hypothesis.

First, we might expect that experienced threats heighten the salience of related social and economic risks, irrespective of the ideological content associated with a policy response (Slovic and Weber, 2013). For instance, agency decision-makers allocate wildfire risk reduction resources close to places where population-adjacent wildfires have recently occurred, rather than where future risks are most severe (Wibbenmeyer et al., 2016). Climate and environmental scholars have placed particular emphasis on this type of link between individual exposure and climate and energy policy preferences (Whitmarsh, 2008; Brody et al., 2008; Zahran et al., 2006). An increasing number of cross-sectional studies find evidence for links between experienced weather and climate beliefs (Egan and Mullin, 2012; Spence et al., 2011; Hamilton and Stampone, 2013; Herrnstadt and Muehlegger, 2014; Howe and Leiserowitz, 2013; Myers et al., 2013), although panel analysis has not found evidence that extreme weather experiences can explain shifting US climate beliefs (Mildenberger and Leiserowitz, 2017).[2] For example, Spence et al. (2011) find that flood experiences are associated with higher belief in climate change and a higher willingness to save energy.

Second, an individual tendency to become more conservative under threat may be overshadowed by a wider range of political and social factors that influence political preferences during crises. A growing literature in political science on the political implications of natural disasters identifies significant shifts in public evaluation of government performance after natural disasters. Bechtel and Hainmueller (2011) find that, in areas impacted by catastrophic 2002 floods

criticized by studies that argue uncertainty primes rather than mortality primes are the most important trigger of preference shifts (van den Bos et al., 2005).

[2]There is also disagreement over the types of weather fluctuations which drive belief shifts and the persistence of these heuristic cues. For instance, Egan and Mullin (2012) argue that the effects of temperature fluctuations only last a few weeks. By contrast, Deryugina (2013) finds that only medium-to-long term temperature fluctations (1 month to 1 year) shape beliefs about climate change. There is also disagreement as to which respondents are most sensitive to weather flyctations. Hamilton and Stampone (2013) finds the effects of weather strongest among Independents. However, Egan and Mullin find effects concentrated in individuals who lean Democrat or Republican but *not* independents and Deryugina, albeit using a distinct dependent variable, finds the strongest effects among conservatives.

of the river Elbe, the German electorate rewarded the governing SPD for their effective disaster response. Likewise, studying public evaluations of the political response to Hurricane Katrina, Malhotra and Kuo (2008) suggest that party cues induce partisans to blame officials from the opposing party but that individuals still make principled evaluations of disaster management. Relatedly, a separate literature has evaluated the specific impact of Hurricane Sandy on US political behaviour, finding mixed results on increased support for President Obama in storm-affected areas (Velez and Martin, 2013; Hart, 2014; Debbage et al., 2014). Relatedly, a similar limitation shapes the literature on belief and attitude responsiveness to climate risks; we still lack a clear understanding of these public opinion differences structure political behavior in a meaningful fashion.

Third, much of the work on ideological responses to threat and uncertainty has been largely confined to laboratory settings, and its relevance to real world events remains largely unexamined. Ideological preference shifts under conditions of threat may be ephemeral, decaying quickly outside of the lab. Alternatively, shifts may prove insignificant in the face of broader social and political forces that structure political attitudes over time. Even if real world threats do induce threat-related shifts in political preferences, those shifts may not lead to consequential changes in real-world political behavior.

The major setting of which we are aware that has engendered field evaluation of the conservatism-under-threat hypothesis is research evaluating the effects of the September 11th terrorist attacks on political attitudes and behaviours. Bonanno and Jost (2006) study political beliefs among high-exposure 9/11 survivors and find a shift towards conservatism associated with elevated levels of PTSD and depression. In a more recent study, Hersh (2013) tracks shifts in political behaviour amongst families or neighbours of people who lost their lives in the attack. He finds a long-term persistent increases in political behaviour, and a systematic shift among the family and friends of attack victims towards identification with and participation in the Republican party. Because Hersh exploits the random nature of death in a terrorist attack and matches victims against otherwise similar populations who were unaffected by the attack, his study comes the closest to providing externally valid evidence for the conservatism-under-threat hypothesis. However, victim shifts towards identification with the Republican party may not be a function of the party's ideological position. Because the political and military response to the attacks were coordinated by Republican political leaders, who happened to be in power at the time of the attack, a shift by victims towards the Republican party could simply entail increased identification with a party perceived as effective in responding to a personally salient crisis.

