Climate-Related Natural Disasters and Voting Behavior

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DOI:
10.1086/722540

Climate Related Natural Disasters and Voting Behaviour: Evidence from Environmental Legislation in the US Senate

August 27, 2022

Abstract

This paper investigates whether United States senators are more likely to vote in favour of environmentally friendly legislation following damages caused by climate related natural disasters. We combine senatorial scores of roll call votes on environmental legislation with modelled state level human and economic natural disaster losses over a 44 year period. Our results show that support for environmental legislation increases in response to unusual human losses but does not respond to unusual economic losses. We also find that the documented response to natural disasters is two years and relatively short-lived. Geography, constituent partisanship, local economic conditions, and senatorial experience affect the magnitude and precision of the treatment effect.

Keywords: US, natural disasters, environmental legislation, politicians, voting.

"Disasters are very political events."

A testimony to senators by James L. Witt, the former director of FEMA
(April 1996)

1 Introduction

Legislation is a key instrument used by governments to tackle environmental issues such as combating climate change, reducing pollution, conserving natural resources, and preventing species and habitat loss. Under democratic systems, citizens theoretically have a voice in the making of environmental laws through the voting behavior of elected officials. In reality, because few voters consider secondary policy issues such as the environment when exercising their electoral power in elections (List and Sturm, 2006; Pacca et al., 2021; Bouton et al., 2021), electoral accountability has been a perpetual cause for concern in US politics. It remains an open question as to whether elected legislators genuinely represent the views of their constituents when shaping and voting on environmental laws that have potentially important implications for the quality of life, economic sustainability and ultimately the environmental health of the planet.

The purpose of this paper is to investigate whether unanticipated, non-partisan, salient events such as extreme losses caused by climate-related natural disasters influences the voting behaviour of elected politicians when it comes to environmental legislation. To do so, we construct a data set that links the 'environmentally friendly' voting behaviour of US senators and unusual damages from climate related disasters in their local constituencies. More specifically, we measure environmental senator voting patterns using scores constructed by the League of Conservation Voters (2018) from records of selected roll call votes at the Senate from 1971 to 2014. The climate related disaster induced costs come from a record of monetary and non-monetary losses due to natural events that are included in the SHELDUS database (Hazards & Vulnerability Research Institute, 2015). The cost estimates are then used to construct localised measures of unusual losses using extreme value theory (EVT).

Our paper contributes to the existing literature on the behavior of legislators with a focus on congressional voting patterns. In the US context, although it has been shown that ideology plays an important and potentially decisive role in the voting behaviour of congressmen (Poole and Rosenthal, 1985; Arnold, 1990; Lee et al., 2004; Ringquist and Dasse, 2004; Clinton, 2006; Poole and Rosenthal, 2007), a number of studies show, to some extent at least, that legislators still take the preferences of their constituencies into account when casting their votes on environmental related legislation issues despite the perception that the environment and climate change remain a 'secondary' issue (Anderson and Mizak, 2006; Tanger et al., 2011; Chupp, 2011; Canes-Wrone et al., 2011; Miler, 2016; Vandeweerdt et al., 2016; Cherniwchan and Najjar, 2021). We focus on senators for two reasons. First, because our natural disaster measures are aggregated to the state level they better fit the constituency borders of senators rather than representatives (who are elected by border-changing districts). Second, senators serve longer terms which means a fixed effects design is more appropriate if we wish to distinguish between voting due to constituent interests from legislative ideology.

The study closest to our own is Herrnstadt and Muehlegger (2014) who suggest that

US congressional members vote are more likely to vote for environmentally friendly legislation when their home state experiences unusual weather patterns. We extend this analysis by looking at climate related natural disasters and senator voting behavior. We argue that natural disasters, which are unanticipated and can have a devastating impact on local communities, may serve as 'focusing' events. In such cases, an event can grab the public's attention, result in extensive media coverage, and trigger public debate, thus possibly changing the balance of competing advocacy groups, and may in turn alter the attitude and actions of elected legislators (Kahn, 2007; Birkland, 2016).[1]

Our study also contributes to the literature on the political consequences of natural disasters. The majority of the existing literature on the US examines the executive branch and how voters punish or reward federal and local governments after a natural disaster has taken place (Abney and Hill, 1966; Achen and Bartels, 2004; Malhotra and Kuo, 2008; Healy and Malhotra, 2009; Gasper and Reeves, 2011; Bechtel and Hainmueller, 2011). In terms of the legislative branch, Birkland (1996, 2016) illustrates how natural disasters can drive changes in legislation regarding disaster relief and disaster prevention. However, political pressure may evolves beyond the disaster domain when these events occur. For example, Liao and Ruiz Junco (2022) show that incumbent legislators with anti-environmental attitudes are punished in elections following a natural disaster. However, to the best of our knowledge, our study is the first to examine the impact of natural disasters on voting behaviours within the broader domain of environmental legislation.

