Preference Updating Under Uncertainty: Evidence from Responses to Global Warming*

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Abstract

How do individuals' policy preferences emerge and change? We synthesize political economy and behavioral approaches to produce a framework that explains how people change their policy preferences when there is uncertainty about the distributive effects of public policies. Our theory proposes a process where people learn from direct experience about how they are affected by policy issues, which leads them to update their preferred response. Climate change serves as a case to test the theory. We use an econometric model of global warming to derive what individuals' policy preferences over action to reduce climate damages might be if they were fully informed about global warming's effects and acting according to self-interest. Then, we leverage geospatial data on climate disturbances to capture experiential shocks. Separate analyses using subregional and panel survey data find that climate shocks cause individuals to become more supportive of action to address climate change in line with how they might be materially affected by global warming. Personal experience that leads to learning about who wins and loses from public policies helps to explain when changing beliefs cause shifts in policy attitudes.

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Theories of politics often begin with preferences: individuals have a ranking of choices given their available information and understanding of outcomes. Policy preferences are the heartbeat of politics because explaining who gets what, when, and how requires first understanding who *wants* what, when, and how. The standard political economy approach to derive policy preferences is to employ a model of the distributive effects of public policies on the income or assets of individuals. However, people are often uncertain about how they are affected by government policies or their absence. How do individuals form and change their policy preferences in the face of uncertainty about who wins and losses from public policies?

We focus on uncertainty about the distributive effects of public policies.[1] This type of uncertainty is ubiquitous. For example, citizens may struggle to understand how global warming will affect different parts of the world and, hence, how will they be affected by policies that seek to limit temperature change. Likewise, the public may have little insight into how global supply chains affect their pocketbooks and, hence, what policies toward them should be favored. Voters might also not understand how technological innovation shapes who wins and loses from trade and, thus, struggle to formulate informed policy preferences. Global warming, supply chains, and innovation all have distributive effects that may be difficult for individuals lacking personal experience with the issues to comprehend. Policies that affect these processes in turn appear to have uncertain consequences, so the public's preferred policies may not initially reflect their self-interest.

Uncertainty about the distributive effects of policy can be consequential (Fernandez and Rodrik 1991), but it is often not incorporated into political economy models. Instead, many political economy theories assume that individuals understand the distributive consequences of public policies for their incomes and assets (Lake 2009; Meltzer and Richard 1981). The advantage of this approach is its ability to deduce precise hypotheses, but as currently constituted, there is little theory about how individuals process uncertainty about who wins

[1]This is separate from uncertainty about the causal effects of a policy.

and loses from public policy, and how their preferred policies might change. While behavioral theories have rich models of preference formation and change (Druckman and Lupia 2000), these more psychological microfoundations, absent further specification, often do not generate clear and distinct hypotheses.

We bring together behavioral and political economy approaches to explain how individuals change their policy preferences when there is uncertainty about who gains and loses from public policies. From psychology, we apply the insight that direct experience provides a credible source of information that can change beliefs (Nisbett and Ross 1980) and help people process uncertainty (Marx et al. 2007). From political economy, we propose using models of how public policies affect material outcomes like the income, assets, and wealth that enable consumption. The economic model provides the foundation for deducing how individuals might adjust their preferred policies in response to experiential shocks. An experiential shock refers to personal experience with material losses or gains from public policy (or its absence). Building on behavioral theories, we argue that individuals learn from experiential shocks, incorporating the new information weighted according to the informativeness of a signal and the strength of their prior beliefs. These updated beliefs should affect policy attitudes if people now think different policies would better serve their self-interest.

Our aim to synthesize political economy and behavioral approaches requires us to abstract from other dynamics that will be enriching to explore in future research. For example, the media can influence when personal experience becomes politicized (Mutz 1994). Our objective is to create a tractable model of preference change, which lays the foundation for analyses of variables that could accentuate or attenuate the role of experience. The analytical choice to bracket potential moderators (e.g., media) should make it harder to find support for the hypotheses because some individuals may not be responding to the direct experiences that our empirical analysis measures.

We apply the general theory in the case of climate change for substantive and theoretical reasons. First, climate change is a pressing global challenge. Extreme weather and disasters accelerated and intensified by higher temperatures will cause substantial societal damage. Second, the time horizon and scientific nature of global warming inject uncertainty into evaluations of where and how much climate change will impact different parts of the world. As a consequence, public concern about global warming has evolved (Egan and Mullin 2017). There has been a particular focus on the role of experience (Bergquist and Warshaw 2019; Hai and Perlman 2022; Hazlett and Mildenberger 2020; Konisky, Hughes, and Kaylor 2016; Krosnick et al. 2006; Egan and Mullin 2012; Marlon et al. 2021; Weber 2006), but with mixed findings (Howe et al. 2019).

Building on these studies about experience and climate attitudes, our framework generates new hypotheses. In response to climate experiences, people's attitudes about actions to address global warming should increasingly align with how they will be materially affected by higher temperatures. This differential reaction based on a location's vulnerability departs from the vast literature on experience and climate opinions (Howe et al. 2019; Weber 2010), which often assumes a universal response to climate experiences or a conditional reaction based on factors like partisanship (Hazlett and Mildenberger 2020).

Our incorporation of political economy models into behavioral theories of experience and policy preference change allows us to derive clear predictions about how people should respond to climate experiences. In particular, we employ spatial integrated assessment models of how global warming will affect local income. Although these models are the subject of uncertainty, they provide a benchmark based on available scientific knowledge of how individuals' beliefs about their material interests should change in response to experiential climate shocks and, hence, how their preferred policy responses should change if they act according to narrow self-interest.

We conduct two empirical tests of the new hypotheses implied by our synthesis of political economy and behavioral theories. First, experience with climate change should heighten the salience of the issue. Unlike previous studies, we predict that this should be most likely to happen for individuals in places exposed to future global warming damages. To evaluate differential responses to climate experiences based on the vulnerability of a location, we deploy existing survey data collected in 123 countries in 2019 ($N = 131{,}380$). Since capturing the effect of personal experience requires high-resolution spatial data, we constructed a crosswalk that maps respondents to 2,255 administrative regions within each country at the lowest level of aggregation possible. A measurement challenge for studying climate experience across subregional boundaries is the lack of standardized data on climate disasters. While it would be ideal to study the effects of a high-impact event like wildfires, in our first analysis, we overcome this measurement challenge by using geospatial data on long-run changes in temperature variability which is more comparable across countries. This allows us to evaluate how direct experience with global warming shapes climate risk salience.

The primary challenge for causal identification is that factors like income, partisanship, or education might influence exposure to temperature variability and climate policy attitudes. To address this inferential problem, we employ covariate balancing propensity scores to weight respondents so their exposure to temperature variability and climate damages is plausibly exogenous to individual and geographic characteristics that might otherwise confound inference (Imai and Ratkovic 2014). We also conduct sensitivity analyses that indicate unobserved confounding is highly unlikely (Cinelli and Hazlett 2020), so this setting is appropriate for drawing causal inferences.

We find that people in subregions experiencing long-run increases in temperature variability are more likely to identify global warming as the most important risk in their daily life, but only if they live in a location facing potential climate damage. Placebo tests indicate that temperature variability only increases concern about climate change but not non-climate issues, which provides further evidence consistent with our proposed mechanism.

Our next analysis moves from climate risk perceptions to focus on support for government action. We employ a difference-in-differences research design with an existing three-wave panel survey in the United States to explore how policy preferences change over time. We pair survey responses with county-level measures of climate damages and benefits. Unlike the first analysis, here we are able to use comprehensive administrative data on wildfires from 2010 to 2014, which represent a high-impact climate shock.

The crucial assumption for causal inference is parallel trends: that individuals exposed to wildfires would have exhibited the same average trajectory of climate policy preferences as people in the control group. To enhance the plausibility of this assumption, we control for time-varying covariates like partisanship, ideology, and income. We also employ panel matching methods based on covariates that could predict exposure to wildfires (Imai, Kim, and Wang 2021). Equivalence tests of the parallel trends assumption indicate that it is plausible (Hartman and Hidalgo 2018), so we treat these estimates of the effects of wildfires on climate policy preferences as causal.

We find that wildfire experience causes a 2.5 percentage point increase in the belief that climate change is a serious threat warranting a government response. Consistent with our theory, this effect only appears among residents of counties facing future income losses from global warming. Notably, people with the strongest prior skepticism about climate change are the least likely to update. In contrast, those undecided about the need for climate action exhibit the most positive updating in response to wildfires. The size of this effect is notable given the polarization of climate change attitudes in the United States (Egan and Mullin 2017).

We conduct placebo tests to further evaluate the experience mechanism and rule out alternative explanations such as disproportionate media coverage of climate change in wildfire-exposed areas. These tests evaluate the effect of future wildfires on present policy preferences. The results indicate that personal experience with climate change drives the results.

Our paper makes three contributions that the conclusion elaborates upon. First, we synthesize political economy and behavioral approaches to generate new predictions about how policy attitudes change when there is uncertainty. While uncertainty has been acknowledged as a source of status quo bias (Fernandez and Rodrik 1991), we show how experience can lead to belief updating amid uncertainty, which enriches political economy approaches by opening new avenues to understand policy preference change. This synthesis is pertinent for understanding policy attitudes when politics intersects with scientific domains like the environment where uncertainty abounds.

Second, in contrast to studies that find a minimal impact of information on policy attitudes or that the public does not respond coherently to new information (Achen and Bartels 2016), we show that making such an evaluation requires careful specification of how individuals should respond to new information.[2] When using an economic model of how specific locations will be affected by global warming, we find that individuals respond in predictable ways to experiential shocks based on their narrow self-interest.

Third, we contribute to the climate politics literature by both extending studies about the role of experience to inform a general theory of how policy preferences change and by generating new predictions grounded in political economy models about the conditions when policy preferences evolve in response to climate experiences. Incorporating political economy models helps to resolve inconsistent results as to the effects of personal experience (Howe et al. 2019). By constructing a benchmark of individuals' preferences if they were fully informed and acting according to self-interest, we can advance and test more precise predictions about how the public will respond to the climate crisis.

Theoretical Silos: Political Economy and Behavior

Political economists and behavioralists take diverging approaches to studying policy preferences. Both agree that preferences are orderings of what individuals want given the available information, but political economists derive an actor's preferred policy from the material effects of policy, whereas behavioralists start with identity, experience, and values. Both approaches have produced important insights. Yet, this theoretical and methodological siloing, where insights do not cross-pollinate, has analytical costs. Theories of policy preference

[2] Also see Ashworth, Bueno de Mesquita, and Friedenberg (2018).

change in political economy lack strong behavioral microfoundations, whereas behavioral theories lack the determinant predictions from political economy. Our goal is to synthesize these approaches.

Certain Policy Preferences in an Uncertain World

Political economy theories often assume that individuals understand with certainty the effects of public policies regarding trade, air pollution, or innovation on their income, assets, and wealth. Theorizing in this tradition begins by taking a model of the economic effects of a policy and based on who wins or losses, the analyst deduces hypotheses regarding the behavior of firms, politicians, and voters. These political actors, by assumption, know with certainty whether they gain or lose.

The Open Economy Politics (OEP) paradigm, as characterized by Lake (2009), is a prominent example of this mode of analysis. The OEP approach has served as a fruitful foundation for theorizing the politics of trade (Milner 1988; Rogowski 1987; Scheve and Slaughter 2001b), immigration (Scheve and Slaughter 2001a; Peters 2015), foreign direct investment (Scheve and Slaughter 2004), climate (Kennard 2020), and finance (Broz 1999; Frieden 1991).

Yet, in some cases, it is unrealistic to assume that individuals know with certainty the effects of government policies on their incomes and assets. Besides disparities in information (Rho and Tomz 2017), some issues by their very nature involve uncertainty that makes it challenging to understand how one is affected. Climate change, for example, will have differential impacts across space and time, which makes it difficult to know precisely how and when one might be affected. In turn, uncertainty makes it harder for individuals to determine their best policy option.

Political economy theories allow for policy preferences to change when the objective situation evolves. However, preferred policies evolve as the level of uncertainty changes, it is not the objective situation that transforms but one's beliefs about the world. For example, there is a true mapping between climate change and its material impacts, but this relationship is not known with absolute certainty even to scientists. If one learns more about how she is impacted by global warming, her preferences over policy responses to global warming may change, but it is not because of the objective situation (e.g., the true mapping from climate change to material impacts) which has remained invariant, but because her understanding of the situation (e.g., distribution of uncertainty) has evolved. Allowing for uncertainty can make sense of why people's preferences may not reflect their objective material interests because they cannot determine their best response while also explaining preference change despite the objective situation remaining the same.

Political economy is not ignorant of uncertainty. Previous work has explored how uncertainty affects the strategies of candidates for public office (Shepsle 1972), the design of international agreements (Koremenos 2005; Rosendorff and Milner 2001), the extent of status quo bias (Fernandez and Rodrik 1991), the behavior of interest groups (Stokes 2020), and monetary policy decision-making (Nelson and Katzenstein 2014). Yet beyond the consequences of uncertainty, this work does not explain policy preference change.

Changing Policy Preferences Without Determinate Predictions

Models of learning provide some traction to begin to understand changes in preferred policy responses. In international relations, countries may learn about the distribution of power through fighting (Powell 2004), and leaders can take lessons from history (Jervis 1976). Learning in this context is about the consequences of policies or preferences of others but less so about how individuals discover the distributive effects of public policies. Ideas provide another mechanism of preference change (Goldstein and Keohane 1993). However, these theories typically explain the effects of ideas rather than their causes, which would be needed to understand preference change over time.

Behavioral research has rich theories to explain how policy attitudes change. Druckman and Lupia's (2000) review highlights how interaction with one's environment can lead to policy preference change by acquiring new information. As Zaller (1992, 6) puts it, "[e]very opinion is a marriage of information and predisposition." Zaller's (1992) foundational "Receive-Accept-Sample" model of public opinion centers on political communication, where new information leads to opinion change by altering top-of-mind considerations. However, this model implies that people do not have true policy attitudes outside of predispositions that influence resistance to new messages, such as partisanship. Instead, the public reacts to what is salient. This would imply that one could not use economic models to make determinate predictions about how policy preferences change, which is necessary for political economy theorizing.[3]

However, there is evidence that people have meaningful policy attitudes. For example, analysis of decades of surveys suggests that public opinion may be collectively rational and adapts to new information and changing circumstances (Page and Shapiro (1992); but see Achen and Bartels (2016)). Ansolabehere, Rodden, and Snyder (2008) show that individuals hold stable preferences when measured carefully.[4] Yet what this stability implies for policy preference change is unclear absent a model of how individuals ought to adjust their preferred policy responses to new information.

Theory of Preference Updating Under Uncertainty

Our theory synthesizes political economy and behavioral theories. The political economy approach allows us to generate determinate predictions by using concrete models, while behavioral theories more faithfully explain how individuals process uncertainty by drawing on psychological microfoundations. Our theory provides a framework for political economy

[3]There is a related literature on framing (Chong and Druckman 2007) and persuasion (Druckman 2022).

[4]This by no means summarizes the large literature on attitude constraint going back to Converse (1964).

and behavioral politics can complement each other to produce novel insights about preference change.

Personal experience is a key variable influencing beliefs that shape preferences and their evolution. Building on behavioral studies (Weber 2006), we argue that the direct experience of being harmed by or benefiting from a public policy (or its absence) alters beliefs about its distributive effects. By consequence, people's preferred policies should update to more closely reflect what the best available economic model indicates is their best response if they were fully informed and made decisions based on what the economic model quantifies such as income, wealth, or assets.[5] We assume that belief updating will resemble a Bayesian model of learning, where the effect of an experience on one's new beliefs will depend on prior beliefs weighted by the probability of experiencing an event given one's expectations.[6]

While previous studies have reached mixed conclusions about the effects of information on beliefs and behavior (Dunning et al. 2019; Gilens 2001; Taber and Lodge 2006), there are two reasons why direct experience should be a more impactful source of information.

First, direct experience increases the probability that an individual learns about an issue. Consider a heat wave. One cannot ignore a heatwave, whereas it would be possible to engage in selective viewership of the media. In this situation, disconfirmation bias, where one avoids contradictory evidence (Taber and Lodge 2006), may be less applicable to direct experience. People cannot engage in selective exposure to natural disasters. To the extent that an individual changes her behavior to avoid climate damage, that would reveal a belief in the seriousness of global warming. This suggests that experiential shocks may be more likely to affect beliefs because it is harder for individuals to opt out of receiving the signal and they

[5]Economic models are themselves subject to uncertainty. Economic models should be thought of as providing the best benchmark of what preferences should be given present knowledge and defined in terms of what the model quantifies.

[6]This claim is not that people are updating probabilities explicitly in their heads but that their behavior should be consistent as if they were doing so on average.

are less likely to doubt the bias of the messenger.

Second, direct experience helps people process information about risks when there is uncertainty (Weber 2006).[7] This is because people use different systems of thinking when evaluating information from direct experience. A large body of social psychology research documents how humans use two systems to process information: experiential and analytic (Chaiken and Trope 1999; Epstein 1994; Evans 2008). The experiential process is fast, automatic, and unconscious, whereas the analytic process is slow, deliberative, and conscious. When people receive information analytically, which is often the case in the media, it requires more cognitive steps to re-contextualize the information to what one should believe or do. In contrast, information in the form of personal experience evokes strong feelings that make it more memorable, accessible, and dominant in one's decision-making (Slovic et al. 2002; Marx et al. 2007; Weber 2006). The lack of direct experience with the costs and benefits of public policy seeks to address could lead people to not recognize a problem that harms them and, hence, their policy preferences would not reflect self-interest.

Even with direct experience, people may interpret the same event using different lenses (Druckman and McGrath 2019). For instance, Democrats and Republicans exhibited different responses to wildfires in California (Hazlett and Mildenberger 2020).[8] Although interpretative differences could impede efficient belief updating in the short run, this may be less likely in the long run. One reason is that when the cost of holding incorrect beliefs is high, self-interest could lead individuals to question their interpretation of events. While behavioral theories are skeptical of self-interest, material motivations can influence preferences when there are unambiguous costs (Citrin and Green 1990). There is also experimental

[7]While there are sources of bias that may cause inefficient belief updating (Tversky and Kahneman 1974), these biases pertain most directly to contemporaneous decisions and speak less to learning over time.