In other words, these 9/11 studies are unable to disentangle shifting voter evaluation of elected official performance from shifting ideological preferences. By extension, additional research is necessary in empirical settings where 1) a threat response demands policymaking in a perceived *liberal* direction; and 2) a research design that can tease apart evaluation of political leaders from the ideological content of their proposed policy programs.

The study of environmental preferences in response to environmental threats provides just such a setting where public responsiveness may be cross-pressured. This is because more conservative ideological positions tend to down-weight the risk of human-caused climate change. To the degree that climate concerns have become bundled with left-leaning partisans in a US context, the conservatism-under-threat hypothesis suggests a plausible scenario where mass publics may *reduce* their climate beliefs exactly at the moment when policy action may become most urgent. Conversely, direct public demands for threat mitigation may generate pro-environment voting behavior irrespective of hypothesized threat-linked ideological shifts.

In this paper, we first examine the effect of wildfires on pro-environmental support narrowly, and then in terms of ideological support broadly. Our approach offers three major improvements over prior work. First, existing well-identified estimates are generally limited to laboratory experiments to ensure the randomization of threat-related treatments. In our case, we consider a real world salient threat – wildfire at the census block group level. Second, as opposed to work using survey-measured outcomes, the outcome of interest here is a behavioral measure, of direct real-world relevance – voting on ballot initiatives. Furthermore, by focusing on ballot-initiatives rather than vote shares for individual candidates, we are able to examine the effect of wildfires on policy preferences as such, rather than their effect on evaluations of incumbents. Finally, despite using a real-world environmental threat which was not randomized by the investigator, we aim to provide a maximally credible estimate of the causal effect. We do so by invoking the assumption that while some census blocks may be more or less vulnerable to wildfire than others, the year in which a wildfire occurs is likely independent of environmental or other policy attitudes at the time. We discuss these methodological and identification issues in further detail below.

2 Data & Methods

We prepare an original panel dataset of political and wildfire data between 2002 and 2014 in the State of California.

Base Panel Construction

First, we collect electoral outcome data and voter registration data at the voting precinct level in California for all national elections between 2002 and 2014, drawing from official data published by the California Secretary of State. The electoral precinct level is the smallest unit with available electoral return data in California. This includes voting-precinct level data on all ballot measures considered by Californians over this time period. However, Californian voting geographies and identifiers change on an election by election basis, blocking 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.

Instead, we project voting precinct data into 2000 census block group geographies as our panel unit of analysis. The California Secretary of State publishes conversion files on a bi-annual basis that specify what percentage of each voting precinct falls within each census block group. This allows conversion of precinct-level electoral and voter registration data to census-block group geographies. Note that these conversion factors are based on geographic overlap between voting precinct and census block group geographies. As such, to convert results from the electoral precinct level to a census block group level, we must assume that votes are distributed uniformly across the voting precinct geography. Formally, we calculate census block-level electoral outcomes by calculating the area-weighted sum:

$$X_{it} = \Sigma \theta_{jt} X_{jt}$$

Here, i is any one particular Californian census block, j_t is an index whose length varies across different census blocks i of voting precincts that spatially overlap census block i in given time period t, θ_{jt} gives the fraction of a given voting precinct, j_t that falls within census block i at time t, X_{jt} gives the electoral outcome or political measurement of interest for precinct j_t, and X_{it} gives the outcome of interest for a given census block i in a given time period t. Recall that the number and shape of voting precincts (j) varies over each election.

From the 2002 through 2010 elections, the California Secretary of State publishes conversion files linking voting precinct geographies to 2000 Census block group geographies. From the 2012 elections onward, these conversion files link voting geographies to the 2010 census block groups. For these elections, we first project voting precinct-level measurements into 2010 block group geographies and then, project 2010 census block group measurements into 2000 census block

Election	Treated N	Control N
2006	21199	921
2008	21025	1099
2010	21377	734
2012	22736	422
2014	22515	634

Table 1: Treatment frequency by election cycle

group geographies using conversion files prepared by the US Census Bureau. The result is panel of Californian electoral data that projects all relevant political data into a standardized political geography to allow for over-time comparisons.