[1] Boscarino (2009) explains this phenomenon as 'problem surfing', where advocacy groups attach any seemingly relevant problem that arises in society to the preferred solution they are fighting for. For example, Boscarino (2009) finds that two sustainable forestry advocacy groups, Wilderness Society and Sierra Club, take advantage of different salient events covered by the media (wildlife, water quality, recreation, economic inefficiency, and climate change) to promote their campaigns.

In terms of methodology, our key contribution is to propose novel indicators of salient events derived from state-level natural disaster damages. One of the main empirical challenges is to identify events that are important, i.e., rare enough to potentially trigger changes in attitude, and to take account of the fact that the perception of 'rare' is likely to differ across states as a result of their historical exposure to natural disasters. The approach we take in this paper is to homogenise the rarity of economic and human losses across time and states. The key identifying assumption is that if the yearly accumulated damage from natural disasters in a constituency exceeds an arbitrary degree of rarity, it is assumed that the damages become a matter of public concern and in turn this influences the behaviour of elected senators. Otherwise, the accumulated damages are considered non-salient and are likely to be ignored. The threshold we use in this study is a 10-year return level, although in robustness checks we also consider whether alternative thresholds may be more appropriate.

A further empirical challenge to overcome is the question of external validity. More specifically, the majority of the existing literature has tended to focus on one, or a small number, of salient events, such as a 500-year flood. It is therefore difficult to generalise from, or directly compare, these findings to other contexts where the 'rarity' of the event may be very different. In addition, restricting the analysis to specific types of disasters (e.g. hurricanes) fails to take account of the fact that many disasters, particularly climatic events, are driven by similar forces, for example the El Niño–Southern Oscillation (Goddard and Dilley, 2005). Thus, it can be argued that it is the cumulative losses due to natural disasters that matter in terms of changing environmental attitudes and not any single event (however large).

To model the possibility of a cumulative effect we use the monetary and non-monetary losses from the SHELDUS database (Hazards & Vulnerability Research Institute, 2015) and identify the pertinent climate related natural disasters using extreme value theory (EVT). EVT is ideally suited to the task at hand as it allows one to take account of 'fat tail' distributions that are typical of losses due to natural events, where there are many smaller, negligible, and a few potentially very damaging events (the 'fat tails'). To isolate and classify the probabilities of the latter we employ a peak over threshold (POT) model for each state, thus allowing the associated derived probabilities of events to differ across space.

Our final methodological contribution is to highlight the potential differences in political impact of human damages versus economic damages. In general, the perception that there is a close relationship between natural disasters and the environment means that issues related to the environment may emerge as a critical concern for public opinion in those constituencies recently impacted. A common argument is that environmental degradation, such as changes to the underlying climate, makes natural hazards more damaging and/or frequent and hence increases the vulnerability of affected communities (UNEP, 2007; Gupta and Nair, 2012; Botzen and Van Den Bergh, 2012; Estrada et al., 2015). At the same time, natural disasters can directly, or indirectly result, result in further deterioration of the environment, for example through ecosystem destruction, oil spills, or contaminant mobilisation (Labadie, 2006; Atkin, 2017; Dart et al., 2018; Natter and Calkins, 2018). Therefore, one may expect natural disasters to trigger greater demand from the public for increases in the stringency of environmental regulation under the assumption that stricter regulations will reduce the probability of future damaging events and/or the magnitude of the damages caused by any future disaster.

There is however a competing argument that there is a positive association between economic prosperity and public concern for the environment and that economic losses as a result of a natural disaster may lead to a reduction of concern for the environment with economic recovery given a greater priority (Elliott et al., 1997; Kahn and Kotchen, 2011; Scruggs and Benegal, 2012; Shum, 2012). If true, constituencies may prefer policies that prioritise short-term economic recovery and redevelopment to policies that focus on the reduction of future but uncertain environmental related damages. Empirically, one may therefore expect economically costly disasters to reduce the likelihood of voting for environmentally friendly legislation.

A final consideration is how politicians react to focusing events such as natural disasters. A typical approach is to anecdotally link significant legislation with salient events at the national level (Birkland, 2016). For example, Hurricane Agnes that struck in 1972 was thought to be instrumental to the passing of the Flood Disaster Protection Act (1973) and the Disaster Relief Amendments (1974). Similarly, Hurricane Fran that hit in 1996 has been linked to the Disaster Mitigation Act (2000) while Hurricane Katrina that landed in 2005 triggered the Post-Katrina Emergency Management Reform Act (2006). In the broader domain of environmental legislation Kuran and Sunstein (1998) provide a detailed narrative on how the Love Canal crisis in 1978 caused a "media snowball" that lead to the Superfund legislation of 1980.