[8]Partisanship may influence incentives to attribute disasters to climate change (Hai and Perlman 2022).

evidence that the public can perform a limited form of Bayesian updating when there are monetary incentives for accuracy (Hill 2017), and are more likely to answer survey questions about the economy correctly (Prior and Lupia 2008; Prior, Sood, and Khanna 2015). For global warming, the distributive effects are profound. These costs should create long-run pressures for accuracy. It may be easier to be a motivated reasoner when it comes to other political beliefs where there are not as large of consequences for being wrong.

Based on the theory, the first hypothesis is that experiential shocks should lead individuals to update their beliefs in line with how they will be materially affected by a public policy (or its absence). The initial change in beliefs should be the recognition that there is a problem. With recognition comes greater salience. Salience means an issue rises in relative importance with respect to other political matters. In other words, exposure to experiential shocks should increase the likelihood that those adversely affected by public policy prioritize the problem relative to other political issues.

The second hypothesis is that as the recognition and salience of an issue grow, individuals who are negatively affected should become more willing to support government actions to address the problem. This is the step where changes in beliefs and issue salience can lead to policy preference change.

How individuals revise their beliefs in response to new information will depend on the strength and content of their prior convictions. As a conceptual device, consider three types of people: *skeptics*, *undecideds*, and *believers*. Skeptics have strong prior beliefs that there is no problem that needs to be addressed. Undecideds do not have sufficient knowledge of the issue, either out of ignorance or uncertainty. While believers understand the problem, how this knowledge translates into preferences will depend on whether the believers gain or lose from a policy. Overall, the strength of one's prior beliefs should influence the extent to which she updates in response to new information. Specifically, our third hypothesis is that individuals with stronger prior beliefs should be less likely to update their preferences following experiential shocks than those with weaker prior beliefs.

Climate Politics Application

We apply our general theory in the context of climate change. We focus on uncertainty regarding how climate change will affect future income for the average individual in a location. This type of uncertainty about global warming's material effects implies uncertainty about the distributive consequences of policies to address climate change – a policy to lower global temperatures will provide more benefits to the places most impacted by climate change.[9] Governments can take distinct approaches to combat climate change, so we examine the broader policy preference for action to address global warming.

There is a large literature about climate policy attitudes (Bernauer and Gampfer 2015; Bush and Clayton 2023; Mildenberger and Tingley 2019; Egan and Mullin 2017). One meta-analysis found that variables such as values, ideologies, worldviews, and political orientations had the strongest correlation with belief in climate change (Hornsey et al. 2016). However, these correlates shed little light on how preferences might change. Another set of studies focuses on the effects of framing or information on climate policy support (Bergquist, Mildenberger, and Stokes 2020; Gazmararian and Tingley 2023; Dechezleprêtre et al. 2022). While these studies demonstrate plausible mechanisms to enhance policy support, they do not explain the observed patterns of climate policy preferences and their evolution.

Building on previous studies about experience and climate attitudes (Bergquist and Warshaw 2019; Hai and Perlman 2022; Hazlett and Mildenberger 2020; Weber 2010; Konisky, Hughes, and Kaylor 2016; Krosnick et al. 2006; Egan and Mullin 2012; Marlon et al. 2021; Weber 2006; Weber 2010), we hypothesize that in response to direct experience with climate hazards — experiential shocks — people will change their beliefs about how they are materially affected by global warming and, thus, their preferred policy responses.

However, departing from this literature, our incorporation of political economy models

[9]There could be other distributive consequences due to the design of a policy (e.g., who is taxed), which future research should examine.

suggests that responses to climate experiences will depend on the extent of an individual's location to future climate damages. Specifically, we expect that people in locations facing potential climate damage should become more concerned and supportive of policies to address climate change than individuals in less vulnerable locations.

> **Hypothesis 1**: Personal experience with the effects of climate change should lead to greater recognition and salience of global warming if one is located in an area facing potential damages.

> **Hypothesis 2**: Personal experience with the effects of climate change should lead to more desire for government action to address global warming if one is located in an area facing potential damages.

> **Hypothesis 3**: People with weaker prior beliefs about climate change should exhibit larger increases in recognition, salience, and desire for action after personal experience with climate change compared to people with stronger prior beliefs.

These hypotheses contrast with previous studies that operate from the implicit assumption that individuals should exhibit a similar response to climate change experience (Bergquist and Warshaw 2019; Egan and Mullin 2012; Hoffmann et al. 2022; Konisky, Hughes, and Kaylor 2016) or a conditional response based on factors like partisanship (Hazlett and Mildenberger 2020). However, absent a model of how people in different locations will be affected by future global warming, one cannot automatically predict how policy preferences should change, if at all. Our incorporation of political economy models generates new predictions that may help to explain the mixed results found in the literature about the effects of climate change experience (Howe et al. 2019).

We focus on the impacts of climate change that can be captured in an economic model. Of course, there are non-economic damages from climate change, like cultural losses, that do not lend themselves to such straightforward quantification. We bracket these for parsimony and anticipate that they would create bias against our hypotheses because there are people who would be responding to experiential shocks that are not accounted for in the measures of damages described below.

Using our theoretical approach that synthesizes political economy models with behavioral insights, we can evaluate how individuals update their preferences in response to climate experience. By starting with an economic model of how climate change will affect income in different parts of the world, we can deduce precise hypotheses about how individuals should respond based on how their location will be affected economically by higher temperatures.

Research Design: Climate Belief and Risk Salience

Our first empirical analysis explores cross-national differences in climate change beliefs. Analyzing variation across space is crucial since the distributive effects of climate change as a physical phenomenon are geographic. Most notably, the Global South will suffer more than the North (Cruz and Rossi-Hansberg 2023). Since some countries are vast and contain areas that may lose or gain from higher temperatures, we conduct our analysis at the subregional level to capture this heterogeneity.

The analysis focuses on how long-term structural changes to climatic variability influence preferences. The theory implies that preferences in a given moment represent a history of experience shocks, which we capture using long-run trends in climate data. Focusing on long-term changes also mitigates the possibility that reported preferences reflect temporary concerns made top-of-mind by a recent event. Although the cross-sectional data limits us from making strong claims about belief change, we use panel data in subsequent empirical tests to capture this related dynamic. Here, the focus is on macro-level pressures that shape beliefs in ways that should manifest differentially across regions.

This analysis focuses on the first hypothesis: recognition and salience of climate change risk. We expect that climate risk salience will be greater for individuals in regions that face potential damages from global warming and have experienced long-term changes in temperature variability. This sustained change in weather patterns should clarify the distribution of climate damages and heighten the issue's salience. It is the interaction of damages and shocks that matters.

Data and Measurement

Subregional Survey Data

Unlike most cross-sectional work that focuses on countries, we leverage survey data at the subregional level. Our data on climate risk perceptions come from the World Risk Poll conducted in 2019 by Gallup and Loyd's Register Foundation. The survey was conducted using probability-based, nationally representative samples covering around 150,000 people in 142 countries and territories.[10] In most countries, the approximate sample size is 1,000.[11] The questions underwent intense piloting and multiple rounds of review. In addition, the survey instrument was translated into the major conversational languages of each country, and teams of trained enumerators administered the survey using face-to-face interviews and telephone calls.

We build on these data by constructing a new crosswalk that maps respondents to 2,043 administrative regions within each country at the lowest level of aggregation possible. We invested considerable resources in building our crosswalk because while the survey identifies a respondent's subregion, this label is not readily connected to any geospatial shapefiles of administrative borders. Connecting respondents to these geospatial areas is necessary to pair respondents with our climate damage and temperature data described below, which forms the crux of our analysis. Appendix A.2 describes the construction of our crosswalk.

[10] We only use the surveys from the countries for which there are climate damage data.

[11] The sample size is higher for China, India, and Russia, and lower for Jamaica.

Climate Risk Perceptions: Outcome Measure

The main outcome evaluated for hypothesis 1 is the perception that climate change is a salient risk. Salience refers to whether a person places greater weight on the importance of a topic. The weight one associates with a risk is an indication of both belief and importance. To state that climate change is a top risk reflects both one's belief that global warming presents a threat and the urgency with which the issue must be addressed relative to other societal matters. We measure climate risk salience by using responses to two open-ended questions:

> In your own words, what is the greatest source of risk to your safety in your daily life?
>
> Other than what you just mentioned, in your own words, what is another major source of risk to your safety in your daily life?

Answers to these questions are mapped to categories including "Climate change, natural disasters or weather-related events (such as floods, drought, wildfires, etc.)."[12] We construct an indicator that takes the value 1 if a respondent identifies climate change as a top or major risk and 0 if not. A separate indicator records if climate change is only the top risk named (more restrictive than including major risks).

We focus on top-of-mind salience rather than stated concern about climate change (e.g., how important is climate change) since stated concern measures suffer from social desirability and hypothetical bias. While many say global warming is important, public support is sensitive to the costs of climate policy (Bechtel and Scheve 2013), which suggests that standard measures of climate risk perceptions overstate concern. Our measure avoids this source of bias by using open-ended questions. This open-ended question is not susceptible to priming and allows for an assessment of whether climate change is indeed a top concern.

[12] Common answers include "Crime/violence/terrorism," "Road-related accidents/injuries," and "Health."

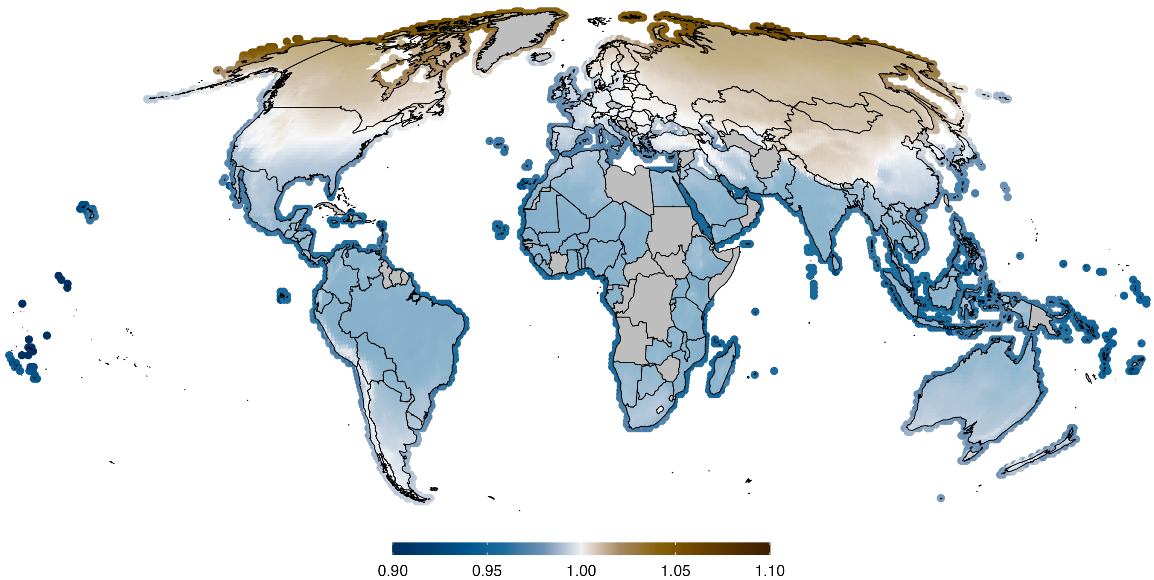
Potential Climate Damages: Moderator

To measure how future income will change in a subregion because of global warming, we leverage climate damage estimates from a spatial integrated economic assessment (SIEA) model (Cruz and Rossi-Hansberg 2023). This model captures how the world economy evolves and incorporates damage functions that account for how local temperature changes impact fundamental productivities and amenities through trade, migration, and innovation. The SIEA builds upon the established spatial growth framework in Desmet, Nagy, and Rossi-Hansberg (2018), which has been validated with backcasting exercises and successfully applied to assess sectoral responses to global warming (Conte et al. 2021) and the effects of sea level rise (Desmet et al. 2021). In particular, the model accounts for damage from long-run temperature changes, which is a substantial means by which global warming will impact economic growth via heat's effects on mortality, human physiology, violence, productivity, crop yields, energy demand, and population movements (Carleton and Hsiang 2016). The damage estimates are recorded at the $1^\circ \times 1^\circ$ longitude-latitude resolution, which we aggregate to the subregion level by taking the average level of damage across the grids.

We construct an indicator for if a subregion faces potential damages from global warming in the year 2050. The moderator is dichotomous since there is considerable uncertainty about global warming's effects, which erodes the substantive meaning of point estimates. Rather than create a false sense of certainty, an indicator is realistic by recording whether a subregion is on the damages or benefits curve of the distribution. The results also obtain when using a continuous moderator (Appendix A.8.7). Figure 1 shows the global distribution of damages and losses from higher temperatures. These estimates are overlaid on a map that indicates if countries are in the survey sample (no shading) or not (grey shading).

The climate damage estimates can be thought of as what individuals' preferences over the future temperature level would be if they cared foremost about income in the place where they presently live. Of course, as we discuss in the conclusion, additional non-material factors may serve as inputs to preferences. Further, individuals differ in their mobility to escape

Figure 1: Effect of Global Warming on GDP in Surveyed Countries



Notes: Map depicts the ratio of 2050 GDP in a world of global warming damages to a counterfactual in which temperature has no effect. Values less than 1 denote losses. Our study includes survey data on all countries except those shaded grey. While the analysis uses subregional data, the plot depicts national borders for exposition.

the damage from climate change. To the extent that the effect of global warming on income is an insufficient first cut at explaining preferences, that should introduce bias against our hypotheses since we would be using a moderator with little predictive power. Nonetheless, we expect income to be an adequate and parsimonious foundation to theorize preferences.

Temperature Variability: Experiential Shock

While it would be ideal to test our hypothesis with data on disasters like wildfires, unfortunately, such data do not exist globally at the subnational level. To overcome this limitation, we employ temperature data that can be standardized across subregions. However, in the subsequent analysis, we are able to employ quality data on wildfires that match the unit of analysis for the public opinion data. The measure examines changes in temperature variability because these fluctuations should be more visible signals over time, as opposed to changes in the absolute temperature level. Climate change has been increasing temperature extremes, resulting in greater variability (Bathiany et al. 2018). Temperature variability is also an important channel through which global warming inflicts economic damage (Kotz et al. 2021).

We construct the measure by taking the difference between temperature variability across months in 2018 (the year before the survey was fielded) and the average long-run monthly temperature variability.[13] The data come from the Global Historical Climatology Network's Climate Anomaly Monitoring System, which measures monthly global land surface temperature averages at the $0.5° \times 0.5°$ resolution (Fan and van den Dool 2008).[14] The dataset combines two individual collections of weather station observations interpolated across space using validated methods. We acquired this data from the National Oceanic and Atmospheric Administration.

Figure 2 plots the spatial distribution of the long-run change in temperature variability. Temperature variability has increased in much of the world, although there are areas where variation has fallen. Since these positive and negative changes fall across regions experiencing both potential damages and benefits from global warming, this provides a more challenging test of the hypothesis. One might expect that increased variability could lead to mistaken inferences in places facing potential net benefits. If this is not the case, the result would be a strong indication that the individuals are updating their preferences in a direction consistent with their narrow self-interest.

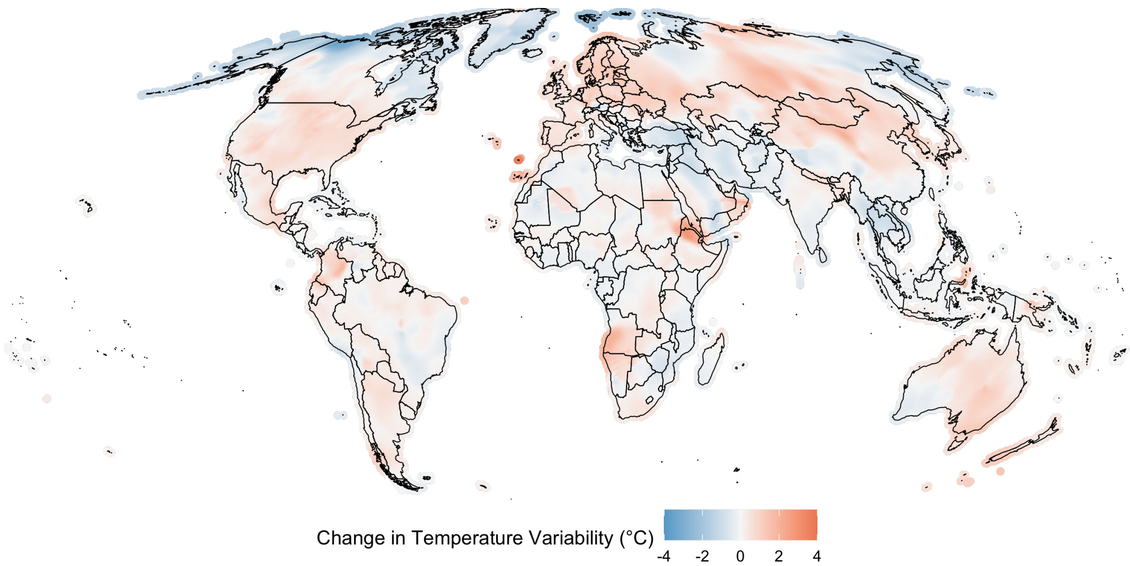
Causal Identification Strategy

The aim is to estimate the effect of climate experience shocks on global warming risk perceptions, conditional on whether a subregion faces potential damages. The primary causal

[13] The benchmark period is from 1980-2000 since the median respondent would have been born around 1980. Results are robust to using an alternative benchmark period (Table A10).

[14] We aggregate these data to the climate damage level since that is the highest level of the gridded data, which we then aggregate to the subregion level.