Treatment Measurement: Wildfire Data

We then collected on wildfire incidence on an annual basis between 2000 and 2014. We use Collection 6 Terra and Aqua Moderate Resolution Imaging Spectrometer (MODIS) active fire measurements at the 1km pixel level. MODIS uses satellite data to evaluate middle-infrared and thermal brightness to evaluate presence of active fires (Giglio et al., 2003).[3] We use USDA Forest Service MODIS data for California. This data gives every square kilometer region of California which experienced a wildfire on an annual basis.

We performed a spatial merge of wildfire data on census block group data to develop an indicator of which census block groups were treated with a wildfire in any given year. We then aggregated from these indicators to treatment indicators for each observation in the panel, indicating whether a given census block group had been treated by a wildfire in the two-year period preceding a given election.[4]

We also calculated the minimum distance between each census block group and a wildfire during each two-year election cycle.

DV Measurement: Ballot Initiatives

Our primary dependent variables are ballot measure electoral outcomes. Californians rarely vote on identical ballot measures across different election cycles; however, they often vote on ballot measures that are substantially similar in their intent and their policy implications. For our core analysis, we group similar ballot measures. For each of these policy domains, we calculate the percentage support for the relevant policy-measure for each election. This measurement

[3]MODIS active fire data also corrects for false positives by comparing potential thermal fire observations against neighboring pixel brightness.

[4]Biannual elections occur in early November. This introduces some limited error into our analysis since some "treated units" will receive their treatment *after* the election. However, this error should bias our result toward 0, by making our control groups look more like our treatment groups.

strategy assumes that voting preferences for each ballot initiative reflects a latent orientation towards the underlying political preference. We confirm that our coded Conservative position on each ballot initiative is negatively correlated with Democratic vote share for all ballot initiatives in all elections where they were on the ballot.

Here, we describe each clustering of ballot measures.

Environmental Votes: Between 2002 and 2014, Californians voted on four ballot initiatives related to environment. In 2006, Californians voted on Proposition 87 (Alternative Energy. Research, Production, Incentives. Tax on California Oil Producers.). This ballot initiative proposed a new $4 billion dollar program to support clean energy alternatives, funded by a 1.5% to 6% on Californian oil producers. The proposition was rejected 55% to 45%. In 2008, Californians voted on Proposition 10 (The California Alternative Fuels Initiative). This proposition proposed a support program for research, education and deployment of alternative fuel technologies. The proposition was rejected 59% to 41%. Californians also voted on Proposition 7 (Standards for Renewable Resource Portfolios). This ballot initiative proposed to require increased utility purchases of renewable energy. It 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. Voters rejected this proposition 62% to 38%. We then calculated the percentage support for the environmental ballot initiative for each election.[5]

Criminal sentencing: Between 2002 and 2014, Californians voted on two ballot initiatives that addressed criminal sentencing. In 2004, the public voted on Proposition 66 (Changes in the 'Three Strikes Law'). This initiative proposed to weaken California's sentencing guidelines for many crimes. This proposition was rejected by 53% to 47%. In 2006, Californians voted on Proposition 83 (Jessica's Law [on Sex Offender Regulation in California]). This proposition proposed substantial new restrictions on sex offenders, including GPS monitoring on parole. It was approved 70% to 30%. We consider opposition to Proposition 66 and support for Proposition 83 to be the pro-conservative positions.

Abortion rules: Between 2002 and 2014, Californians voted on two ballot initiatives that addressed issues pertaining to abortion rules. In 2006, Californians voted on Proposition 85 (Parental Notification Initiative), which would have required parents be notified prior to in-state abortions by minors. The proposition was defeated with 47% in favor and 53% opposed. In

[5]For Proposition 23 (2010), a vote *against* the ballot initiative was the pro-environment position

2008, Californians voted on a new Proposition 4 that would have similarly required a waiting period before receiving an abortion as a minor conditional on parental notification. Proposition 4 was also defeated with 48% in support and 52% in opposition.