In this study we document changes in the voting behaviour of the average senator after potential focusing events caused by unusual damages that result from a natural disaster in their constituency. As the examples above illustrate, there is likely to be a delay between the focusing event and the passing of new legislation. There are a number of explanations. First, it can take months, if not years, to get reliable estimates of the state-level losses the publication of which could alter the opinions of both the public and the elites. Second, the response of senators needs to be 'catalysed' by their interaction with external factors such as the changing power of lobbying groups or pressure from the media and the families of victims. Finally, legislation is a lengthy process where any individual bill can take a long time to pass from introduction to a roll call vote in the Senate. Hence, a senator's decision could take place immediately after a natural disaster but this change in voting behaviour would only show up at the time of the vote in the Senate.

To briefly summarise our results, we find that the environmental voting pattern of a senator is responsive to extreme losses from climate related natural disasters in their constituency. However, this response is not immediate. The overall results show a significant signal that occurs on average two years after an event and that this signal lasts only one year. However, the signal is one year for senators in those states that have had relatively little exposure to disasters in the past where constituents are likely to be more sensitive to events they have not previously experienced. The positive voting effect appears to be driven solely by an unusual number of injuries and fatalities while senators' attitude toward the environment does not appear to respond to extreme economic damages. Geography, constituent partisanship, local economic conditions and senatorial experience are all found to affect the magnitude and precision of the treatment effect.

The remainder of the paper is organised as follows. Section 2 describes our data and the construction of our variables. Section 3 presents our regression specification, the results of which are shown and discussed in Section 4. In Section 5 we conduct a counter-factual analysis. Section 6 explores heterogeneity issues with further discussion provided in section 7. Section 8 concludes.

2 Data

2.1 Senator Environmental Voting Scores

Our indicator of the environmental voting behavior of senators is derived from the National Environmental Scorecard constructed by the League of Conservation Voters (2018) (LCV). More precisely, to generate a LCV score for a senator, every year since the first Earth Day in 1970, LCV consults experts from over 20 organisations that have a reputation for supporting environmental conservation to select a list of key votes on environment-related issues. Accordingly, each vote is scored 1 (pro-environment) if in line with LCV's position, or 0 (anti-environment) otherwise, including absentee votes. The annual score for each legislator, except for those who were ill or died, and the Speaker of the House, whose vote is discretionary, is computed as the average score on votes on all selected issues within a year and then transformed into a scale from 0 (environment enemies) to 100 (environment heroes). To construct scores, in some years a number of important issues are counted twice while the sponsorship of certain bills or petitions may be selected as replacements for real votes.

Finally, for each piece of environmental related legislation the LCV examines the content of the bill and categorises the content into different environmental related topics. We take these topics and aggregate them into eight broad categories that we call: (1) Air, (2) Clean energy, (3) Dirty energy/Toxins/Public right to know issues, (4) Water, oceans and drilling, (5) Lands/Forest/Wildlife, (6) Transportation, (7) Climate change, and (8) Other. To control for changes in the topics that are included in legislative bills over time we construct a variable that captures the share of each of these eight broad categories for each year.[2]

2.2 Senatorial Characteristics

To control for senator characteristics we use information from Voteview (Lewis et al., 2017) and the Congress Legislator project on GitHub.[3] The Voteview dataset provides information about the birth date, party affiliation, and ideology.[4] As an ideology control we include the two dimensions of NOMINATE, which are calculated from DW-NOMINATE (Dynamic Weighted NOMINATE Three-step Estimation) (Poole, 2005; Poole and Rosenthal, 2007). The first dimension captures ideology in terms of economic/re-distributive issues, while the second accounts for social/racial issues. For both dimensions, which are normalised between -1 and 1, a higher estimate means a more conservative politician. The estimates are changed for those that switched party. The GitHub data set also provides information on the gender and cohort of the senators (to determine the election year) and the history of their appointment to the

[2]Note that the sum of these shares could exceed 100 percent as a piece of legislation may span multiple topics and hence be included twice. Note that these share variables are not absorbed by year fixed effects as they can vary over legislators each year if some senators, for certain reasons (such as midterm elections, illness or death), cast fewer votes on those selected issues.

[3]The Voteview data set collates information on US congressmen, president and vice presidents from 1789 to the present day, using various information sources. See https://github.com/unitedstates/congress-legislators for details.

[4]The party affiliation of legislators in the LCV dataset is corrected for those senators who switched party or changed jobs.