Figure 2: Long-Run Changes in Temperature Variability



Notes: Data at the $0.5° \times 0.5°$ resolution from Fan and van den Dool (2008).

identification assumption is that exposure to long-run temperature change in subregions facing potential damages is (conditionally) exogenous to factors that would otherwise explain climate change risk perceptions.[15] For example, if income levels explained the decision to live in a location and climate beliefs, that could confound inference.

To rigorously enhance the plausibility of the conditional ignorability assumption, we balance observations according to their exposure to long-run temperature variability changes (the treatment) interacted with the potential damages indicator (moderator). We estimate weights using the covariate balancing propensity scores (CBPS) proposed by Imai and Ratkovic (2014). This approach is advantageous since it can be applied to continuous treatments like ours and has demonstrably better empirical performance over other weighting methods.

We estimate the weights to maximize balance on subregional, individual, and national covariates theoretically predictive of treatment assignment and the outcome. At the sub-

[15]Reverse causality is unlikely given that present-day individual and subregional characteristics do not immediately cause long-run changes in temperature variability.

regional level, we include covariates for population, gross domestic product (GDP), carbon dioxide emissions, and the development potential for fossil fuels. Studies find the level of economic development influences the ability to adapt to climate change and correlates with risk perceptions (Bush and Clayton 2023; Kim and Wolinsky-Nahmias 2014). At the individual level, we balance age, gender, education, household income, and risk interpretation, which the climate politics literature identifies as relevant determinants of attitudes (Bush and Clayton 2023; Dechezleprêtre et al. 2022; Lee et al. 2015).[16] Lastly, at the country level, we balance using regime type because democracy could influence information availability or the free press. Appendix A.1 details the construction of these covariates.

Appendix A.5 reports the results of equivalence tests, which indicate that the covariate balancing weights improved sample balance such that the treatment is plausibly as if random. Unlike a conventional null hypothesis that there is no difference, the equivalence approach begins with the initial hypothesis that the data are unbalanced (Hartman and Hidalgo 2018). Thus, this test is more conservative since the threshold for evidence to show no imbalance is higher.

Estimating Equation

We estimate the following linear regression model with covariate balancing weights:

$$y_i = Temp_{r(i)} + Damage_{r(i)} + \delta(Temp_{r(i)} \times Damage_{r(i)}) + \eta_{r(i)} + \epsilon_{r(i)}. \tag{1}$$

The subscript i denotes individuals. $r(i)$ and $c(i)$ are functions that map individuals to subregions and countries, respectively. y_i is the outcome measure that denotes whether a

[16] Unfortunately, this survey did not ask questions about ideology or partisanship, which are relevant predictors of climate attitudes (Hornsey et al. 2016). We use a sensitivity analysis to evaluate how large an omitted variable would have to be to change the results and find that confounding is unlikely to alter our conclusions. Our subsequent analysis with panel data pays careful attention to these political factors.

respondent identifies climate change as the greatest or a major risk. τ is the long-run change in temperature variability for a subregion. $Damage_{r(i)}$ indicates whether a subregion faces potential damages or benefits from global warming. δ is the coefficient from our interaction term of interest. η is a global region fixed effect accounting for time-invariant regional factors that could confound inference. We employ the HC1 heteroskedasticity-robust covariance estimator with standard errors clustered by subregion. Controls are included in the data pre-processing step when we employ the CBPS methodology to reduce the dimensionality of the covariates into weights for each observation such that the probability that an individual is exposed to temperature variability is statistically independent from the observable covariates.[17]

The empirical approach assumes that for the subregion-level measures, all individuals within the administrative borders are equally exposed to climate damages or changes in temperature variability. This claim is plausible for temperature variability, which exerts a common effect across a subregion. In terms of climate damages, some individuals may have more resources to adapt to climate change than others, which could attenuate damages. For this reason, we include a control for household income when constructing balancing weight. Even if present, ecological measurement issues would introduce bias against our hypotheses because there would be units presumed to be treated but are actually not.

Results: Effect of Long-Run Temperature Variability Change on Climate Risk Salience

Figure 3 plots the conditional average treatment effect of long-run temperature variability on climate risk salience and the placebo tests for respondents in subregions facing potential damages and benefits. The moderator is dichotomous, which ensures that there is common

[17] The results also obtain when using covariates instead of weights to satisfy the conditional exogeneity assumption (Appendix A.8.1).

support at all levels of the moderator (Hainmueller, Mummolo, and Xu 2019). A standard deviation increase in long-run temperature variability causes a 1 percentage point increase in identifying climate change as a top or major concern for people in places facing potential damages. As hypothesized, there is no effect on recognition or salience of climate change for individuals in places facing potential benefits. A similar result appears with the more restrictive climate risk salience measure that codes only those identifying global warming as the top risk.

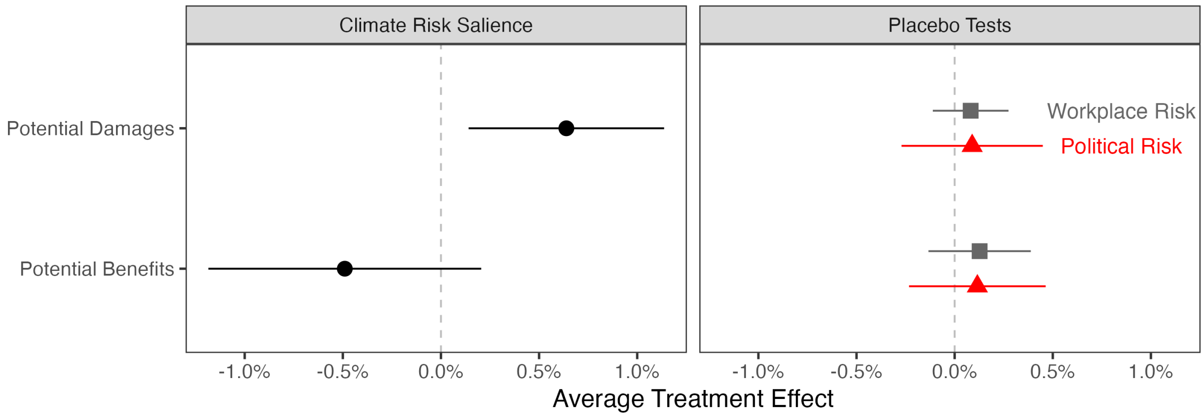
How meaningful is this effect? Given the uniqueness of the open-ended question format for measuring risk salience, we lack a comparable benchmark to assess treatment effect sizes. Other studies of the effect of temperature extremes on climate beliefs have found short-term effects around a 5% (Egan and Mullin 2012). However, there is considerable heterogeneity (Howe et al. 2019). One appropriate referent would be to examine the baseline level of climate support in the sample.[18] Given that only 3 to 5 percent of the sample identify global warming as a top or major risk to their daily life, the treatment effect is 20 to 30 percent of the size of the outcome mean, which is appreciable.

The right plot in Figure 3 presents the results from placebo tests that estimate the effect of temperature variability on the perception that workplace accidents or political strife are the most important risks in one's daily life. These outcomes are theoretically unrelated to temperature variability and should not be affected by climate experience shocks.[19] The coefficient on the interaction term in both cases is almost zero. These placebo tests suggest that the effect of long-run temperature variability is due to updated beliefs about global warming.

[18] It would be inappropriate to benchmark against other covariates in the model because they are not causally identified, what is sometimes called the "Table 2 fallacy."

[19] The first placebo, *work*, is whether a respondent says that "work-related accidents; physical injuries" is the greatest risk in her daily life. The second placebo, *politics*, is whether a survey-taker says that "politics/political situation/corruption" is the greatest risk in her daily life.

Figure 3: Effect of Long-Run Temperature Variability Change on Climate Risk Salience and Non-Climate Placebos



Notes: Conditional average treatment effect of long-run temperature variability change on climate risk salience (left panel) and placebo tests with non-climate risks (right panel) for respondents in subregions facing potential damages or benefits from future climate change. Estimates from a linear regression with covariate balancing weights and HC1 standard errors clustered by subregion (N = 130,377). Bars denote 95% confidence intervals. Table A4 contains the results.

Potential alternative explanations for these patterns would be that risk perceptions are instead explained by unobserved confounders like values or worldviews. However, it is unclear why these factors would correlate with temperature variability. Nonetheless, we conduct a sensitivity analysis to see how large an unmeasured confounder would have to be to change our conclusions (Appendix A.7). Applying the methodology proposed by Cinelli and Hazlett (2020), we find that a confounder that is orthogonal to the covariates in the model would have to have more than 15× the correlation of age — one of the strongest predictors of climate risk salience — with both the probability of treatment assignment and the outcome to bring the lower bound of the 95% confidence interval to touch 0.[20] To completely erase the point estimate of the interaction term, the confounder would have to have more than 70× the effect of age. Given the literature on the predictors of climate opinion, such an extreme confounder is unlikely.

These results, which provide support for hypotheses 1 and 2, are robust when employing

[20]We estimate a model with covariates to permit benchmarking.

a multi-level model with random intercepts for subregion (Table A6); using a different benchmark period for long-run changes in temperature variability (Table A10); controlling for the national level of climate policy (Table A9); employing an alternative measure of regime type (Table A7); and controlling for country-level measures of fossil fuel rents as a share of GDP (Table A8).

Research Design: Preference Change

The first empirical analysis demonstrated how long-run changes in temperature variability shape the distribution of climate change risk salience across space. Our next analysis turns to preference change. Since our goal is to understand change, we employ panel data — repeated interviews with the same respondent. To reiterate our expectations, in response to experiencing climate hazards, individuals in places facing damages should become more concerned about global warming and supportive of actions to mitigate the harm from higher temperatures (hypotheses 1 and 2). We also explore how the strength of prior beliefs influences preference change, with the expectation that people with stronger convictions should be less responsive to climate change experiences (hypothesis 3).

Data and Measurement

The panel survey data come from the Cooperative Congressional Election Study's 2010-2014 Panel Study (Ansolabehere and Schaffner 2015). The panel study was conducted over the Internet by YouGov. The sampling method used the firm's matched random sampling methodology, a validated and widely used technique to select a representative sample. After accounting for attrition, 9,500 respondents were interviewed in 2010, 2012, and 2014 using a common set of questions across the waves.

Climate Beliefs and Preferences

The outcome measure to capture climate beliefs and preferences comes from this item:

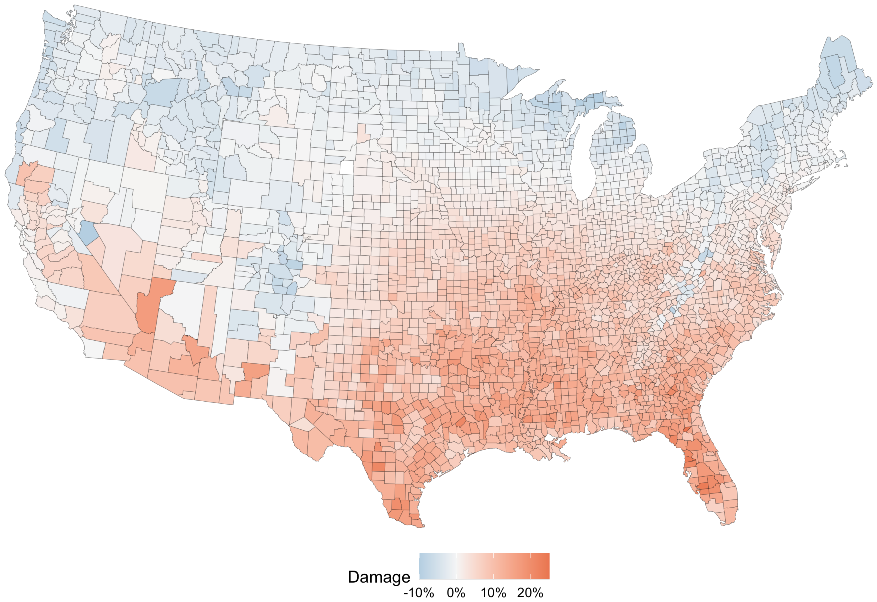
> From what you know about global climate change or global warming, which one of the following statements comes closest to your opinion?
>
> *Global climate change has been established as a serious problem, and immediate action is necessary; There is enough evidence that climate change is taking place and some action should be taken; We don't know enough about global climate change, and more research is necessary before we take any actions; Concern about global climate change is exaggerated. No action is necessary; Global climate change is not occurring, this is not a real issue.*

This question simultaneously captures two related constructs. The first construct is the extent to which one believes that global warming is happening and is a serious problem. The second construct is the degree to which one thinks action is needed to address climate change. Together, the item encompasses a belief in the seriousness of the climate problem and the urgency to act, which corresponds with hypotheses 1 and 2. The question is well-suited for the objective of assessing the level of climate action desired. Since most respondents exhibit high levels of climate concern, we dichotomize the measure so 1 represents the preference that action should be taken and 0 if not.

Potential Climate Damages as Moderator

An advantage of studying the American context is that there are high-resolution economic models of climate change that permit even more fine-grained analysis than the Cruz and Rossi-Hansberg (2023) model allows. Using additional models also reduces the potential dependence that might result from relying on one approach. We use the county-level estimates of climate change damages from Hsiang et al. (2017). Their model estimates the value of market and non-market damages from higher temperatures in agriculture, crime, coastal storms, energy, human mortality, and labor. Like Cruz and Rossi-Hansberg (2023), they find substantial spatial heterogeneity in the economic effects of higher temperatures. As

Figure 4: Climate Damage to GDP, County-Level



Notes: Estimates from Hsiang et al. (2017). Red denotes potential climate damages, while blue denotes potential climate benefits.

visualized in Figure 4, parts of the North and West of the United States experience potential gains in terms of GDP, while the South incurs large losses.

We focus on the total damage to GDP to maximize comparability across the models. As before, we construct an indicator for if a county faces potential damages defined as experiencing greater than 0 percent GDP loss from climate change by the late 21st century. In Appendix B.5, we employ a continuous moderator and the binning estimator proposed by Hainmueller, Mummolo, and Xu (2019) and find consistent results for where there is the most common support of the moderator, while for higher values of the moderator with sparser support, the estimates become less precise.

Wildfires as Experiential Shocks

Wildfires serve as the experiential shock in this analysis for substantive and empirical reasons. First, fires are becoming more frequent and longer lasting due to climate change (Westerling et al. 2006). Wildfires can be exceptionally destructive and impressionable, making them a powerful experience that could alter one's beliefs and preferences. Secondly, the data on fires match our county-level damage estimates, facilitating the empirical analysis. While wildfires are a salient climate hazard in the United States, they are not as applicable to all countries, which is why the previous cross-national analysis focused on temperature variability.

Though there is work exploring disasters more broadly, and fires in particular, these findings have been mixed. Hui et al. (2022) find that proximity to wildfires increases Republican support for adaptation policy, while Hazlett and Mildenberger (2020) find a conditional relationship between fire experience and voting in climate-related referenda. Most relevant for our purposes, previous studies have not analyzed wildfires alongside a model of political actors' preferences if they were acting according to self-interest.[21]

We use data on the annual count of wildfires declared disasters in a county. Data come from the FEMA Disaster Declaration's Summaries. The reports are generated when a county (or other level of government) declares an emergency, which the federal government certifies. Although there might be wildfires where emergencies were not declared, these are likely of lesser damage. Plus, local governments have an economic incentive to make a disaster declaration because it unlocks federal funding to assist with the recovery. Only around 4 percent of the respondents reside in a county with a wildfire during a panel wave. The reduced statistical power should introduce bias against detecting an effect.

Methods

We estimate the effect of a within-unit change in exposure to wildfires by using the following empirical specification,

$$y_{it} = \alpha + Fire_{c(i)t} + Damage_{c(i)} + \delta(Fire_{c(i)t} \times Damage_{c(i)}) + X_{it}^{\intercal}\beta + \lambda_t + \eta_i + \epsilon_{it}, \quad (2)$$

[21]Future studies could also use different types of disasters to test our hypotheses.

with standard errors clustered by county to tend to potential auto-correlation. y_{it} is the climate belief and preference outcome measure. $Fire_{c(i)t}$ is a count of the fires in a county for a panel wave, with $c(i)$ representing a function that maps respondents, i, to counties. $Damage_{c(i)}$ indicates if a county faces potential damages from higher temperatures. δ is the coefficient of interest, representing the differential effect of wildfire exposure in counties facing damages versus those with potential net benefits. The λ_t term is a panel-wave fixed effect, while η_i is a vector of individual fixed effects, which remove all time-invariant omitted variable bias.

The matrix $X_{it}^{\intercal}$ contains time-varying covariates. Since our empirical strategy estimates the effect of *within*-unit changes in covariates, we include only covariates that exhibit within-unit variation and might confound our results.[22] We control for individual-level employment, education, partisan identification, ideology, household income, and religious importance.[23] Ideology and partisan identification, in particular, help capture the values and political orientations that previous studies find predict climate beliefs (Hornsey et al. 2016) and responsiveness to climate experiences (Hazlett and Mildenberger 2020). Even if there were no covariates, to bias our results, there would have to be systematic time-varying change in one of these measures that also corresponds with personal experience with climate change, which is unlikely.

Assumptions Required for Causality

There are two strategies for drawing causal inferences given this setup. First, we adopt a difference-in-differences approach, which has the advantage of not requiring exogenous

[22]Age increases by the same rate, so it is not a necessary control. Respondents report having the same gender and racial identity through all panel waves, so we do not include these measures.

[23]We impute values for the approximately 10 percent of respondents who do not answer the income question or indicate that they prefer not to answer. The imputation procedure replaces the missing values using the mean household income level.

treatment assignment, only parallel trends. The parallel trends assumption is challenging to assess in this context because the treatment is staggered and repeated. Despite these challenges, Appendix B.2 provides some evidence of comparable pre-trends that suggests this assumption is plausible from both a visual assessment of pretrends and formal placebo tests (Hartman and Hidalgo 2018).