Housing bonds: We can also construct one measure of public support for a ballot initiative outcome with far weaker ideological content: supporting for housing bonds. In 2002, Californians voted on Proposition 46 (Bonds for Housing Projects) to create a $2 billion dollar fund to support housing development. This proposition was approved 58% to 42%. In 2006, Californians voted on Proposition 1C (Bonds for Housing), a similar $2 billion effort to fund low-income and senior housing.

2.1 Identification Strategy

Our identification strategy relies on the assumed absence of unobserved time-varying confounders in our California wildfire panel. Consider a particular block group-level outcome in a given year, Y_{it}, such as the percent of the block group that votes in a pro-environmental fashion on a given ballot initiative. We consider a wildfire "treatment", for each block preceding each period, $wildfire2yr_{it}$, indicating whether a wildfire occurred within that electoral precinct in the preceding two year period. For convenience we may say "wildfire occurred in period t" to indicate that wildfire actually occurred in the two years preceding the election in period t. Finally, we designate potential outcomes: the observed outcome Y_{it} is a realization of a non-treatment (or non-wildfire) potential outcome, $Y(0)_{it}$, when no wildfire occurred ($wildfire2yr_{it} = 0$). By contrast, Y_{it} is equal to $Y(1)_{it}$ – the treatment/wildfire potential outcome if block i did experience a wildfire preceding year t ($wildfire2yr_{it} = 1$).[6]

In a traditional difference-in-difference setup, we would consider units with wildfires in neither of the two years (controls) and those with wildfire in the second year (treated).[7] We would then assume that the expected trend in non-wildfire outcomes, $\mathbb{E}[Y(0)_{i,t=1} - Y(0)_{i,t=0}]$, looks the same for the units that received wildfire in the second period and those that did not. If so, a simple regression with unit and year fixed effects provides an average treatment effect among the treated,

[6]This notation implies that the treatment status of other units – or even the same unit at a different time – is irrelevant, as in the Stable Unit Treatment Value Assumption. However, we can contemplate various violations of this, such as the effects of spatial or temporal spillovers, which we discuss below as issues arise.

[7]Note that, the data of interest regarding any given outcome variables will contain only the periods in which the constituent ballot measures occur. For example ballot measures that appear in two periods only, the resulting dataset of interest contains only two periods ($t = \{0, 1\}$).

$$Y_{it} = \gamma_i + \omega_t + \alpha D_{it} + \eta_{it} \tag{1}$$

where $D_{it} = 1$ only when $wildfire2yr_{it}$ and $t = 1$ (the second period).

However, the parallel-trends assumption (and the two-way panel fixed effects estimator for it) can be generalized to allow units that had wildfire in the first year but not in the second, simply redefining D_{it} in equation 1.[8]

We can then generalize to outcomes that are measured in three or more periods rather than only two, again using a model as in Equation 1. The parallel trends assumption remains, though these trends are now more accurately characterized as"parallel trajectories".

Finally, the parallel trajectories assumption is isomorphic to the traditional logic of panel fixed effects in an econometric framework in which we speak of "no unobserved time-varying confounders." Specifically, any time invariant feature of the blocks that could be confounded is absorbed by the unit fixed effects, γ_i. Any period-specific shocks are covered by the period fixed effected, ω_t. If any feature that varies over time and by unit makes some units both more likely to experience wildfire and have a high (or low) outcome, it would appear as a component of η_{it} that is correlated with D_{it}, Hence the classical condition, $cov(D_{it}, \eta_{it}) = 0$, for consistent estimation of α as a causal effect of D_{it}. [9]

[8]We do note that allowing units to "turn off" wildfire treatment as well as the usual "turning on" remains valid but imposes an additional burden on the parallel trends assumption. Specifically, consider a unit that had wildfire in period $t = 0$, but not in period $t = 1$. If there is a lingering effect of the first period's wildfire such that even by the second period, it's effect is felt, then the $Y(0)_{i,t=1}$ for unit i will be different than it would have been in the absence of a first-period wildfire. Suppose for example that wildfire increases the outcome, and that this increase spills-over temporally into the second period, making $Y(0)_{i,t=1}$ higher than it would otherwise be. This type of spillover will generally reduce the estimated effect: for this unit, we use the outcome in the "treated" minus it's outcome in the "control" period, which is made smaller by temporal spillover of this type. This is analogous to the "partial treatment" logic, and thus makes our estimates conservative. In order for such a temporal spillover to exaggerate our effects, a wildfire's impact on the subsequent period would have to be opposite in direction to its impact on the first period. We view this as extremely unlikely in the current empirical setting.