Senate.[5]

2.3 Natural Disasters

To measure the impact of natural disasters we rely on the SHELDUS™ database (Hazards & Vulnerability Research Institute, 2015). The database tracks the damages from natural hazards which are classified as belonging to one of eighteen different types of disaster.[6] As the focus of this paper is on climate-related disasters, we exclude avalanches, tsunamis/seiches, volcanoes, and earthquakes.[7] We have access to data at the county level for the period 1960 - 2014.

Annual losses are given in terms of human losses (injuries and fatalities) and monetary losses (crops and property). Note that the latter is often estimated based on insured losses and not all crops or property will be insured. Since senators are elected by their states as a whole, we aggregate losses to the state level. We compute human losses at the state level as the sum of annual injuries and fatalities and calculate economic losses as the aggregate damage to crops and properties in a given year. Economic losses are in 2014 prices. In addition, we consider the intensity of losses (per capita variables) rather than their levels to take into account the changes in population and the density of economic activity.

Figure 1 summarises the cumulative damages by climate-related disaster type in

[5]The tenure of senators is six years. Every two (even numbered) years, one-third of senators (representing 33-34 seats) are re-elected and the cohort helps determine the year of election. For example, those of cohort 1, 2, and 3 are up for re-election in 2018, 2020 and 2022, respectively.

[6]For completeness the eighteen types of natural hazards included in SHELDUS™ are: avalanches, coastal floods, droughts, earthquakes, floods, fog, hail, heat, hurricanes/tropical storms, landslides, lightning, severe thunderstorms, tornadoes, tsunamis/seiches, volcanoes, wildfires, high winds, and winter weather.

[7]The results are, however, qualitatively similar if we include these additional disasters. For example, only Hawaii includes volcano related damages.

the US between 1960-2014. The disasters that caused the most injuries and fatalities in descending order are tornadoes, hurricanes/tropical storms, lightning, winter weather, and high winds. Meanwhile, disasters with the largest damages in monetary terms are hurricanes/tropical storms, floods, tornadoes, droughts, and winter weather. Given the diversity of the terrain in the US, damages are unevenly distributed. For example, tornadoes tend to be concentrated in the central part of the country (Tornado Alley), wildfires are common in the West, and hurricanes typically strike the East Coast and the Gulf of Mexico. Hazards such as floods, lightning, and droughts tend to be more evenly geographically distributed.

[Figure 1 about here]

To identify the salient, i.e., relatively rare, human and economic loss years in a state we use extreme values theory (EVT) and estimate a Peak Over a Threshold model (POT) (Coles, 2001). The approach can be summarised as follows. If, for each of the 50 states, for each of the two series of annual losses, we assume that they are a sequence of independently and identically random variables, then an appropriate normalisation of the maximum of such a sequence, according to the Extremal Types Theorem (Fisher and Tippett, 1928), must asymptotically approximate one of three extreme value distribution families, namely Fréche, Gumbel, or Weilbull, which can be generalized by a Generalised Extreme Value (GEV) distribution (Jenkinson, 1955). Under certain asymptotic arguments, the probabilities of the extremes of these sequences being above a certain threshold can be modelled via a generalised Pareto distribution (GPD); see (Pickands, 1975).

For the practical implementation of peak over threshold models, the choice of the threshold is important. A high threshold limits the number of observations, making the estimation less precise, while a low threshold undermines the underlying asymptotic assumptions, posing a potential risk of bias in the estimation. We follow the guidelines presented in Coles (2001) and use a series of visualisation tools to determine the threshold for each series. First, we draw the mean residual life plot (MRL), which captures the relationship between the average exceedances and thresholds, and select a low threshold where such a relationship still plausibly mirrors a straight line, as one would expect from the theory. The selection is verified by a POT parameter stability plot.[8] After the threshold is chosen, the GPD is estimated by a Maximum Likelihood Estimator (MLE). Post-estimation graphs are used to further assess the selection of the threshold. Appendix A provides a detailed explanation of our EVT and POT approach and an explanation of the POT parameter stability plot approach.

The estimated GPD distribution is used to determine the return level $z(N)$ of the extreme loss intensity values corresponding to a predefined return period of N years. For our baseline results we choose a return period of 10 years, which corresponds to a return level $z(10)$ such that the annual natural disaster damages/losses in a given state in any year can exceed such a level with a probability of $1/10 = 0.1$.