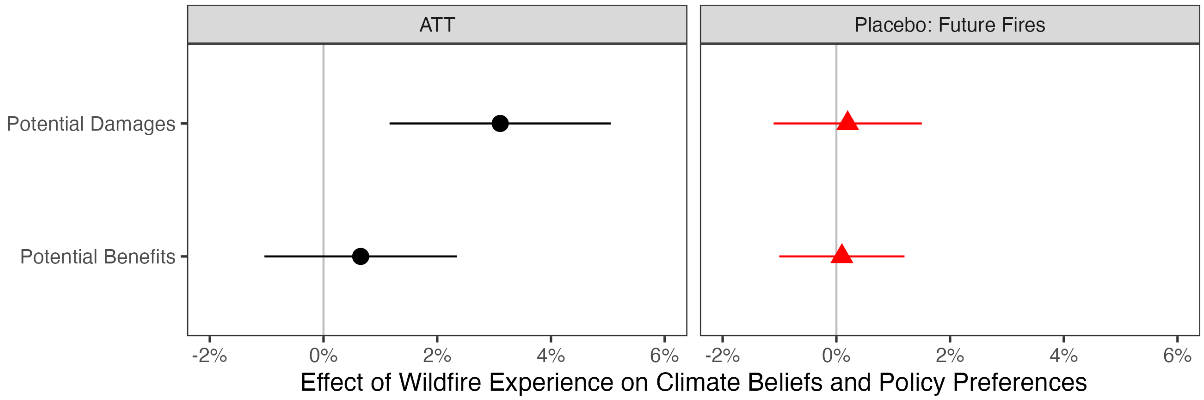
The second approach is to satisfy the strong ignorability assumption — treatment assignment is independent of potential outcomes — by conditioning on a relevant set of covariates. To this end, our individual-level covariates account for time-varying factors that might confound inference and capture features that might influence an individual's decision to locate in an area prone to wildfires. To further strengthen the plausibility of conditional exogeneity, we add state fixed effects in one model. The state fixed effects help to account for local forest management policies and environmental characteristics to the extent that they are time-invariant, which might influence the incidence of forest fires. Finally, we conduct a sensitivity analysis that indicates there is unlikely to be an unobserved covariate that would alter our conclusions (Figure B2).

We also estimate a model using a panel matching estimator, which should help maximize covariate balance based on the pre-treatment trajectories (Imai, Kim, and Wang 2021). Appendix B.6 reports the results from this separate matching analysis, which are consistent.

Results: Effect of Wildfires on Climate Policy Preference Change

Figure 5 plots the average treatment effect on the treated (ATT) of wildfire experience on climate beliefs and preferences. Experiencing a wildfire causes a 2.5 percentage point increase in support for climate policy for people who live in counties that face future climate damages but not those in areas that do not anticipate damages. This is similar to the 5-6 percentage points estimate that Hazlett and Mildenberger (2020) find for the effect of wildfires on support for climate-related ballot measures. Moreover, the treatment effect is meaningful given the polarization of climate attitudes (Egan and Mullin 2017).

Figure 5: ATT of Wildfires and Placebo Tests on Climate Beliefs and Preferences



Notes: Bars denote 95% confidence intervals. Estimates from Table B2. Placebo tests use future fires.

One alternative explanation is that there is a time-varying process taking place in counties that are especially vulnerable to wildfires which explains this change in opinion. For example, vulnerable counties might hear more media coverage about climate change and it is those political messages, as opposed to direct experience, that cause preference change.[24]

To evaluate the possibility of an unobserved time-varying confounder like media coverage, we conduct placebo tests that use future fires in 2016, 2018, and 2020 as the treatment, which would affect preferences if there is an unobserved time-varying process but would not if the hypothesis about experience is right. The right facet of Figure 5 presents the placebo test results. The coefficients for both the constituent term and interaction are zero. This increases confidence that there is not a time-varying feature of places predisposed to fires that drives these results; instead, it is the experience of a wildfire that leads to belief and preference updating if one resides in a location facing future climate damage.

In all, these results provide evidence in support of the first two hypotheses. Wildfire experience leads individuals to recognize that climate change is a serious problem and become more supportive of taking action to address its impacts. However, preference updating only occurs for individuals in places facing income loss from future climate damages, which

[24]The media might also cover wildfires and help people connect their personal experiences with climate policy preferences (Mutz 1994), which would complement our argument.

suggests that people are changing their beliefs and preferences according to how their income will be affected by global warming in the future.

Skeptics, Undecideds, and Believers

Our third hypothesis states that individuals with stronger prior beliefs should be less likely to update their preferences following experiential shocks. We now test this conjecture by subsetting respondents to three groups based on their climate beliefs and preferences in the first survey wave.[25] The first group is the *skeptics*, defined as individuals who believed that "Concern about global climate change is exaggerated. No action is necessary," or that "Global climate change is not occurring, this is not a real issue." The second group is the *undecideds*. These individuals believe that "There is enough evidence that climate change is taking place and some action should be taken" or "We don't know enough about global climate change, and more research is necessary before we take any actions." The last group is the believers, those who are convinced that "Global climate change has been established as a serious problem, and immediate action is necessary." We expect to see preference updating among the undecideds but less so for the skeptics and believers given the short time frame of the panel study.[26]

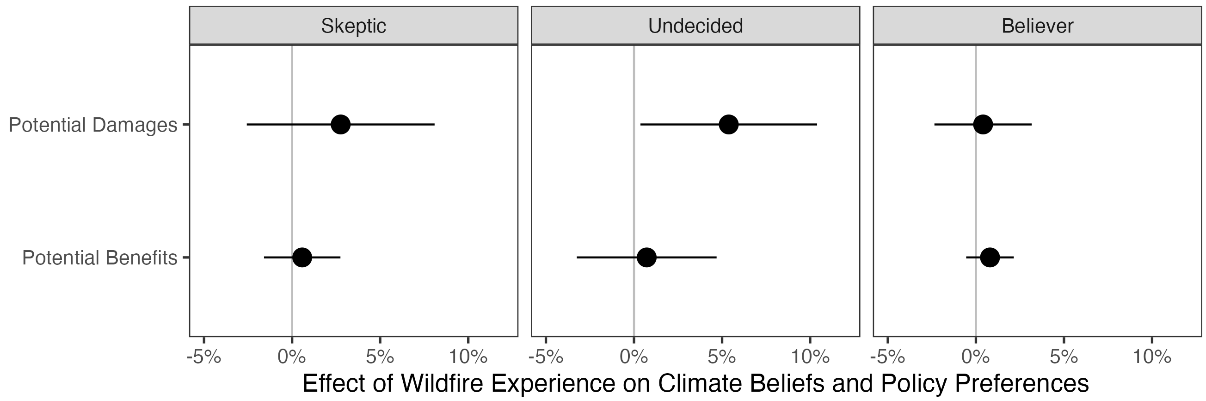
There are two notes of caution in interpreting these results. First, subsetting the data limits the statistical power required to detect an effect if one exists. Second, whether someone is a skeptic, undecided, or believer is not randomly assigned and might be correlated with unobserved factors that confound inference.[27] To the extent that the forces shaping prior

[25] Subsetting also has the useful statistical property of allowing each covariate to have a conditional effect based on the subsetted strata.

[26] Changing the minds of skeptics might take repeated experiences over longer periods given the strength of their beliefs.

[27] See Table B5 for correlations with climate skepticism. Stronger Republicans and more ideologically conservative individuals exhibit greater skepticism.

Figure 6: ATT of Wildfires on Climate Beliefs and Preferences, Conditional on Prior Beliefs



Notes: Bars denote 95% confidence intervals. Estimates from Table B4. The coefficient on the interaction term for the "Undecideds" subset, which indicates whether there is a difference between individuals in counties facing potential damages and benefits, is statistically significant at the 5% level.

beliefs are time-invariant, the individual fixed effects statistically remove this potential source of confounding. However, if the omitted variables are time-varying and not accounted for by our controls, that would undermine a causal interpretation of prior beliefs as a moderator.

Figure 6 presents the results from estimating the effect of wildfires, conditional on climate damages, on climate policy preferences for skeptics, undecideds, and believers. There is a strong, positive effect of wildfire experience on beliefs and preferences among undecideds — a 4.7 percent increase in belief in the seriousness of climate change and that the issue merits action. However, experiential shocks do not affect individuals with stronger prior beliefs. There is no effect for believers, which is unsurprising because there was no room for upward movement. Still, the result conforms with our expectation that beliefs and preferences would not revert. For skeptics, the coefficient on the interaction term is positive but noisily estimated. The research design may have been underpowered to detect what we anticipated to be a small effect here due to the lesser number of skeptics. In all, these results offer support for the third hypothesis.

Conclusion

Our paper synthesized political economy and behavioral theories to explain how preferences change when there is uncertainty about who benefits and loses from government policies. Direct experience with the consequences of public policy or its absence — experiential shocks — causes beliefs about the distributive effects of public policies to update, which leads individuals to adjust their preferred policies.

We applied our theory to the pressing question of whether experiencing climate change will lead to greater recognition of its existence, the salience of the issue, and support for action to mitigate damages. Departing from earlier studies, our results show that individuals respond to climate shocks in line with how their future income would be impacted by global warming; if they live in areas facing future climate damages, climate recognition, salience, and policy support grows. However, people living in areas that may experience little damage or even potential benefits in terms of income do not update their beliefs and preferred policies following experiential shocks. Future research should explore the durability of these effects, as some have begun to do (Arias and Blair 2023; Bergquist and Warshaw 2019), which may be crucial for translating opinion into government responses.

A political implication of this finding is that public mobilization to mitigate climate change will not automatically emerge from experiencing disasters. In places expecting damages, the public should become increasingly concerned, which could translate into policy outcomes in the long run. However, climate disturbances appear insufficient to change beliefs and preferences in places facing potential benefits. Building a political coalition for decarbonization in these locations may instead depend on emphasizing the co-benefits from emissions mitigation, such as reduced air pollution, as seen with the political logic of the recent climate law in the United States.

Our theory has broad applicability. It is common in emerging technological, scientific, and economic domains for there to be uncertainty about the costs and benefits of an issue. For example, people may be unsure about the consequences of automation, but with the deployment of artificial intelligence chatbots, they come to learn how they are affected through personal experience in the workplace. As we theorize, these experiences could alter their preferences regarding the appropriate public policy response. Future research could use our theory to explore how individuals change their policy preferences when there is uncertainty about the effects of automation, emerging technologies, or air pollution.

There are four limitations of this study that we are addressing in future work. First, although the outcome measures capture if beliefs and preferences evolve, more fine-grained measures would be useful to better pinpoint what precisely changed inside one's mind in response to an experiential shock. Additional efforts should focus on a broader set of climate beliefs, expectations of future damages, and preferences over policy responses.

Second, while our theory predicts that skeptics will eventually update their beliefs, we find no evidence of this in our current study. An appropriate research design would require panel surveys over a more extended period and at more frequent intervals to capture how skeptics update their beliefs which is likely an incremental process, if at all.

Third, our focus here has been on beliefs and policy support, which may not capture if these attitudes translate into action. In parallel work, we are examining behavioral outcomes like voting for politicians who support addressing climate change or making costly personal adaptation decisions.

Lastly, we do not focus on the role of the media, parties, and elites in politicizing personal experience (Mutz 1994). This is necessary to build the foundation of a preference updating model, and our empirical evidence indicates that our parsimonious framework generates new insights about political responses to climate change. Our findings invite further research on the role political communication in the media from parties and elites might play in activating or depoliticizing personal experience. For example, Hai and Perlman (2022) find that Republican lawmakers have few electoral incentives to attribute disasters to climate change, which could contribute to different responses to personal experiences among partisans (Hazlett and Mildenberger 2020).

This paper makes three contributions. First, we demonstrate how political economy theory can incorporate behavioral insights without sacrificing parsimony. Our synthesis is productive as it generates new predictions. Whereas political economy theories would predict that the material consequences of government policies must change for preferences to update, our synthesis indicates that personal experience provides information that can lead to preference updating even when the material consequences remain the same. This helps move beyond the insight that uncertainty contributes to status quo bias (Fernandez and Rodrik 1991) to understand how the level of uncertainty can change and, with it, individuals' beliefs and policy preferences.

Second, in contrast to the view that information has little effect on policy attitudes or that voters lack competence (Achen and Bartels 2016), we show how individuals can sometimes learn the right lesson from personal experience. By using an economic model of individuals' preferences, we avoid making ad hoc assumptions about how one should react to new information. Previous work on information and preferences should be revisited using the tools of political economy to provide a benchmark of how attitudes should change in response to direct experience. To the extent that these models fail to generate accurate predictions, it would also be informative by revealing the limits of the assumption that an individual's preferences are based partly on economic self-interest. This would help to pinpoint non-economic sources of belief change, including the potential for people to have other-regarding preferences (Kennard 2021).

Lastly, we contribute to the climate politics literature by integrating climate assessment models with theories of climate politics. Our approach helps resolve inconsistent results in the fast-advancing body of research on behavioral responses to climatic events (Howe et al. 2019). Previous studies evaluate the effect of experience shocks but without a model of what individuals' preferences should be if they were fully informed and acting according to economic self-interest. Instead, our climate model provides a baseline for what policy preferences should converge to, while our theory supplies a microfounded causal pathway for how individuals' preferred policies change in response to experiential shocks. In doing so, we help to better understand when and how political mobilization will occur in response to climate change.

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Online Appendix
"Preference Updating Under Uncertainty"

A Cross-Sectional Analysis Appendix

A.1 Covariate Data and Measurement

A.1.1 Subregion

We go to great lengths to find subregion-level covariates that match the administrative areas where the survey respondents reside. The first subregional covariate is population. The regional population may influence climate risk preferences through the relationships between population and factors like carbon dioxide emissions and economic growth. Population data come from WorldPop, which uses a Random Forest algorithm and a combination of census, survey, satellite, and cell phone data to generate gridded predictions of population (Tatem 2017). This approach has been validated for its accuracy (Stevens et al. 2015). We use data from 2018 at the 1-kilometer spatial resolution, which we aggregate to the $1° \times 1°$ level of our climate damage predictions. Once these are aggregated to the region level, we take the natural logarithm of the population quantity to correct for right skew.

We also include gross domestic product (GDP), a standard measure of economic development that influences the level of education in a location and the resources to adapt to climate change and implement mitigation policies. High-resolution global spatial data on GDP come from Kummu, Taka, and Guillaume (2018). Instead of down-scaling national-level data, the database employs subnational administrative data where available and only scales values where necessary, producing a more accurate measurement of GDP. We use the most recent year of data available (2015) for a measure of total GDP in 2011 international US dollars at the 30 arc-sec resolution, which we aggregate up to the level of the damage measure. Once aggregating to the region level, we take the logarithm to adjust for right-skew. Since we control for population in our model specifications, we do not convert GDP to a per capita value.

We also include variables to account for distributive politics theories of climate preferences (e.g., Mildenberger 2020). Distributive politics theories would predict opposition to climate change emerges from consumers with high carbon footprints. We use high-resolution data on CO2 emissions to account for how carbon intensity could influence preferences. Data come from EDGAR and cover all fossil sources of carbon dioxide, such as fossil fuel consumption, cement production, and agricultural use. The measure excludes organic sources of carbon dioxide, such as forest fires and land-use change. Data are from 2018 at the $0.1° \times 0.1°$ resolution.

As additional covariates to capture distributive politics theories, we map geospatial indices on coal and oil development potential to the region level. Oakleaf et al. (2019) construct these indices at the 1-kilometer resolution, using data on resource potential and development feasibility, validated by recent leases and claim boundaries for fossil fuels and mining development. Higher index values indicate greater potential for developing fossil fuels, which within standard distributive politics theories would imply lower support for climate action. Figures A1 and A2 show the spatial variation in these development potential indices.

A.1.2 Individual

We balance on a set of individual-level covariates from the climate politics literature that may be predictive of climate beliefs: age, gender, education, and household income quintile. This appendix describes the construction of these variables. We also balance on a variable measuring what the respondent thinks when she hears the word risk (e.g., opportunity or danger), as this mental association might influence answers to our main outcome about one's most significant daily risk.

- **Gender**. Gender differences in climate policy preferences manifest in high-income countries (Bush and Clayton 2023). Our empirical model specifications include a binary indicator for if a respondent identifies as female.

- **Education**. Education supplies information about the scientific mechanisms behind global warming, which is predictive of climate concern (Lee et al. 2015). We control for education using a categorical variable for if a respondent has a primary, secondary, or tertiary education. Since countries have distinct ways of classifying education levels, these categories devised by Gallup represent the most consistent way of accounting for respondents' educational attainment. Primary is the equivalent of completing up to eight years of education; secondary is the equivalent of completing between nine and 15 years of education but not a bachelor's degree or equivalent; and tertiary is 16 years or more, the functional equivalent of a bachelor's degree or more. Since countries have distinct ways of classifying education levels, these categories devised by Gallup represent the most consistent way of accounting for respondents' educational attainment. Primary is the equivalent of completing up to eight years of education; secondary is the equivalent of completing between nine and 15 years of education but not a bachelor's degree or equivalent; and tertiary is 16 years or more, the functional equivalent of a bachelor's degree or more.

- **Household income**. We account for income using a categorical variable for the respondent's monthly household income quintile. This variable is constructed from two questions. The first asks respondents about their monthly household income before taxes, which includes all wages, remittances, and other sources. If respondents did not know or refused, they were presented with an income range in the local currency and asked which group they fell into. If the respondent answered neither question, Gallup imputes the missing value using a hot-deck imputation procedure. Then, the income data are annualized, and a per capita annual income value is calculated by dividing the household income by the number of people living in the household. This per capita annual income value then serves as the basis for the income quintiles in each country survey.

- **Age**. We use a continuous variable to measure age. A few observations are right-censored since the survey firm codes all individuals over 100 in a single category to protect the respondent's identity.

- **Risk**. The item to measure risk interpretation asks, "When you hear the word RISK, do you think more about opportunity or danger?" with possible answers including, "Danger," "Opportunity," "Both," and "Neither."