[9]To make the isomorphism between this asumption and the "parallel trajectories" assumption more evident: were some time-varying influence to come along, it would change the outcome without changing treatment status, thus changing $Y(0)_{it}$. For it to be a confounder, it would have to change $Y(0)_{it}$ in a different way for units treated in a given year than for those that are not. This would break the parallel trajectory assumption. We note that the parallel trajectory assumption is stated in terms of $Y(0)$ without making claims on $Y(1)$, because difference-in-difference identifies the Average Treatment Effect on the treated (ATT): $Y(1)$ is taken from the treated unit and the parallel trends/trajectory assumption helps to fill in for the missing $Y(0)$ of those treated units. In the econometrics framework when speaking of "no-unobserved confounders," a constant treatment effect (α) is typically the object of inference, so no distinction needs be made between an Average Treatment Effect (ATE) and an ATT.

3 Results and Discussion

3.1 Environmental Voting

For each year in the data, between 2% and 5% of blocks experienced wildfire in the two preceding years (see 1). 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	-0.147^{***}	-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 blocks 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 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. 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. To further verify this, column (5) in Table 2 uses an indicator of wildfire preceding the *next* election to explain variation in environmental support in the current election, and finds a similarly strong relationship.

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. If we examine the correlation between wildfire occurrence and lagged democratic voteshare (lagging to ensure we are not seeing an effect of wildfire, akin to model 5 in Table 2), we are restricted to elections in and between 2004 to 2012.

Table 3: Cross-Sectional Comparison of Democratic Vote Share and Wildfire

	Dependent variable:				
	voteshare_dem_l2				
	2004	2006	2008	2010	2012
	(1)	(2)	(3)	(4)	(5)
wildfire2yr	−0.234***	−0.241***	−0.226***	−0.218***	−0.231***
	(0.007)	(0.008)	(0.007)	(0.009)	(0.009)
Constant	0.597***	0.610***	0.661***	0.673***	0.598***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	21,915	22,086	22,096	22,107	22,125
R^2	0.047	0.036	0.040	0.025	0.030
Note:			*p<0.1; **p<0.05; ***p<0.01		

As expected, the associated between places that get wildfire and Democratic vote share (in the *prior* election) is strong and negative, with blocks having wildfire at time t being 22 to 24 percentage points lower on Democratic vote share at time $t-1$ than those without wildfire.

We have thus far simply established that the types of places experiencing wildfire are very different from those that don't, and in particular, are far more conservative and thus on average less supportive of environmental measures. However an appropriate strategy for identifying the *effect* of wildfire on environmental behavior can – and in our case, will – reveal an effect in the opposite direction.

3.2 Effects on Environmental Voting

Turning to our estimation strategy for the effect of wildfire on environmental voting, we first examine whether a two way (block and year) fixed effects approach is suitable. We first check whether the most troubling potential confounder we can think of – Democratic vote share – is "balanced" under this approach. That is, in a panel model with block and year fixed effects, we treat the onset of wildfire as effectively random. If correct, then wildfire at year t should be unassociated with Democratic vote share at year $t-1$ once these fixed effects are included.

Table 4: Conditional Balance on Democratic Vote Share

	Dependent variable:
	voteshare_dem_l2
wildfire2yr	−0.003
	(0.002)
Year=2006	0.016***
	(0.001)
Year=2008	0.067***
	(0.001)
Year=2010	0.082***
	(0.001)
Year=2012	0.008***
	(0.001)
Observations	110,329
R^2	0.155
Note:	*p<0.1; **p<0.05; ***p<0.01

Table 4 shows results for a two way panel fixed effect model consistent with our approach, but which simply uses lagged democratic vote share as if it were the outcome, and with standard errors clustered by block. Conditional on block and year fixed effects, we find no detectable relationship between wildfire incidence in year t and Democratic vote share in year $t-1$. With this, we turn to estimating the effect of interest.