2.4 Other State Level Controls

In our estimations we also control for a number of state characteristics including demographic, economic, and weather related, that could be correlated with voting behavior and the possibility that natural events translate into large losses. In terms of demo-

[8] As suggested by the theory, once the threshold is considered appropriate, relevant (transformed) parameters must be stable when there is an increase in the value of the threshold.

graphic data we take information from 'The US Population Data by National Cancer Institute', which provides an estimate of the annual population by age, sex, and race at the county level.[9] To generate controls for the macro-economic environment (GDP, personal income, and implicit price deflator) at the state level, we use data from Bureau of Regional Economic Analysis (BEA).[10]

As noted by Auffhammer et al. (2013), when modelling climate related phenomena it is important to also control for general weather patterns that could be correlated. To construct a state level measure of annual temperature and precipitation we use data from the nClimDiv database (NOAA, 2014; Vose et al., 2014) that interpolates climatic data from stations at a 5km $\times$ 5km resolution using an area weighting method and taking into account topographic and network variability. The data covers states in the contiguous United States (CONUS) and Alaska (added in 2015, based on 1971-2000 PRISM averages) and excludes Hawaii. For Hawaii, we use the Global Summary of the Year data provided by National Climatic Data Center (NCDC).[11]

2.5 Summary Statistics

Table 1 summarises the data used in our regressions. After the data matching processes, we obtain 4,414 observations from 381 senators for which we were able to obtain the full set of senatorial environmental voting scores, natural disaster measures, senatorial characteristics, and other state-level controls. The average age of a Senator is 58.6. The oldest senator in the dataset is Sen Strom Thurmond (1902), who was

[9]The data is accessible at https://seer.cancer.gov/popdata/methods.html.

[10]Data is available at https://www.bea.gov/regional/downloadzip.cfm.

[11]More specifically, we compute temperature and precipitation for Hawaii as the average of the data from three stations located at Hilo International Airport, Honolulu observatory, and the Lihue weather service office airport.

100 years old when serving the Senate in 2002 before his death in 2003. The female representation is 7.3% over the entire period although it has risen dramatically since 1992 and reached nearly 20% in 2014.

[Table 1 about here]

Regarding our variables of interest (the modelled natural disaster measures), we present the spatial distribution of human and economic losses in Figure 2. As can be seen, the 10-year return levels for economic loss intensity in 2014 prices ranges from 16.5 (Connecticut) to 510.5 (Iowa) dollars per head, while the 10-years return levels for human loss intensity ranges from 4.7 (Rhode Island) to 144.5 (Mississippi) deaths and injuries per million of the population. Spatially, the South and the Midwest, have the highest 10-year return levels of extreme climate induced damages, both in terms of economic and, human losses. Figure 3 shows the temporal variation of human and economic losses in conjunction with the number of states affected. As can be seen, there is considerable variation in both the incidence and the intensity during the last 50 years or so.

[Figures 2, 3 about here]

3 Specification

Our baseline specification to estimate whether natural disaster shocks have an impact on environmental voting patterns of senators is given by:

$$Y_{ijt} = \alpha + \sum_{k=0}^{q} \beta_k shock_{jt-k} + \Psi' X_{it} + \Omega' Z_{jt-q-1} + \sum_{k=0}^{q} \delta_k W_{jt-k} + \mu_{i\hat{t}} + \nu_t + \varepsilon_{ijt} \qquad (1)$$

where Y_{ijt} is the average LCV score in year t of Senator-by-term i elected to state j. The binary variable $shock_{jt-k}$ indicates whether there were unusual losses due to natural disasters (per capita) in state j, k years prior to year t that exceeded the 10-year return level. One should note that we allow for potential lagged effects of these shocks by including q lags of their realizations. The set of coefficients $\{\beta_k\}$ are the main parameters of interest.

The vector X_{it} contains our set of variables that control for characteristics of senators including ideology (measured by 2-dimensions of the NOMINATE estimates), party affiliation, age, tenure (whether they are in their first year in the chamber and whether they are in an election year) and a range of policy issues (share of issues by topic) voted in year t. Z_{jt-q-1} is a vector of state conditions, including demography (share of voters by ethnicity and age), economy (average personal income, and real growth rate). Note that to avoid bad controls that might be impacted by the extreme climate shocks and hence might absorb some of their impact on voting, we lag these variable at time $t-q-1$, i.e., one year just before the earliest modelled lag of disaster losses $(t-q)$. With W_{jt-k} we also control for general state-level weather variables (precipitation, and temperature) and their lags (similar to disaster losses). Time fixed effects, ν_t, capture common national factors that vary across years.