A.1.3 Country

Finally, we balance on a country-level measure of regime type because the level of democracy could influence the availability of information or freedom of news coverage, which could be mechanisms through which experience shocks transmit. However, we do not have strong expectations about the effect of democracy due to the complex relationship between regime type and environmental performance. The main regime type measure is the polyarchy index from V-Dem (Coppedge et al. 2019).[28]

A.2 Sub-Region Data Crosswalk Construction

We construct a crosswalk between the sub-region names in the Gallup data and spatial polygons representing the region. We then map our climate damages and temperature data, which are geocoded to longitude-latitude grids, to the respondent's region as identified by the crosswalk.

Creating the crosswalk is a labor-intensive process. First, we locate shape files for the administrative areas, which are not standardized across countries. Second, we inspect each region's name in the Gallup data since spellings are non-uniform. Third, for cases where the Gallup regions do not match official administrative boundaries, we must create shape files using the best available information for the country. This appendix describes each of these steps and the data sources we employ.

A.2.1 Core Shapefile Data

Most shape files come from version 4.0.4 of the Database of Global Administrative Areas (GADM), which delimits 397,119 administrative areas. The administrative areas include lower-level subdivisions of countries, such as provinces and counties. We downloaded the `world.dbf` attribute table, which contains the smallest administrative-level divisions possible.[29]

A.2.2 Additional Shapefile Data

There are some countries whose sub-regions are unable to be created by what GADM provides. Table A1 contains information about the sources of shapefiles we use for these cases. Some of this data comes from the Humanitarian Data Exchange (HDX), an open platform managed by the United Nations Office for the Coordination of Humanitarian Affairs' Field Information Services Section. Additional sources include the World Bank, national statistical agencies, geographic information system experts, and other standard data repositories. All files were downloaded between June and July 2022.

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[28]The results are equivalent in a linear regression that uses the Polity2 index as a control (Table A7).

[29]Available online at: https://gadm.org/download_world.html.

Table A1: Data Sources for Additional Shapefiles

Country	Shapefile Source	URL
Botswana	HDX	https://bit.ly/3RAPNIq
Kosovo	Justin Meyers	https://bit.ly/3RyHJbb
Montenegro	StackExchange (GIS)	https://bit.ly/3TyPO1o
Morocco	Minnesota Population Center (2020)	https://bit.ly/3RATNsE
Philippines	HDX[30]	https://bit.ly/3TH27Zt
Turkey	`TRmaps` R package[31]	https://bit.ly/3QeH65E
Uganda	World Bank's energydata.info	https://bit.ly/3CWpyZc
United Kingdom	Open Geography PortalX[32]	https://bit.ly/3AJFoDD

A.2.3 Unavailable Subregion Shapefiles

There are a handful of sub-regions listed in the Gallup data where shape files could not be found. To address this challenge, we identified larger regions that contained the sub-regions. Table A2 lists the country, sub-region ID in the Gallup data and the name of the larger region containing the subregions.

Table A2: Special Cases Where Subregion Shapefiles Were Unavailable

Country	Gallup Subregion ID	Supra-Region Name
Benin	48,53	Athieme and Lokossa
Gambia	41,42,43	Banjul
Gambia	31,33	Upper Baddibu
Gambia	35,36,37	Fulladu East
Gambia	38,39	Wuli
Lebanon	1,3	Beirut
Latvia	1,6	Riga and Pieriga

A.2.4 Special Cases

There are a handful of cases where we make assumptions about the regions referenced in the Gallup data. We discuss these cases and the reasons for the coding assumptions below.

Bosnia and Herzegovina The Gallup Data Dictionary divides Republika Srpska (RS) into RS West, RS East, and RS South. However, RS West/East/South are not official sub-regions of RS. Hence, geographical quadrants were used to define these sub-regions.

[30] File downloaded: `phl_adminboundaries_candidate_exclude_adm3.zipSHP`

[31] This is an R package for making maps of Turkey.

[32] UK Office for National Statistics.

Vietnam The Data Dictionary has a sub-region named Ha Tay (22). However, Ha Tay is a former province as of 2008 and is not a part of Ha Noi.[33] Ha Tay is also not associated with any survey data, so we can trim this region.

Botswana Four assumptions were made in this country because the sub-region names in the Data Dictionary were difficult to match with the shapefile's attribute table. These assumptions are: (1) When a larger sub-region matches a Data Dictionary name, assume that the Data Dictionary name refers to this sub-region. (There are often smaller sub-regions of the same name); (2) Bobononj (Shapefile Name) refers to Bobirwa (Data Dictionary) because Google Maps indicates that these regions are, in fact, the same region; (3) Ngamiland (Shapefile Name) refers to Ngami (Data Dictionary) because there is no other match similar to Ngami; and (4) Okavango (Data Dictionary) is a delta in Ngamiland. Hence, we assume that Okvango refers to Ngamiland Delta (Shapefile Name). There is also no other match similar to Okavango.

Japan The region Niigata is associated with the Gallup sub-region Hokuriku and Koshinetsu. Since each shape can only be associated with one Gallup sub-region (so no errors occur when regions are merged to form shapes corresponding to Gallup sub-regions), Niigata is associated with Hokuriku only.

Azerbaijan Three of the four subregions listed in the Data Dictionary ("Eastern part," Northern part," and" Southern part") are not official sub-regions. Hence, geographical quadrants were used to define these sub-regions.

Ivory Coast Four of the five sub-regions listed in the Data Dictionary ("South," "West," "Northeast," and "Center") are not official sub-regions. Hence, geographical quadrants were used to define these sub-regions.

Tanzania The sub-regions Central, Coastal, Islands, Northern, Southern, and Western were roughly determined using secondary cartographic depictions of the country.

[33]It is possible that Ha Tay refers to Gia Lai. However, Gia Lai is already listed as a sub-region.

A.3 Summary Statistics

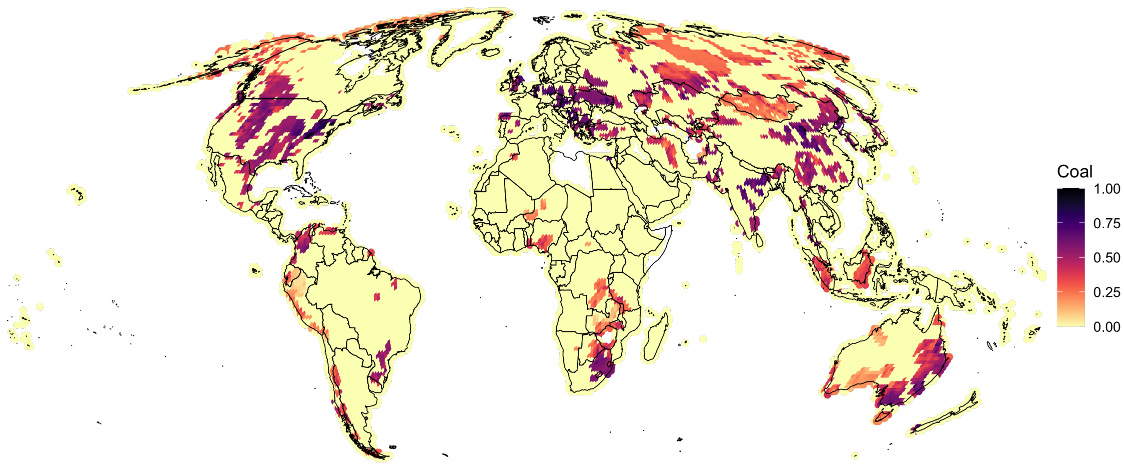
Table A3: Summary Statistics for Cross-national Survey

	Mean	SD	Min	Max	N	Missing
Outcome Variables:						
Climate is Top/Major Risk	0.05	0.22	0.00	1.00	133683	0
Climate is Top Risk	0.03	0.17	0.00	1.00	133683	0
Politics is Top Risk (placebo)	0.02	0.13	0.00	1.00	133683	0
Work Accident is Top Risk (placebo)	0.02	0.15	0.00	1.00	133683	0
Explanatory Variable:						
Δ Temp. Variability	0.07	0.99	−2.54	7.49	133272	411
Moderator:						
Potential Damages	0.79	0.41	0.00	1.00	133182	501
Individual-Level Controls:						
Age	42.40	18.23	15.00	99.00	133372	311
Female	0.54	0.50	0.00	1.00	133683	0
Risk is Danger	0.66	0.47	0.00	1.00	133683	0
Tertiary Education	0.17	0.38	0.00	1.00	133683	0
Income Quintile	3.20	1.42	1.00	5.00	132603	1080
Subregion-Level Controls:						
GDP (log)	26.89	2.43	13.53	32.28	132140	1543
CO2 Emissions (log)	−10.15	2.31	−19.69	−4.80	133182	501
Population (log)	17.76	2.26	0.04	22.41	132986	697
Coal Development Potential Index	0.10	0.21	0.00	0.92	133182	501
Oil Development Potential Index	0.24	0.30	0.00	0.99	133182	501
Country-Level Controls:						
Polyarchy	0.55	0.26	0.02	0.91	133683	0

Notes: Data cover 123 countries and 2,255 regions. Temperature variability change is standardized. Risk is Danger and Tertiary Education are dichotomized in the summary table for exposition but treated as categorical in analysis to avoid information loss. Income Quintile is numeric in the summary table but treated as categorical in analysis.

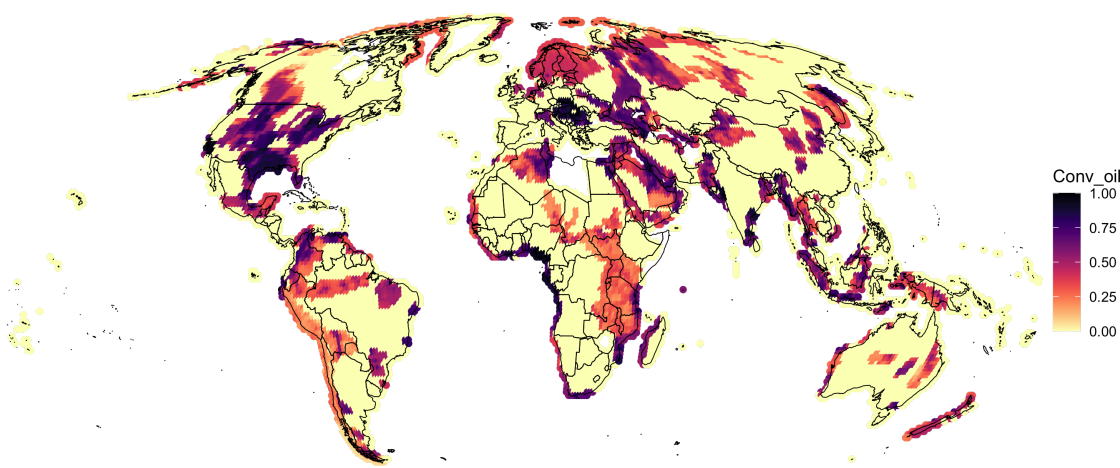
A.4 Spatial Distribution of Subregion-Level Data

Figure A1: Coal Development Potential



Notes: Data from Oakleaf et al. (2019).

Figure A2: Conventional Oil Development Potential



Notes: Data from Oakleaf et al. (2019).

A.5 Covariate Balancing

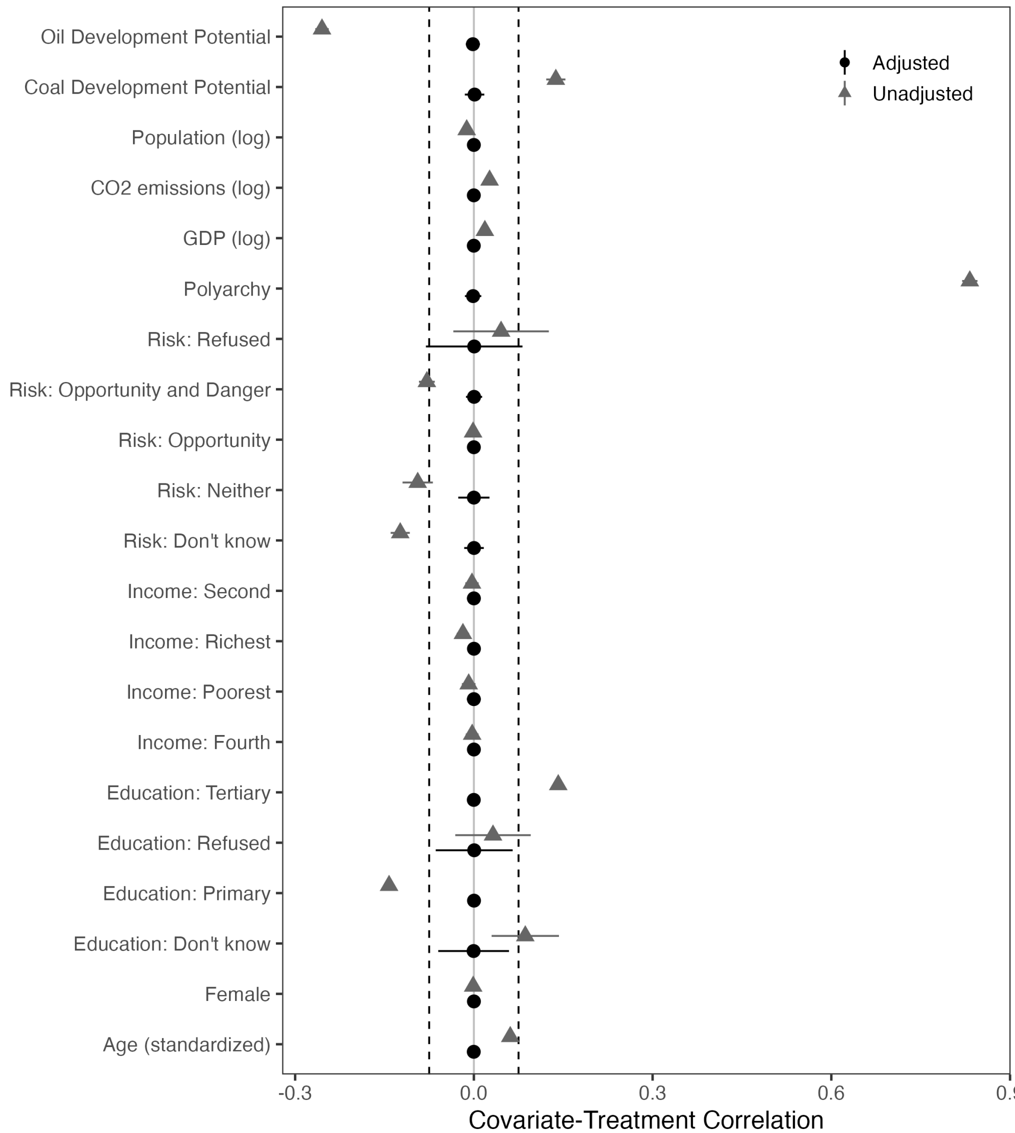
To enhance the plausibility of the conditional ignorability assumption, we estimate covariate balancing propensity scores (CBPS) from which we construct weights. The CBPS has two primary advantages for our research setting. First, it can be applied to continuous treatments. Second, "the CBPS estimation mitigates the effect of the potential misspecification of a parametric propensity score model by selecting parameter values that maximize the resulting covariate balance" (Imai and Ratkovic 2014, 244).

The treatment is the interaction of the potential damages moderator with the long-run change in temperature variability (Δ Temp. Variability $\times$ Potential Damages). The covariates upon which balance will be optimized include the individual-level, subregion-level, and country-level variables depicted in Figure A3. Since the sample has many observations, we use a computing cluster to estimate the covariate balancing weights. We verify that the model converges.

Figure A3 reports the correlation between the treatment and covariates, which is the standard statistic to report for balance when the treatment is continuous. If there is balance, the correlation should approach 0. Figure A3 shows that in the unadjusted sample, there is mild covariate imbalance for some variables like age and education. However, almost perfect balance is achieved once the sample is adjusted with covariate balancing weights.

We follow the default recommendation from Kruschke (2018) to set the equivalence range between $[-0.1\sigma_y, 0.1\sigma_y]$.

Figure A3: Equivalence Tests for Covariate Balance in Unadjusted and Adjusted Samples



Notes: Covariate balancing propensity scores estimated (Imai and Ratkovic 2014). The treatment is defined as the interaction of the potential damages moderator with the long-run change in temperature variability (Δ Temp. Variability $\times$ Potential Damages). Since the treatment is continuous, the reported balancing statistic is the treatment-covariate Pearson correlation. Dashed black lines denote the equivalence range $[-0.1\sigma_y, 0.1\sigma_y]$. Bars around the point estimates are 95% confidence intervals. The sample is from the Gallup World Risk Poll ($N = 131{,}380$).