We show our main estimates in Table 5. The first model is the panel model with block and year fixed effects, taking wildfire in the preceding period as the election and the environmental ballot initiative votes per block as the outcome. Standard errors are clustered by block. We find that wildfire in the two years preceding an election is associated with a 1.2 percentage point increase in the proportion of pro-environment votes. The second model in Table 5 is similar,

Table 5: Effects on Environmental Voting

	DV: fraction pro-environmental vote		
	twoway panel	with voteshare	placebo
	(1)	(2)	(3)
wildfire2yr	**0.012*****	**0.013*****	0.012***
wildfire2yr_f2			$-$**0.004**
(placebo)			(0.002)
voteshare_dem_l2		0.032***	0.032***
		(0.004)	(0.004)
Year=2008	$-$0.080***	$-$0.081***	$-$0.081***
	(0.001)	(0.001)	(0.001)
Year=2010	0.153***	0.151***	0.151***
	(0.0004)	(0.0004)	(0.0004)
Note:			*p<0.1; **p<0.05; ***p<0.01

but adding the potential confounder we are most worried about – Democratic vote share – to the model with a one-election (two year) lag. As we have shown vote share is uncorrelated with wildfire conditional on the fixed effects, it should not change the effect estimate – which is what we see here.

Finally, Model (3) in Table 5 offers as a placebo test through the coefficient on an indicator for *future* wildfire. The outcome variable here is still environmental support in the current election, but now the main coefficient of interest is an indicator for wildfire in the *future* two years, *wildfire2yr_f2*. If there is no confounding (or reverse causality from environmental support to wildfire), then a future wildfire should not appear to influence current pro-environmental behavior. While the coefficient comes out farther from zero than would be ideal, it has a negative rather than positive sign, and is not significant at conventional levels ($p = .11$). Thus, the placebo test is "passed", but not with confidence. We hope that once we collect additional data, this coefficient will become statistically weaker rather than gain significance.[10]

[10]In some cases the ideal placebo tests would be to regress the current outcome on future wildfire (with the necessary fixed effects or other controls), without re-including recent wildfire (*wildfire2y*). However this approach fails in the panel setting whenever there is autocorrelation in the treatment (wildfire). Wildfire shows strong negative auto-correlation within block groups, because once a wildfire occurs, another wildfire in the same block group becomes far less likely. This is because there is less biomass to burn again. This negative auto-correlation makes "future wildfire" actually predictive of "recent wildfire". Thus one can obtain a significant relationship between future wildfire and the current outcome, even in the absence of a confounder (or reverse causality for that matter). Hence we estimate the effect of future wildfire on the current outcome, conditional on recent wildfire to solve this problem. This makes our test slightly weaker than we'd like, as a null result could simply imply that recent wildfire is the better predictor of the outcome than future wildfire. We thus seek to add additional placebo tests.

3.3 Effects on Measures of Conservatism

Of the other ideologically oriented ballot measures we identified, only three occurred in multiple elections for which we have wildfire data, the outcome, and lagged Democratic vote share. These were measures regarding the criminal sentencing (Proposition 66 in 2004; Proposition 83 in 2006) abortion (Proposition 85 in 2006; Proposition 4 in 2008), and housing bonds (Proposition 46 in 20023 and Proposition 1C in 2006). If threat such as wildfire more commonly has a pro-conservative effect on attitudes, we would expect to see a positive coefficient when regarding the criminal sentencing (because it is oriented so that positive is "tougher"), a negative effect on abortion support, and a null effect on the less ideological issue of housing. Table 6 presents results that conform to theoretical predictions.[11] These results would appear to provide empirical confirmation in a field setting of the conservatism-under-threat hypothesis. Moreover, they emphasize that the public's pro-environmental shift in response to a wildfire threat is sufficiently strong to counteract this broader tendency to move away from an ideological position (liberalism) currently associated with pro-climate voting behavior in the United States.