Finally, we include senator-by-term fixed effects, $\mu_{i\hat{t}}$, that vary across senators (i) and their terms $(\hat{t})$ and control not only for time invariant unobserved senator heterogeneity, but also allows these unobserved factors to vary with senators' political terms. Senator fixed effects alone may not be adequate because, first, some senators hold office for decades and second, senator turnover is endogenous. For example, if a senator in a conservative area decides to vote for an environmental issue they may then be elected out of office in the following election cycle, which would make the new senator less likely to vote for environmental issues. As such senator-by-term fixed effects also absorb any state specific time invariant factors. Arguably, after controlling for these fixed effects, any differences in statewide extreme climate event exposure should only capture random, unanticipated realisations of their local distributions.[12]

Of course whether an climate event translates into a natural disaster depends also on time varying ex-ante population exposure, disaster mitigation policies, and local infrastructure, all of which could feasibly also be correlated with voting patterns. We assume that these concerns are controlled for through our rich set of time varying state specific controls. Standard errors are clustered at the state level in all specifications to capture shock correlations across senators through the extreme climate events.

4 Results

4.1 Baseline regression

Table 2 shows the results from our baseline specification in Equation (1) using human losses as the proxy for *shock*. In Columns (1)-(4), we show the results of incrementally increasing q from 0 to 3, i.e., allowing for an increasing lagged impact of the extreme climate losses. As can be seen, while there is no contemporaneous or $t-1$ impact of extreme climate damages on voting behaviour, two years after the event senatorial voting for environmentally friendly legislation increases. This effect disappears, how-

[12]We thank an anonymous referee for the suggestion to include senator-term fixed effects.

ever, by $t-3$.[13] Thus, the impact is delayed and is short-lived. One should note that the impact of unusual human losses at $t-2$ is significant at the 1% level if we set $q = 2$ (Columns 3 and 7) and at the 5% level if we set $q = 3$ (Columns 4 and 8). The estimates on the two year lag are slightly larger and more accurate if we restrict the time horizon to $q = 2$. The smaller standard errors suggest the redundancy of lags beyond $t-2$.

[Table 2 about here]

In Columns (5) through (8) we consider monetary losses as the *shock* proxy, similarly incrementally allowing for increasing lagged impacts. In contrast, to human losses there is no significant impact of monetary damages on senatorial voting. One may want to note in this regard that, while the standard errors are similar, the coefficients on the lagged coefficients tend to be substantially smaller.

We also explore whether the $t-2$ effect of human extreme climate related losses is robust to simultaneously including monetary losses rather than using the latter as an alternative proxy. The results are shown in the final four columns of Table 2. The results do not change qualitatively and only marginally increase the estimated coefficient on the significant human loss term at $t-2$.[14] Hence, for the remainder of the analysis we simply use human losses as our proxy of extreme climate natural disaster events.

Using Column (3) as our preferred benchmark regression, the estimated coefficient

[13] Compared with the full sample (N=4,414), we lose 100 and 200 observations when extend the time horizon to $q = 2$ (Column 3) and $q = 3$ (Column 4), respectively due to the unavailability of (lagged) control variables in earlier years. The significance of $t-2$ and the insignificance of earlier shocks hold when we move between Columns (3) and (4) despite the difference in sample size.

[14] A z-test suggests that the coefficients in either Columns (3) or (4) are not significantly different from their counterparts in Columns (11) and (12), respectively.

on $t-2$ suggests that unusual damages to humans by climate-related disasters in a particular state adds 2.24 points to a senatorial environment score two years after the event. To put this in context, the average senatorial environmental score is 47.20 (N=4,314) for the corresponding sample, 69.25 for Democratic senators (N=2,238) and 23.42 for non-Democratic senators (N=2,076).

4.2 Robustness Checks

To ensure our results are not sensitive to changes in specification we conduct a number of robustness checks. First, in Table 3, we compare the findings of our preferred benchmark specification of Column (3) in Table 2 (for convenience we repeat these results in Column 1) to a range of alternative specifications (Columns 2 to 10). More specifically, to address any concern that ideology and party affiliations could be bad controls if some senators switch parties or change their ideology as a consequence of previous natural disasters, we remove these controls in Column 2. Reassuringly the estimated coefficients appear almost unchanged and the impact at $t-2$ of *shock* remains significant at the 1% level.

[Table 3 about here]

Another concern might be that once we control for senator-by-term fixed effects our proxy for ideology varies very little (since it can only change due to a switch of party or a change to another political position), so may be capturing ideological changes concurrent with extreme losses only by chance. To further investigate we replace the NOMINATE measures by a more time-varying ideology proxy, namely Liberal Quotient (LQ) by American for Democratic Action (ADA). The LQ is a 'standard measure of political liberalism' (Americans for Democratic, 2017) and is constructed for each senator from the records of 20 selected votes each, which in addition to environmental concerns, cover a wide range of issues, including domestic and international social and economic issues. As for the other time varying controls, we lag the LQ alternative proxy for ideology at $t-3$ to avoid it potentially acting as a bad regressor. Unfortunately, this entails a reduction of our sample to 3,318 because the LQ variable does not have the same level of coverage. Results including this alternative ideology proxy are presented in Column (3) of Table 3. While the coefficient of human loss shocks at $t-2$ is reduced to 1.76, possibly due to sample variation, it nevertheless remains significant at the 5% level.