A.6 Regression Estimates

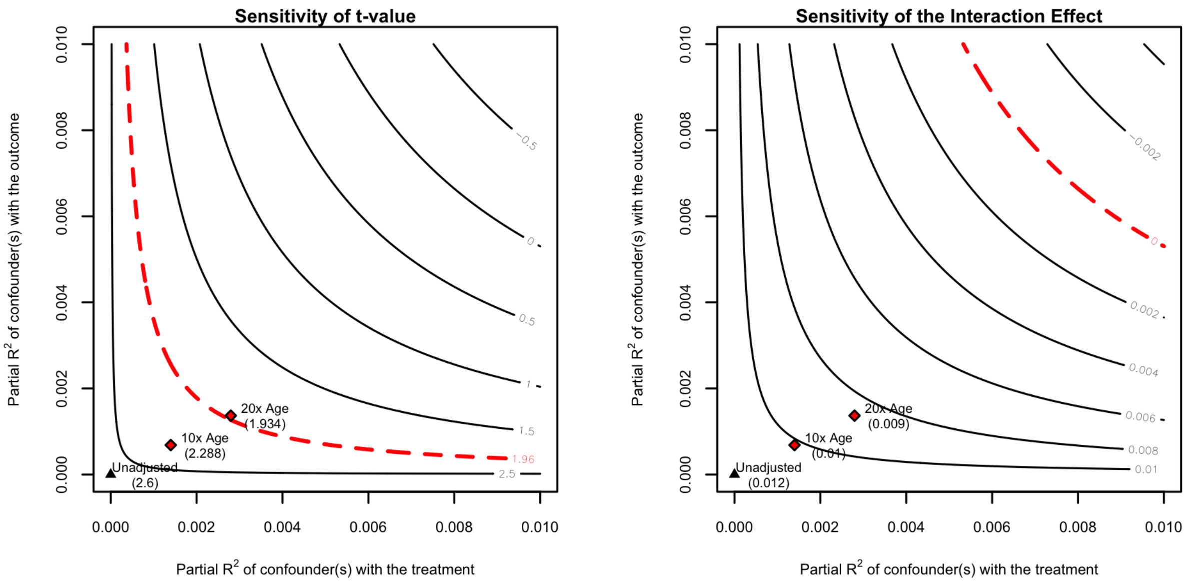
Table A4: Linear Regression of Climate Risk Salience and Non-Climate Placebos on Long-Run Temperature Variability Change

	Climate Risk Salience:		Placebo Tests:	
	Top/Major	Top	Work	Politics
Δ Temp. Variability	−0.005	−0.004*	0.001	0.001
	(0.004)	(0.002)	(0.001)	(0.002)
Potential Damages	−0.022**	−0.016**	0.005*	0.001
	(0.010)	(0.007)	(0.003)	(0.004)
Δ Temp. Variability $\times$ Potential Damages	0.011***	0.009***	0.000	0.000
	(0.004)	(0.003)	(0.002)	(0.002)
N	130 377	130 377	130 377	130 377
Outcome Mean	0.054	0.03	0.023	0.019
F	9.3	6.36	22.26	11.2
Global Region Fixed Effects	Yes	Yes	Yes	Yes
Covariate Balancing Weights	Yes	Yes	Yes	Yes

Notes: HC1 standard errors clustered by subregion. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

A.7 Sensitivity Analysis

Figure A4: Sensitivity Analysis of Omitted Variable Bias on the Interactive Effect of Long-Run Temperature Variability Change and Potential Damages

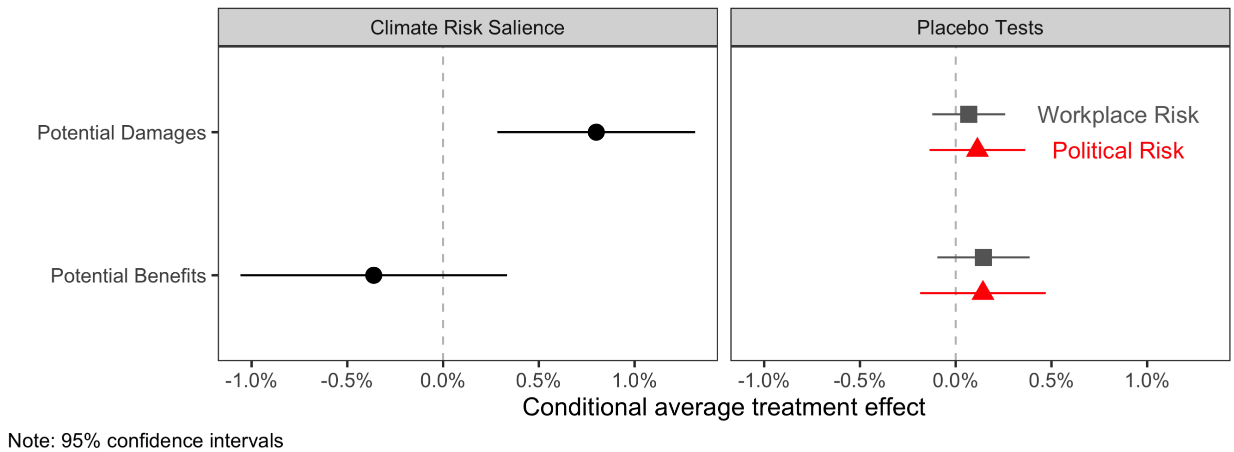


Notes: Bias contour plots of the t-value (left) and ATT estimate (right). Red diamonds indicate that a confounder up to 1500% as strong as the observed age covariate would not bring the lower bound of the confidence below 0 at the 5 percent significance level, while a confounder at least 7000% as strong as the observed age covariate would not bring the estimate to 0. Estimates from a linear regression with controls and HC1 standard errors clustered by subregion (Table A5).

A.8 Robustness Checks

A.8.1 Covariates Instead of Balancing Weights

Figure A5: Conditional Average Treatment Effect of Long-Run Changes in Temperature Variability



Notes: Conditional average treatment effect of long-run changes in temperature variability (left panel) and placebo tests (right panel) on risk salience for respondents in regions facing potential damages or benefits. Models include the full set of individual-, subregion-, and country-level controls. HC1 standard errors clustered by region. Table A5 contains the complete regression results.

Table A5: Linear Regressions of Risk Salience on the Interaction of Long-Run Change in Temperature Variability and Potential Climate Damages

	Climate Risk Salience:		Placebo Tests:	
	Top/Major	Top	Work	Politics
Intercept	0.098	0.076	0.015	−0.013
	(0.075)	(0.057)	(0.027)	(0.033)
Effect of Climate Experience				
Δ Temp. Variability	−0.004	−0.004*	0.001	0.001
	(0.004)	(0.002)	(0.001)	(0.002)
Potential Damages	−0.019**	−0.014**	0.006**	0.002
	(0.009)	(0.007)	(0.002)	(0.003)
Δ Temp. Variability × Potential Damages	0.012***	0.009***	−0.001	0.000
	(0.004)	(0.003)	(0.001)	(0.002)
Individual-Level Controls				
Age (standardized)	0.003***	0.002***	−0.005***	0.002***
	(0.001)	(0.001)	(0.001)	(0.000)
Female	0.000	0.001	−0.026***	−0.007***
	(0.002)	(0.001)	(0.001)	(0.001)
Education: Primary	0.003	0.005***	0.000	−0.003***
	(0.002)	(0.002)	(0.001)	(0.001)
Education: Tertiary	0.007***	0.002	−0.011***	0.010***
	(0.002)	(0.001)	(0.001)	(0.002)
Education: Don't know	−0.014	−0.005	−0.013***	−0.013***
	(0.009)	(0.007)	(0.004)	(0.003)
Education: Refused	−0.010	−0.004	−0.015***	−0.003
	(0.011)	(0.007)	(0.004)	(0.006)
Income: Poorest	0.000	0.002	−0.003**	−0.001
	(0.003)	(0.002)	(0.001)	(0.001)
Income: Second	0.001	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Income: Fourth	0.001	0.000	0.002*	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Income: Richest	0.000	−0.002	0.003**	0.003**
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Opportunity	−0.016***	−0.008***	−0.002*	−0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Neither	−0.022***	−0.011	−0.009***	−0.003
	(0.008)	(0.008)	(0.003)	(0.003)
Risk: Opportunity and Danger	−0.002	−0.002	0.004*	0.002
	(0.003)	(0.003)	(0.003)	(0.002)
Risk: Don't know	−0.034***	−0.020***	−0.011***	−0.007***
	(0.003)	(0.002)	(0.001)	(0.001)
Risk: Refused	−0.044***	−0.028***	−0.003	0.003
	(0.012)	(0.010)	(0.008)	(0.009)
Subregion-Level Controls				
GDP (log)	−0.005*	−0.004**	0.001	−0.003
	(0.003)	(0.002)	(0.001)	(0.002)
CO2 emissions (log)	−0.001	0.000	0.000	−0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Population (log)	0.004*	0.003*	−0.002**	0.005**
	(0.002)	(0.002)	(0.001)	(0.002)
Coal Development Potential	−0.022***	−0.014**	0.003	−0.006
	(0.008)	(0.006)	(0.003)	(0.004)
Oil Development Potential	−0.004	−0.003	0.008***	−0.001
	(0.006)	(0.004)	(0.002)	(0.003)
Country-Level Controls				
Polyarchy	0.027**	0.012	0.017***	0.033***
	(0.011)	(0.008)	(0.004)	(0.009)
N	130 377	130 377	130 377	130 377
Global Region Fixed Effects	Yes	Yes	Yes	Yes

Notes: Temperature variability change is standardized. HC1 standard errors clustered by subregion. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

A.8.2 Multi-Level Model

Table A6: Multi-Level Model with Random Intercepts for Subregions

	Climate Risk Salience:		Placebo Tests:	
	Top/Major	Top	Work	Politics
Intercept	0.011	0.013	0.022	0.008
	(0.062)	(0.042)	(0.029)	(0.029)
Effect of Climate Experience				
Δ Temp. Variability	−0.004	−0.003	0.002	0.002
	(0.003)	(0.002)	(0.002)	(0.002)
Potential Damages	−0.018***	−0.012***	0.007**	0.006**
	(0.006)	(0.004)	(0.003)	(0.003)
Δ Temp. Variability $\times$ Potential Damages	0.010***	0.008***	−0.001	−0.002
	(0.004)	(0.003)	(0.002)	(0.002)
Individual-Level Controls				
Age (standardized)	0.004***	0.002***	−0.005***	0.002***
	(0.001)	(0.001)	(0.000)	(0.000)
Female	0.001	0.001	−0.026***	−0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Education: Primary	0.000	0.003***	0.001	−0.004***
	(0.002)	(0.001)	(0.001)	(0.001)
Education: Tertiary	0.009***	0.003*	−0.010***	0.009***
	(0.002)	(0.001)	(0.001)	(0.001)
Education: Don't know	−0.015	−0.008	−0.013*	−0.011*
	(0.010)	(0.008)	(0.007)	(0.006)
Education: Refused	−0.007	−0.004	−0.012	0.000
	(0.011)	(0.009)	(0.008)	(0.007)
Income: Poorest	0.000	0.001	−0.003**	−0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Income: Second	0.000	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Income: Fourth	0.001	0.000	0.003**	0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Income: Richest	−0.001	−0.002	0.004***	0.003***
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Opportunity	−0.013***	−0.006***	−0.001	−0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Neither	−0.016***	−0.007**	−0.008**	−0.002
	(0.005)	(0.003)	(0.003)	(0.003)
Risk: Opportunity and Danger	−0.003	−0.002	0.004**	0.001
	(0.002)	(0.002)	(0.002)	(0.001)
Risk: Don't know	−0.030***	−0.018***	−0.009***	−0.005***
	(0.003)	(0.002)	(0.002)	(0.002)
Risk: Refused	−0.027*	−0.015	−0.004	0.005
	(0.014)	(0.011)	(0.010)	(0.008)
Subregion-Level Controls				
GDP (log)	−0.003	−0.002	0.002	−0.002
	(0.003)	(0.002)	(0.001)	(0.001)
CO2 emissions (log)	−0.003*	−0.002	0.000	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Population (log)	0.005*	0.002	−0.003***	0.002**
	(0.002)	(0.002)	(0.001)	(0.001)
Coal Development Potential	−0.016**	−0.010*	0.002	−0.007*
	(0.008)	(0.005)	(0.004)	(0.004)
Oil Development Potential	−0.003	−0.002	0.008***	0.004
	(0.005)	(0.004)	(0.003)	(0.002)
Country-Level Controls				
Polyarchy	0.039***	0.017**	0.012***	0.031***
	(0.010)	(0.007)	(0.005)	(0.005)
N	130 377	130 377	130 377	130 377
Global Region Fixed Effects	Yes	Yes	Yes	Yes

Notes: Temperature variability change is standardized. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

A.8.3 Alternative Regime Type Measure

Table A7: Polity 2 Instead of Polyarchy

	Climate Risk Salience:		Placebo Tests:	
	Top/Major	Top	Work	Politics
Intercept	0.105	0.080	0.019	−0.009
	(0.076)	(0.057)	(0.027)	(0.032)
Effect of Climate Experience				
Δ Temp. Variability	−0.004	−0.004	0.002	0.002
	(0.004)	(0.003)	(0.001)	(0.002)
Potential Damages	−0.019**	−0.014**	0.006***	0.003
	(0.009)	(0.007)	(0.002)	(0.003)
Δ Temp. Variability × Potential Damages	0.011**	0.009***	−0.001	−0.001
	(0.004)	(0.003)	(0.002)	(0.002)
Individual-Level Controls				
Age (standardized)	0.003***	0.002***	−0.005***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	0.000	0.001	−0.026***	−0.007***
	(0.002)	(0.001)	(0.001)	(0.001)
Education: Primary	0.002	0.004***	0.000	−0.004***
	(0.002)	(0.002)	(0.001)	(0.001)
Education: Tertiary	0.007***	0.002	−0.011***	0.010***
	(0.002)	(0.002)	(0.001)	(0.002)
Education: Don't know	−0.015*	−0.005	−0.013***	−0.014***
	(0.009)	(0.007)	(0.004)	(0.003)
Education: Refused	−0.010	−0.004	−0.016***	−0.004
	(0.011)	(0.007)	(0.004)	(0.006)
Income: Poorest	0.000	0.002	−0.003**	−0.001
	(0.003)	(0.002)	(0.001)	(0.001)
Income: Second	0.001	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Income: Fourth	0.001	0.000	0.002*	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Income: Richest	0.000	−0.002	0.003**	0.003*
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Opportunity	−0.016***	−0.008***	−0.002*	−0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Neither	−0.023***	−0.011	−0.009***	−0.004
	(0.008)	(0.008)	(0.003)	(0.003)
Risk: Opportunity and Danger	−0.002	−0.002	0.004	0.002
	(0.004)	(0.003)	(0.003)	(0.002)
Risk: Don't know	−0.035***	−0.020***	−0.011***	−0.007***
	(0.003)	(0.002)	(0.001)	(0.001)
Risk: Refused	−0.044***	−0.028***	−0.004	0.002
	(0.012)	(0.010)	(0.008)	(0.009)
Subregion-Level Controls				
GDP (log)	−0.004	−0.003*	0.002	−0.002
	(0.002)	(0.002)	(0.001)	(0.002)
CO2 emissions (log)	−0.001	0.000	0.000	−0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Population (log)	0.003	0.002	−0.003***	0.003*
	(0.002)	(0.002)	(0.001)	(0.002)
Coal Development Potential	−0.022***	−0.014**	0.004	−0.005
	(0.008)	(0.006)	(0.003)	(0.005)
Oil Development Potential	−0.004	−0.003	0.009***	−0.001
	(0.006)	(0.004)	(0.002)	(0.003)
Country-Level Controls				
Polity2	0.000	0.000	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
N	129 378	129 378	129 378	129 378
Global Region Fixed Effects	Yes	Yes	Yes	Yes

Notes: Linear regressions of risk salience on the interaction of long-run change in temperature variability and potential climate damages. Temperature variability change is standardized. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

A.8.4 National Fossil Fuel Rents Covariate

Table A8: Country-Level Fossil Fuel Rent Controls

	Climate Risk Salience:		Placebo Tests:	
	Top/Major	Top	Work	Politics
Intercept	0.111	0.086	0.003	0.000
	(0.075)	(0.057)	(0.027)	(0.034)
Effect of Climate Experience				
Δ Temp. Variability	−0.004	−0.004	0.000	0.002
	(0.004)	(0.002)	(0.001)	(0.002)
Potential Damages	−0.020**	−0.015**	0.008***	0.000
	(0.009)	(0.007)	(0.002)	(0.004)
Δ Temp. Variability $\times$ Potential Damages	0.012***	0.009***	0.001	−0.001
	(0.004)	(0.003)	(0.001)	(0.002)
Individual-Level Controls				
Age (standardized)	0.002***	0.002***	−0.005***	0.002***
	(0.001)	(0.001)	(0.000)	(0.000)
Female	0.000	0.001	−0.026***	−0.007***
	(0.002)	(0.001)	(0.001)	(0.001)
Education: Primary	0.003	0.005***	0.000	−0.003***
	(0.002)	(0.002)	(0.001)	(0.001)
Education: Tertiary	0.007***	0.002	−0.011***	0.010***
	(0.002)	(0.001)	(0.001)	(0.002)
Education: Don't know	−0.014	−0.004	−0.014***	−0.013***
	(0.009)	(0.007)	(0.004)	(0.003)
Education: Refused	−0.010	−0.004	−0.015***	−0.003
	(0.011)	(0.007)	(0.004)	(0.006)
Income: Poorest	0.000	0.002	−0.003**	−0.001
	(0.003)	(0.002)	(0.001)	(0.001)
Income: Second	0.001	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Income: Fourth	0.001	0.000	0.002*	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Income: Richest	0.000	−0.002	0.003**	0.003**
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Opportunity	−0.016***	−0.008***	−0.002*	−0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Neither	−0.022***	−0.011	−0.008***	−0.003
	(0.008)	(0.008)	(0.003)	(0.003)
Risk: Opportunity and Danger	−0.002	−0.002	0.005*	0.002
	(0.003)	(0.003)	(0.003)	(0.002)
Risk: Don't know	−0.034***	−0.020***	−0.010***	−0.007***
	(0.003)	(0.002)	(0.001)	(0.001)
Risk: Refused	−0.044***	−0.028***	−0.004	0.003
	(0.012)	(0.010)	(0.008)	(0.009)
Subregion-Level Controls				
GDP (log)	−0.006**	−0.004**	0.002	−0.004**
	(0.003)	(0.002)	(0.001)	(0.002)
CO2 emissions (log)	−0.001	0.000	0.000	−0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Population (log)	0.006**	0.004*	−0.003**	0.006***
	(0.003)	(0.002)	(0.001)	(0.002)
Coal Development Potential	−0.019**	−0.012**	0.000	−0.003
	(0.008)	(0.006)	(0.003)	(0.004)
Oil Development Potential	−0.005	−0.004	0.009***	−0.002
	(0.006)	(0.004)	(0.002)	(0.003)
Country-Level Controls				
Polyarchy	0.035***	0.016*	0.013***	0.040***
	(0.012)	(0.009)	(0.004)	(0.010)
Coal rents as % of GDP	−0.003	−0.003	0.006***	−0.004***
	(0.003)	(0.002)	(0.001)	(0.001)
Oil rents as % of GDP	0.001**	0.000	0.000	0.001*
	(0.000)	(0.000)	(0.000)	(0.000)
N	130 377	130 377	130 377	130 377
Global Region Fixed Effects	Yes	Yes	Yes	Yes