We do not, however, see an effect of wildfire on Democratic vote share itself, Model (3). This is unsurprising as the intention to vote for a candidate is likely to be much more resistant to change than ballot measures. The non-significance of this coefficient however does give a degree of support to our assumption that the strong confounding described in Table 2 has been conditioned out by the estimation approach. That we get theoretically-consistent results while using a variety of measures which each occur over different elections and time-frames also provides some confidence that our results are not the artifact of a particular election or arbitrary distribution of wildfires at a given moment in time.

4 Next Steps

The arbitrary distribution of Californian wildfires provides a new empirical setting to evaluate the effect of environmental threats on political behavior. Do real-world environmental threats such as wildfires meaningfully induce shifts in pro-environment political behavior? And if these effects occur, how do they compare to the effects of such threatening events on other political attitudes as predicted by the conservatism-under-threat hypothesis?

According to our estimates, blocks experiencing wildfire in the prior year show higher support

[11] Note that we cannot include Democratic voteshare as a covariate in our analysis of housing bonds because we do not yet have data for 2000 Democratic voteshare (necessary such that the voteshare measurement is not post-treatment).

Table 6: Effects on Measures of Conservatism

	Dependent variable:			
	Measure type			
	Sentencing	Abortion	Housing Bond	Dem Vote Share
	(1)	(2)	(3)	(4)
wildfire2yr	**0.017*****	**−0.004*****	**−0.002**	**−0.004**
	(0.004)	(0.001)	(0.002)	(0.003)
voteshare_dem_l2	−0.013**	0.021***		−0.266***
	(0.006)	(0.002)		(0.004)
Year=2006	0.198***		0.003***	0.055***
	(0.001)		(0.0003)	(0.001)
Year=2008		0.027***		0.084***
		(0.0003)		(0.001)
Year=2010				0.014***
				(0.001)
Note:				*p<0.1; **p<0.05; ***p<0.01

for environmental ballot measures by 1.3 percentage points. We regard this as large enough to be meaningful, while being quite plausible. In addition, we also a pro-conservative effect of wildfire on non-environmental ideological measures. Both effects move beyond existing literature whose focus has often been on attitudes and opinions. Instead, we find evidence of threat exposure on realized political outcomes. Further, we find a null effect on a seperate ballot initiative issue (housing bonds) that we might expect has weaker ideological content.

While our constellations of findings comports with both the expected impacts of environmental threats and the conservatism under threat hypothesis, this combination of findings is also particularly difficult to explain away by any confounder. For our results to be an artifact of an omitted variable, we would need to identify a confounder that would areas that experience wildfires more liberal on environmental issues *and* more conservative on other ideological issues.

One remaining concern is that, even while difficult to identify time-varying confounders that would produce pro-conservative effect estimates on various ballot measures but an anti-conservative effect on environmental measures, we are relying on very few time periods. The environmental measures come from three elections, and the ideological ones from just two each. While any global shocks pertaining to one election versus another should be absorbed by the year fixed effects, it is possible that chance shocks occurred in these years that happened to be distributed differently across places experiencing wildfire and those not experiencing it. Yet, the fact that our effects are distributed across a range of different election years should somewhat moderate this concern.

To this end, further refinement of this research will demand several extensions First, data from the 2016 election, will allow us to extend our analysis (additional time periods for some issues and several new issues). This is a function of the extensive set of ballot initiatives that were included on the ballot in 2016, many of which revisited topics that had previously been voted on since 2002.

Second, we will undertake additional placebo tests, to probe potential failures of our identification strategy. Third, we will examine alternative definitions of the wildfire treatment, including 1 or 3 years windows, and a measure of the distance to the nearest wildfire. Fourth, we are working on producing additional covariate data at the census block group level (race, education, and income) distributions to better evaluate balance.[12] Finally, rather than measure ideological ballot measures across the few issues areas for which we could construct a panel, we may consider creating a latent ideological score for each block, across all issues voted on in that block in a given election cycle.

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[12]Note that we are engaged in within-precinct analysis here. For such covariates to explain outcomes, we would need that shifts across time in education or income were correlated with both wildfire exposure and ballot initiative support. We do not see this as likely on the short panel time-scales of the analysis here.

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