In Column (4) we exclude (lagged) controls for economic conditions from the benchmark regression, while Column (5) drops the the contemporaneous and $t-1$ values of *shock*. Our results are not sensitive to these modifications in that the changes in coefficient of the shocks at $t-2$ and their significance are negligible. Columns (6) and (7) modify the specification in Column (1) by adding one and two lags of the dependent variable, respectively, in order to allow for dynamics in environmental legislative voting by senators. Our findings remain significant at the 5% level.

The remainder of Table 3 explores how important it is to control for senator-by-term fixed effects in trying to capture any correlated unobservables. In this regard Column (8) replaces senator-by-term FE by simple senator FE, thus assuming that term-invariant characteristics of senators throughout their political career at the Senate are sufficient to account for unobserved confounders.[15] Column (9) uses state FE

[15]As senators do not change their constituencies, senator FE also accounts for time-invariant unobserved characteristics of states.

instead and only considers time-invariant unobserved confounders at the state level (besides senate-wide time-varying factors).[16] The results show that compared to the benchmark results the impact of extreme climate event losses falls somewhat in both magnitude (by about 4%) and in precision (larger standard errors). Using only state FE further reduces the coefficient, again estimated with less precision than the senator-by-term fixed effects (Column 1).

Finally, Column (10) presents the results of a specification that excludes any state or senator fixed effects FE and retain only year FE. The magnitude of impact at $t-2$ is similar to the state FE, while also losing some precision. Overall, the results are remarkably consistent to different specifications.

4.3 Granger-style causality tests

After purging the impact of fixed effects and relevant controls, we assume that the extreme climate related losses are unanticipated realisations of their state level distributions and as such our estimates can be interpreted causally. A potential counter-argument is that the dependent variable (LCV score of elected senator) may serve as a proxy of local elite attitudes and public opinion towards the environment in the associated state, which could possibly be correlated with natural hazard prevention and preparedness and predicts the actual loss caused by natural disasters later.

To shed a light on this issue, in Figure 4 we conduct a test for causality inspired by Granger (Granger, 1969) as suggested by Angrist and Pischke (2008). Hence, we

[16] Note that in this specification (and other alternative specifications where appropriate), we add gender as an additional control as this would have been absorbed by the senator-by-term fixed effects in our preferred benchmark specification.

take the the leads of the climate extreme event proxy and incrementally add them to the baseline specification in Columns (6) to (8) of Table 2. We present the estimated coefficients and show the 95% confidence intervals of the full set of coefficients on our *shock* variable. More precisely, the modified specifications in Figure 4 are labelled m1, m2, m3 with an assumed symmetric structure, where the lead lengths are set equal to the lag lengths (at 1, 2 and 3 respectively). The point estimates of the shock lags change little with the inclusion of future shocks in that the impact at $t-2$ remains significant at the 5% level in all models. Importantly, the coefficients of all future shocks are small in magnitude and insignificant. This provides evidence of a lack reverse causality, where the environmental voting process might predict climate events translating into natural disasters.

[Figure 4 about here]

4.4 Monte-Carlo permutation test

To ensure that our significant results are not due to a chance occurrence of extreme climate shocks and changes in environmental voting we also undertake a randomization (Monte Carlo permutation) test. More precisely, within each state, climate shocks are randomly assigned to a year with a probability of 10 per cent, corresponding to their 10-year return period exceedance probability, using a binomial distribution. Then the benchmark specification of Column (3) of Table 2 is re-estimated 1,000 times, where for each simulation we construct the t-statistics on our climate shock indicator and its lags. The corresponding p-values of our actual estimates relative to the distribution obtained from the simulations are provided in Figure 5. As can be seen, the test confirms the significance (at the 1% level) at time $t-2$ of a human loss shock (p-value=0.005). At the same time the values of the t-statistics for the shocks at time t and $t-1$ are not unusual given the simulated distribution, with p-values 0.623 and 0.82, respectively.

[Figure 5 about here]

4.5 Alternative definition of extreme climate events

Thus far we have defined extreme climate event losses as those that had a return period of at least 10 years. To explore how sensitive our results are to this choice we also investigate different return levels to define an extreme shock and re-estimate the benchmark regression of Column (3) of Table 2.