Notes: Linear regressions of risk salience on the interaction of long-run change in temperature variability and potential climate damages. Temperature variability change is standardized. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

A.8.5 National Climate Law Covariate

Table A9: Control for the National Climate Law Stock

	Climate Risk Salience:		Placebo Tests:	
	Top/Major	Top	Work	Politics
Intercept	0.098	0.077	0.015	−0.013
	(0.075)	(0.056)	(0.027)	(0.033)
Effect of Climate Experience				
Δ Temp. Variability	−0.004	−0.004	0.002	0.001
	(0.004)	(0.003)	(0.001)	(0.002)
Potential Damages	−0.019**	−0.014**	0.006**	0.002
	(0.009)	(0.007)	(0.002)	(0.003)
Δ Temp. Variability × Potential Damages	0.011**	0.009***	−0.001	0.000
	(0.005)	(0.003)	(0.001)	(0.002)
Individual-Level Controls				
Age (standardized)	0.003***	0.002***	−0.005***	0.002***
	(0.001)	(0.001)	(0.001)	(0.000)
Female	0.000	0.001	−0.026***	−0.007***
	(0.002)	(0.001)	(0.001)	(0.001)
Education: Primary	0.003	0.005***	0.000	−0.003***
	(0.002)	(0.002)	(0.001)	(0.001)
Education: Tertiary	0.007***	0.002	−0.011***	0.010***
	(0.002)	(0.001)	(0.001)	(0.002)
Education: Don't know	−0.014	−0.005	−0.013***	−0.013***
	(0.009)	(0.007)	(0.004)	(0.003)
Education: Refused	−0.009	−0.003	−0.015***	−0.003
	(0.011)	(0.007)	(0.004)	(0.006)
Income: Poorest	0.000	0.002	−0.003**	−0.001
	(0.003)	(0.002)	(0.001)	(0.001)
Income: Second	0.001	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Income: Fourth	0.001	0.000	0.002*	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Income: Richest	0.000	−0.002	0.003**	0.003**
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Opportunity	−0.016***	−0.008***	−0.002*	−0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Neither	−0.022***	−0.011	−0.009***	−0.003
	(0.008)	(0.008)	(0.003)	(0.003)
Risk: Opportunity and Danger	−0.002	−0.002	0.005*	0.002
	(0.003)	(0.003)	(0.003)	(0.002)
Risk: Don't know	−0.034***	−0.020***	−0.011***	−0.007***
	(0.003)	(0.002)	(0.001)	(0.001)
Risk: Refused	−0.044***	−0.028***	−0.003	0.003
	(0.012)	(0.010)	(0.008)	(0.009)
Subregion-Level Controls				
GDP (log)	−0.005*	−0.004**	0.001	−0.003
	(0.003)	(0.002)	(0.001)	(0.002)
CO2 emissions (log)	−0.001	0.000	0.000	−0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Population (log)	0.004*	0.003*	−0.002**	0.005**
	(0.002)	(0.002)	(0.001)	(0.002)
Coal Development Potential	−0.022***	−0.014**	0.003	−0.006
	(0.008)	(0.006)	(0.003)	(0.004)
Oil Development Potential	−0.003	−0.003	0.009***	−0.001
	(0.006)	(0.004)	(0.002)	(0.003)
Country-Level Controls				
Polyarchy	0.022*	0.010	0.014***	0.033***
	(0.012)	(0.008)	(0.004)	(0.009)
Climate Law Stock	0.000	0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
N	130 377	130 377	130 377	130 377
Global Region Fixed Effects	Yes	Yes	Yes	Yes

Notes: Climate law data from (Nachmany et al. 2017). Linear regressions of risk salience on the interaction of long-run change in temperature variability and potential climate damages. Temperature variability change is standardized. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

A.8.6 Alternative Temperature Baseline

Table A10: 1980-1990 as Historical Benchmark for Temperature Variability

	Climate Risk Salience:		Placebo Tests:	
	Top/Major	Top	Work	Politics
Intercept	0.094	0.073	0.015	−0.013
	(0.075)	(0.056)	(0.027)	(0.033)
Effect of Climate Experience				
Δ Temp. Variability	−0.003	−0.003	0.002	0.001
	(0.004)	(0.002)	(0.002)	(0.002)
Potential Damages	−0.017*	−0.013**	0.006**	0.002
	(0.009)	(0.006)	(0.002)	(0.003)
Δ Temp. Variability $\times$ Potential Damages	0.011**	0.009***	0.000	−0.001
	(0.004)	(0.003)	(0.002)	(0.002)
Individual-Level Controls				
Age (standardized)	0.003***	0.002***	−0.005***	0.002***
	(0.001)	(0.001)	(0.001)	(0.000)
Female	0.000	0.001	−0.026***	−0.007***
	(0.002)	(0.001)	(0.001)	(0.001)
Education: Primary	0.003	0.005***	0.000	−0.003***
	(0.002)	(0.002)	(0.001)	(0.001)
Education: Tertiary	0.007***	0.002	−0.011***	0.010***
	(0.002)	(0.001)	(0.001)	(0.002)
Education: Don't know	−0.014	−0.005	−0.013***	−0.013***
	(0.009)	(0.007)	(0.004)	(0.003)
Education: Refused	−0.010	−0.004	−0.015***	−0.003
	(0.011)	(0.007)	(0.004)	(0.006)
Income: Poorest	0.000	0.002	−0.003**	−0.001
	(0.003)	(0.002)	(0.001)	(0.001)
Income: Second	0.001	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Income: Fourth	0.001	0.000	0.002*	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Income: Richest	0.000	−0.002	0.003**	0.003**
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Opportunity	−0.016***	−0.008***	−0.002*	−0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Neither	−0.022***	−0.011	−0.009***	−0.003
	(0.008)	(0.008)	(0.003)	(0.003)
Risk: Opportunity and Danger	−0.002	−0.002	0.004*	0.002
	(0.003)	(0.003)	(0.003)	(0.002)
Risk: Don't know	−0.034***	−0.020***	−0.011***	−0.007***
	(0.003)	(0.002)	(0.001)	(0.001)
Risk: Refused	−0.044***	−0.028***	−0.003	0.003
	(0.012)	(0.010)	(0.008)	(0.009)
Subregion-Level Controls				
GDP (log)	−0.005*	−0.004*	0.001	−0.003
	(0.003)	(0.002)	(0.001)	(0.002)
CO2 emissions (log)	−0.001	0.000	0.000	−0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Population (log)	0.004*	0.003	−0.003**	0.005**
	(0.002)	(0.002)	(0.001)	(0.002)
Coal Development Potential	−0.022***	−0.014**	0.003	−0.006
	(0.008)	(0.006)	(0.003)	(0.004)
Oil Development Potential	−0.004	−0.004	0.009***	−0.001
	(0.006)	(0.004)	(0.002)	(0.003)
Country-Level Controls				
Polyarchy	0.027**	0.012	0.017***	0.033***
	(0.011)	(0.008)	(0.004)	(0.009)
N	130 377	130 377	130 377	130 377
Global Region Fixed Effects	Yes	Yes	Yes	Yes

Notes: Linear regressions of risk salience on the interaction of long-run change in temperature variability and potential climate damages. Temperature variability change is standardized. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$

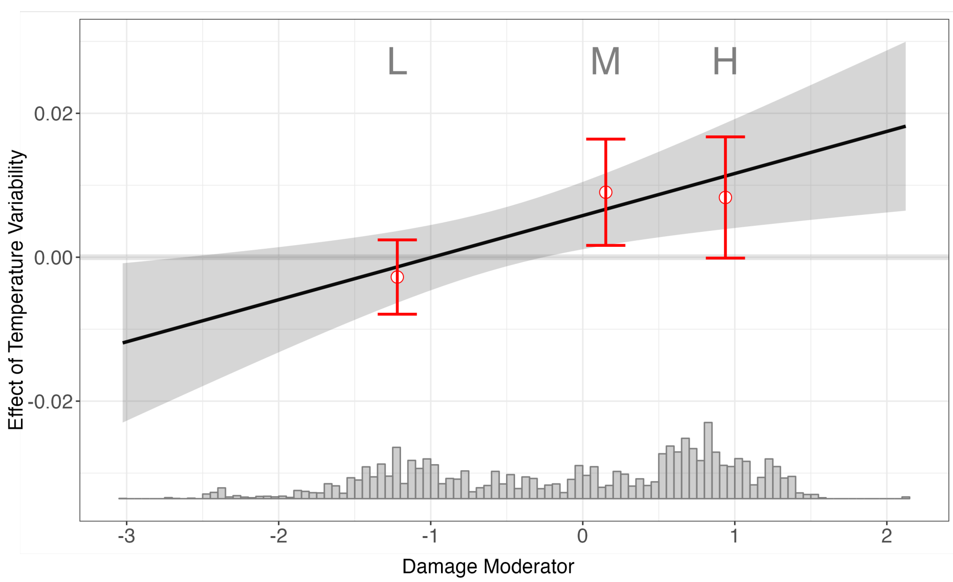
A.8.7 Continuous Moderator

Table A11: Linear Regressions of Risk Salience on the Interaction of Long-Run Change in Temperature Variability and a Continuous Measure of Climate Damages

	Top/Major	Top	Work	Politics
Δ Temp. Variability	0.004**	0.003**	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.002)
Potential Damages (continuous)	−0.010**	−0.007**	−0.001	−0.004**
	(0.005)	(0.004)	(0.002)	(0.002)
Δ Temp. Variability $\times$ Potential Damages (continuous)	0.005**	0.004***	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
N	130 377	130 377	130 377	130 377
Global Region Fixed Effects	Yes	Yes	Yes	Yes
Covariate Balancing Weights	Yes	Yes	Yes	Yes

Notes: HC1 standard errors clustered by subregion. CBPS employed (Imai and Ratkovic 2014). $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

Figure A6: Binning Estimator for Moderating Relationship of Potential Damages on the Effect of Long-Run Temperature Variability Changes



Notes: Binning estimator as recommended by Hainmueller, Mummolo, and Xu (2019) for the model with a continuous damage moderator (Table A12). Standard errors clustered by subregion. The red error bars are 95% confidence intervals around the point estimates from the tercile bins of the moderator. There is no effect of long-run temperature variability at low values of the damage moderator, a positive effect in the middle and upper tercile of damage moderator values. The plot also suggests that the conditional expectation function of the outcome given the treatment is well-approximated by a linear model across the values of the moderator.

Table A12: Linear Regressions of Risk Salience on the Interaction of Long-Run Change in Temperature Variability and a Continuous Measure of Climate Damages, Covariates Included

	Climate Risk Salience:		Placebo Tests:	
	Top/Major	Top	Work	Politics
Intercept	0.084	0.067	0.022	−0.004
	(0.074)	(0.055)	(0.028)	(0.032)
Effect of Climate Experience				
Δ Temp. Variability	0.006**	0.004**	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Potential Damages (continuous)	−0.008	−0.006*	0.000	−0.004**
	(0.005)	(0.004)	(0.002)	(0.002)
Δ Temp. Variability × Potential Damages (continuous)	0.006***	0.005***	−0.001	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Individual-Level Controls				
Age (standardized)	0.003***	0.002***	−0.005***	0.002***
	(0.001)	(0.001)	(0.001)	(0.000)
Female	0.000	0.001	−0.026***	−0.007***
	(0.002)	(0.001)	(0.001)	(0.001)
Education: Primary	0.003	0.004***	0.000	−0.003***
	(0.002)	(0.002)	(0.001)	(0.001)
Education: Tertiary	0.008***	0.002	−0.011***	0.010***
	(0.002)	(0.001)	(0.001)	(0.002)
Education: Don't know	−0.013	−0.004	−0.013***	−0.013***
	(0.009)	(0.006)	(0.004)	(0.003)
Education: Refused	−0.009	−0.003	−0.016***	−0.003
	(0.011)	(0.007)	(0.004)	(0.006)
Income: Poorest	0.000	0.002	−0.003**	−0.001
	(0.003)	(0.002)	(0.001)	(0.001)
Income: Second	0.001	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Income: Fourth	0.001	0.000	0.002*	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Income: Richest	0.000	−0.002	0.003**	0.003**
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Opportunity	−0.016***	−0.008***	−0.002*	−0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Risk: Neither	−0.022***	−0.011	−0.009***	−0.003
	(0.008)	(0.008)	(0.003)	(0.003)
Risk: Opportunity and Danger	−0.003	−0.003	0.004*	0.002
	(0.003)	(0.003)	(0.003)	(0.002)
Risk: Don't know	−0.034***	−0.020***	−0.011***	−0.007***
	(0.003)	(0.002)	(0.001)	(0.001)
Risk: Refused	−0.044***	−0.028***	−0.003	0.003
	(0.012)	(0.010)	(0.008)	(0.009)
Subregion-Level Controls				
GDP (log)	−0.004*	−0.003*	0.001	−0.003
	(0.003)	(0.002)	(0.001)	(0.002)
CO2 emissions (log)	−0.001	0.000	0.000	−0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Population (log)	0.004*	0.002	−0.002**	0.005**
	(0.002)	(0.002)	(0.001)	(0.002)
Coal Development Potential	−0.024***	−0.016***	0.003	−0.007
	(0.008)	(0.006)	(0.003)	(0.004)
Country-Level Controls				
Oil Development Potential	−0.004	−0.004	0.008***	−0.001
	(0.006)	(0.004)	(0.002)	(0.003)
Polyarchy	0.026**	0.011	0.017***	0.031***
	(0.011)	(0.008)	(0.004)	(0.009)
N	130 377	130 377	130 377	130 377
Global Region Fixed Effects	Yes	Yes	Yes	Yes

Notes: HC1 standard errors clustered by subregion. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$

B Panel Analysis Appendix

B.1 Summary Statistics

Table B1: Summary Statistics for Panel Survey Data Analysis

	Mean	SD	Min	Max	N	Missing
Climate Belief	0.54	0.50	0.00	1.00	28427	73
Fire	0.05	0.26	0.00	5.00	28498	2
Fire (=1)	0.04	0.20	0.00	1.00	28498	2
Fire (Placebo)	0.19	0.74	0.00	5.00	28498	2
Potential Damage (=1)	0.80	0.40	0.00	1.00	28498	2
Employed	0.40	0.49	0.00	1.00	28500	0
Education	3.92	1.44	1.00	6.00	28500	0
Party ID	4.16	2.28	1.00	7.00	28440	60
Ideology	2.81	1.21	1.00	5.00	28498	2
Household Income	6.89	3.07	1.00	12.00	25107	3393
Household Income (imputed)	6.89	2.88	1.00	12.00	28500	0
Religion Importance	2.83	1.17	1.00	4.00	28499	1

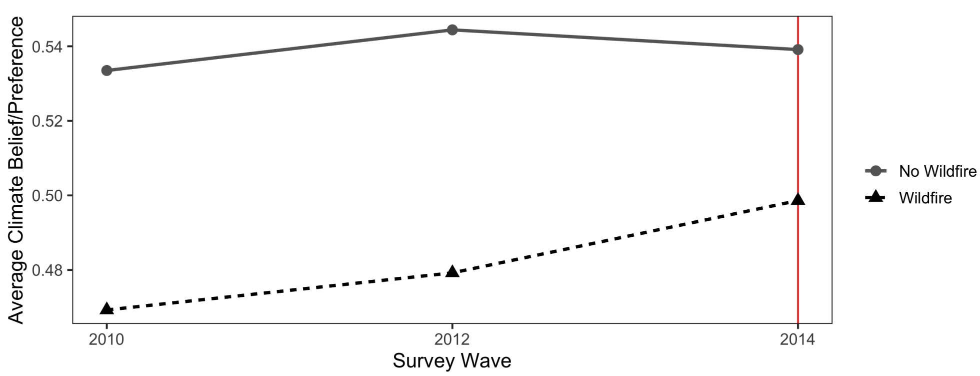
B.2 Pre-Trends

We conduct two analyses to assess the plausibility of parallel trends. First, with the panel matching estimator, we conduct a placebo test using wildfires to periods before. Figure B6 in Appendix B.6 presents the results, which indicate no violation of the parallel trends assumption.

Second, we plot the average outcome trajectory for treated and untreated units over the panel wave. Since the treatment is staggered, there is no simple pre-trend plot for all groups. The other challenge is that there are only three survey ways, so any assessment of pre-trends must compare the last wave with the first two. We overcome these limitations by subsetting the data to cases where counties have not experienced a fire until 2014 — there is no history of being treated until 2014. Then, we compare individuals in counties where there was a fire in 2014 and where there was not. While this is a subset of cases that might not generalize to units with different treatment trajectories, at the very least, it allows us to probe the plausibility of the parallel trends assumption.

Figure B1 presents the average climate beliefs for groups in counties experiencing a wildfire in 2014 with those that did not. The slopes of the lines from the 2010 to 2012 survey waves are parallel, which suggests that it is plausible to assume that treated units would have changed at the same rate as untreated units had they not been exposed to a wildfire.

Figure B1: Climate Beliefs and Preferences over Time and by Wildfire Experience



Notes: Point estimates represent the average for each group in the corresponding panel year. All units are not treated until 2014 for a valid comparison. The slopes of the line between the 2010 and 2012 survey waves are parallel, which suggests that units with this pre-treatment history exhibited comparable beliefs and preferences.

B.3 Regression Estimates

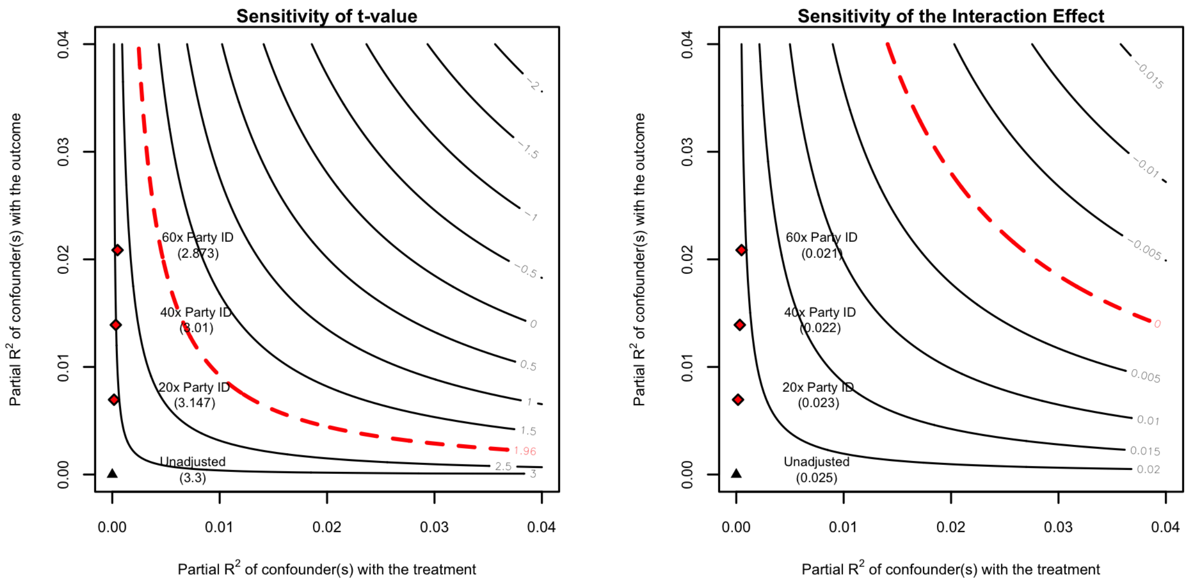
Table B2: Linear Regression of Climate Policy Support on Wildfire Experience and Placebos

	ATT on Climate Policy Support				Placebo
	(1)	(2)	(3)	(4)	(5)
Fire × Potential Damages		0.025***	0.023***		
		(0.007)	(0.007)		
Fire (=1) × Potential Damages				0.036***	
				(0.010)	
Placebo Fire × Potential Damages					0.001
					(0.004)
Fire	0.017***	0.007	0.007		
	(0.004)	(0.004)	(0.004)		
Fire (=1)				−0.005	
				(0.006)	
Placebo Fire					0.001
					(0.004)
Potential Damages	−0.019	−0.022	−0.021	−0.023	−0.021
	(0.017)	(0.017)	(0.022)	(0.017)	(0.017)
Employed	0.006	0.006	0.006	0.006	0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Education	0.004	0.004	0.004	0.004	0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Party ID	0.007*	0.007*	0.007*	0.007*	0.007*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Ideology	0.004	0.004	0.004	0.004	0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Household Income	0.001	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Religion Importance	−0.005	−0.005	−0.005	−0.004	−0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Panel Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	No	No
N	28 362	28 362	28 362	28 362	28 362
Adjusted R^2	0.841	0.841	0.841	0.841	0.841

Notes: The outcome mean is 0.54 averaged across the treatment and control groups. Heteroskedasticity robust standard errors clustered by county. Education runs from no high school (1) to post-grad (6). Party ID runs from strong Republican (1) to strong Democrat (7). Ideology runs from conservative (1) to liberal (5). Religion runs from not at all important (1) to very important (4). $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

B.4 Sensitivity Analyses

Figure B2: Sensitivity Analysis of Omitted Variable Bias on the Interactive Effect of Wildfire Experience and Potential Damages



Notes: Bias contour plots of the t-value (left) and ATT estimate (right). Red diamonds indicate that a confounder at least 60× the strength of the observed partisan identification covariate would not bring the lower bound of the confidence below 0 at the 5% significance level, while a confounder at least 60× as strong as the same benchmark covariate would not bring the estimate to 0 (a bias of 100%). Estimates from the linear regression model (2) are reported in Table B2.

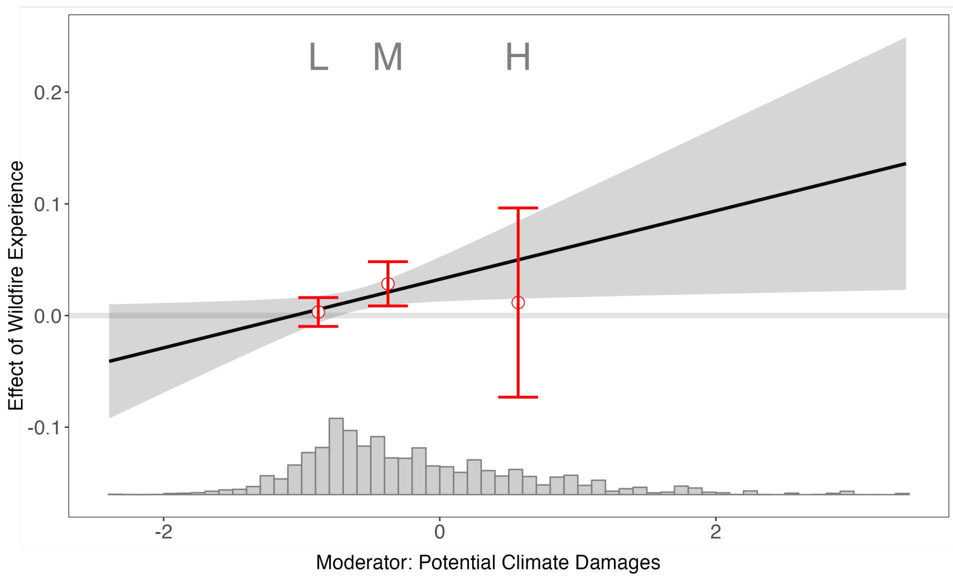
B.5 Continuous Moderator

Table B3: Linear Regression of Climate Policy Support on Wildfire Experience and Placebos, Continuous Climate Damages Moderator

	ATT on Climate Policy Support				Placebo
	(1)	(2)	(3)	(4)	(5)
Fire × Potential Damages (continuous)		0.024**	0.024**		
		(0.010)	(0.010)		
Fire (=1) × Potential Damages (continuous)				0.031**	
				(0.014)	
Placebo Fire × Potential Damages (continuous)					0.001
					(0.002)
Fire	0.017***	0.031***	0.031***		
	(0.004)	(0.008)	(0.008)		
Fire (=1)				0.032***	
				(0.010)	
Placebo Fire					0.002
					(0.002)
Potential Damages (continuous)	−0.014	−0.014*	−0.037**	−0.014*	−0.014*
	(0.008)	(0.008)	(0.016)	(0.008)	(0.008)
Employed	0.006	0.006	0.006	0.006	0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Education	0.004	0.004	0.004	0.004	0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Party ID	0.007*	0.007*	0.007*	0.007*	0.007*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Ideology	0.004	0.004	0.004	0.004	0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Household Income	0.001	0.000	0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Religion Importance	−0.005	−0.005	−0.005	−0.005	−0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes
Panel Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	No	No
N	28 362	28 362	28 362	28 362	28 362
Adjusted R^2	0.841	0.841	0.841	0.841	0.841

Notes: The outcome mean is 0.54 averaged across the treatment and control groups. Heteroskedasticity robust standard errors clustered by county. Education runs from no high school (1) to post-grad (6). Party ID runs from strong Republican (1) to strong Democrat (7). Ideology runs from conservative (1) to liberal (5). Religion runs from not at all important (1) to very important (4). $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

Figure B3: Binning Estimator for Moderating Relationship of Potential Damages on the Effect of Wildfire Experience



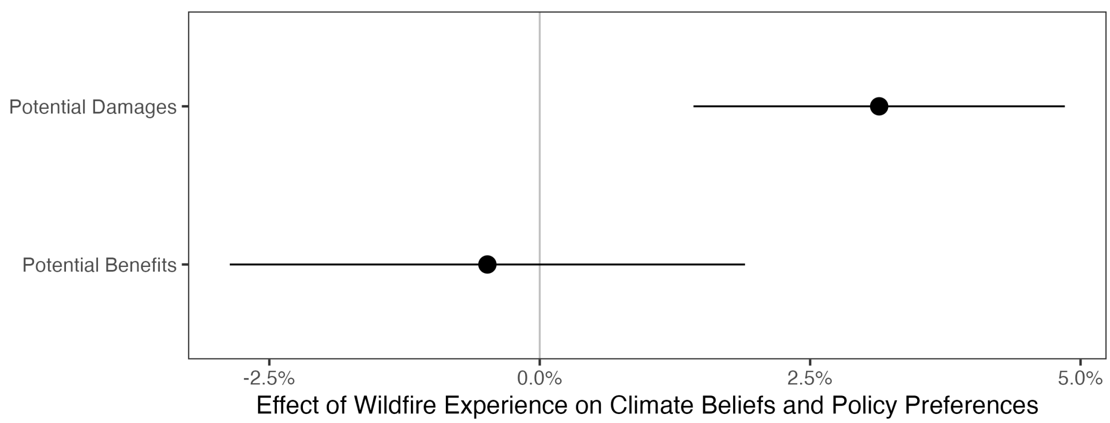
Notes: Binning estimator as recommended by Hainmueller, Mummolo, and Xu (2019) for the model with a continuous damage moderator (Table B3). Estimates from the unmatched sample with covariates for employment, education, partisan identification, ideology, household income, and religiosity. Standard errors clustered by respondent. The red error bars are 95% confidence intervals around the point estimates from the tercile bins of the moderator. There is no effect of wildfire experience at the lowest level of the damage moderator and a positive effect in the middle tercile of damage moderator values. At the highest tercile of damages, the effect is imprecisely estimated.

B.6 Panel Matching Estimator

We employ the panel matching estimator designed by Imai and Kim (2021). Since we are interested in the effect of wildfires, conditional on whether a location faces potential damages or benefits, we estimate models of the effect of forest fires on climate beliefs and preferences in places facing damages and benefits, respectively. We employ covariate balance propensity score matching using the following individual-level covariates: employment, education (scale), party identification (7-point scale), ideology (5-point scale), household income, religious importance (4-point scale), birth year, and gender. Unfortunately, the technique only allows for continuous covariates, so we could not match using race. However, that should not bias results because the covariate would drop out due to the individual fixed effects. Figure B5 shows that this matching procedure improves covariate balance.

Figure B4 presents the results of estimating the effect of wildfires in the matched sample of individuals in places facing damages and benefits, respectively. Consistent with the main results, wildfires lead to increased concern about climate change and support for action, but this effect only occurs for respondents in counties facing potential damages. Wildfires have no effect on climate beliefs and preferences in places facing potential benefits.

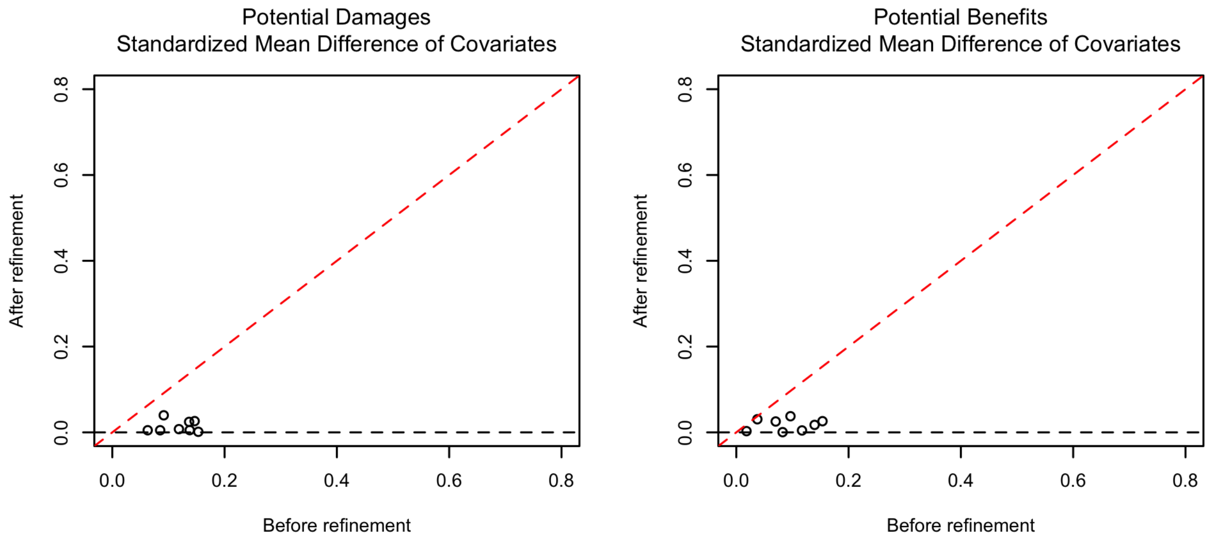
Figure B4: ATT of Wildfire Experience on Climate Beliefs and Policy Preferences, Panel Matching Estimator



Notes: Data are subsetted according to whether individuals are in a county that faces potential damages and benefits, then the treatment effects are estimated using each subset. This procedure has the advantage of permitting each covariate used in matching to have heterogeneous effects based on the level of expected climate damages. Covariate balancing propensity scores used for matching (Imai and Kim 2021).

B.6.1 Improvement in Covariate Balance

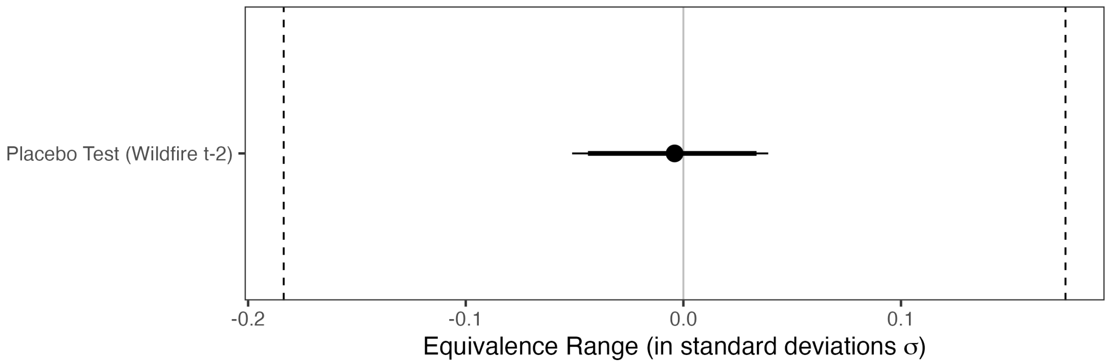
Figure B5: Improvement in Covariate Balance from Panel Matching



Notes: The scatter plot compares the absolute value of standardized mean difference for each covariate before (horizontal axis) and after (vertical axis) the refinement of matched sets. The plot shows no considerable imbalance prior to matching, and the matching procedure successfully reduces any remaining covariate imbalances.

B.6.2 Placebo Test for Parallel Trends

Figure B6: Placebo Test for Violations of Parallel Trends



Notes: Equivalence range selected by using 0.36σ, where σ is the standard deviation of the climate beliefs and policy preferences outcome, which corresponds with the range recommended by Hartman and Hidalgo (2018) for when researchers do not have strong prior expectations as to the effect size. The estimates for the placebo test come from $1,000$ bootstrap samples, with the bars denoting 95 and 90% confidence intervals constructed by taking the corresponding quantiles from the bootstrapped replicates. With both the conventional null hypothesis and the equivalence hypothesis, the is no violation of the parallel trends assumption.

B.7 Motivated Reasoning Regression Results

Table B4: Linear Regression of Climate Policy Support on Wildfire Experience Conditional on Prior Beliefs

	Skeptic	Undecided	Believer
Fire × Potential Damages	0.022	0.047**	−0.004
	(0.025)	(0.021)	(0.012)
Fire	0.006	0.007	0.008
	(0.007)	(0.010)	(0.007)
Potential Damages	−0.006	−0.051	−0.002
	(0.014)	(0.041)	(0.009)
Employed	0.006	0.007	0.005
	(0.011)	(0.018)	(0.006)
Education	0.005	0.007	−0.003
	(0.009)	(0.011)	(0.005)
Party ID	0.005	0.009	0.005
	(0.005)	(0.008)	(0.004)
Ideology	−0.006	0.009	0.003
	(0.006)	(0.011)	(0.005)
Household Income	0.003	0.002	−0.002
	(0.002)	(0.003)	(0.001)
Religion Importance	−0.011	−0.012	0.001
	(0.007)	(0.010)	(0.003)
Individual Fixed Effects	Yes	Yes	Yes
Panel Wave Fixed Effects	Yes	Yes	Yes
N	8051	11 691	8578
Adjusted R^2	0.224	0.690	0.185

Notes: The outcome mean is 0.54 averaged across the treatment and control groups. Heteroskedasticity robust standard errors clustered by county. Education runs from no high school (1) to post-grad (6). Party ID runs from strong Republican (1) to strong Democrat (7). Ideology runs from conservative (1) to liberal (5). Religion runs from not at all important (1) to very important (4). $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

B.8 Correlates of Skeptics, Undecideds, and Believers

Table B5: Correlates of Climate Skeptics, Undecideds, and Believers

	Skeptic	Undecided	Believer	Fire	Damage	Employed	Edu	Party ID	Ideology	Income	Religion
Skeptic	1	.	.	.	.	.	.	.	.	.	.
Undecided	−0.53	1	.	.	.	.	.	.	.	.	.
Believer	−0.41	−0.55	1	.	.	.	.	.	.	.	.
Fire	0.00	−0.01	0.02	1	.	.	.	.	.	.	.
Damage	0.02	−0.01	−0.01	−0.08	1	.	.	.	.	.	.
Employed	0.01	0.00	0.00	0.00	0.03	1	.	.	.	.	.
Edu	−0.09	−0.07	0.16	0.02	0.03	0.17	1	.	.	.	.
Party ID	−0.54	−0.01	0.54	0.01	−0.01	0.01	0.12	1	.	.	.
Ideology	−0.55	−0.02	0.57	0.02	−0.02	0.02	0.17	0.76	1	.	.
Income	0.02	−0.02	0.00	0.02	0.04	0.30	0.34	−0.05	−0.01	1	.
Religion	0.24	0.06	−0.30	−0.04	0.06	−0.04	−0.10	−0.33	−0.43	−0.08	1

Notes: Damage is an indicator for potential climate damages. Fire is an indicator of if a county had a wildfire. Edu stands for education.

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