Figures 6 show the estimated contemporaneous and lagged coefficients on *shock*, along with their 95% confidence bands, using various return period thresholds ranging from 2 to 20 years. Accordingly, while the coefficients at t and $t-1$ are consistently insignificant no matter what return period threshold is chosen, this is no longer the case for the coefficients of the $t-2$ lags. Rather, significant (at the 95% level) effects are found only at return period thresholds between 6 years and 11 years, which correspond to the possibility of extreme events between 16.7% and 9.1%. The point estimates are all positive and gradually increase when we raise the rarity bar to the 11-year return level then fluctuates slightly. After that point the confidence intervals widen considerably, possibly because the estimates are based on fewer treated observations in the underlying POT model.

[Figure 6 about here]

4.6 Compounding of extreme climate shocks

So far our baseline specifications implicitly assume the independence of the impact of climate extreme events on voting across years. Alternatively, we may want to explore whether their effect may compound over time. To this end we construct a measure that sums the number of extreme climate events over a given t to $t-q$ period, where we allow q to range from 1 up to 4 in Columns (1) to (4) of Table 4 and shocks based on the 10-year return level (similar to the baseline specification). In Columns (5) to (8) we mimic the previous columns but instead use the lower threshold of 5-year return level events.[17] As the results in Table 4 show, allowing events to compound, no matter over how many years and what return level, renders any impact insignificant. Thus, arguably, as our benchmark specification suggested, the impact of extreme climate events is short-lived and independent over time.

[Table 4 about here]

4.7 Spillover effects

Another empirical concern is whether extreme losses in one state might affect voting behaviour of senators in nearby states (for example through flows of migrants from the affected state or economic redistribution). To explicitly test for spillover effects we construct two measures. The first is a binary variable that records a value of 1

[17] As shown in section 4.5, the impact at $t-2$ in the benchmark regression remains positive and significant if we replace the 10-year return level by the 5-year return level to define the shock.

if any neighbouring states experience extreme climate related losses. The second is a continuous variable bounded between 0 and 1 that is the average value of *shock* in all neighbouring states. We add these variables and their lags (up to $t-2$) to the benchmark regressions and the results are shown in Columns (2) and (3) of Table 5, respectively shown next to the results for our preferred specification taken from Column (3) of Table 2). As can be seen, the impact from neighbouring states is insignificant, inducing essentially no change in the estimated magnitude of the local shocks.

[Table 5 about here]

5 Counter-factual analysis

In this section we ask how the Senate might have performed if there had been no extreme climate shocks. To this end we use the preferred benchmark regression results (Column 3 of Table 2) and sum up the effect of losses as measured at $t-2$ for each senator-by-year observation. Figure 7 visualises this counterfactual analysis, where we compare the average LCV scores of senators by party affiliation (upper panel) and the average impact of shocks across all modelled senators (lower panel). In general an average Democratic senator (blue line) is more likely to vote for more environmentally friendly legislation than his Republican counterpart (red line) during this period. The gap between their LCV scores is fairly narrow initially but then widens considerably in the 1990s, reflecting the well known increased polarisation along party lines in the US environmental politics from that period onward. The average impact of extreme climate shocks on voting appear to be modest (even if compared to the average LCV score of non-Democratic senators) with a peak at 0.6 percentage points (in 1976) and an average of 0.22 percentage points.

[Figure 7 about here]

6 Heterogeneous treatment effects

6.1 Heterogeneity over time and topics

Our baseline regressions implicitly assume that legislators' responses to ND shocks are stable over time. However, it is well known that American politics, at both the general public and elite level, has become more polarised over time (Garand, 2010). This also applies to environmental politics in general and environmental legislation in particular (Kim and Urpelainen, 2017). As can be seen from the upper panel of Figure 7, the divergence in environmental voting patterns between parties seems to have emerged from early 1990s onward. Thus, in order to investigate the potential heterogeneity in treatment effects over time we split our sample by the year 1990 with results shown in Table 6. Notably, the latter period also coincides with an increasing backlash against climate scepticism in the US after the Intergovernmental Panel on Climate Change (IPCC) raised the alarm about science-backed human-made global warming (Collomb, 2014).

[Table 6 about here]

In Table 6, we show the results of the the two sub-samples split by voting year: before 1990 (Column 2) and since 1990 (Column 3) against the benchmark regression on the full sample in Column (1) (equivalent to Column (7) in Table 2). Despite changes in political polarisation and the salience of climate issues, for both periods our key finding of extreme damages due to climate-related disasters in a constituency driving a tendency to vote more environmentally friendly at the Senate two years later still holds. Qualitatively, the impact is slightly lower in the later period (a coefficient of 2.08 compared to 2.86). A possible explanation is that senators in the later period might weight ideology more heavily and hence be less responsive to non-partisan events such as natural disasters when make a decision on legislative voting.

We also construct two alternative measures for the dependent variable and explore how they respond to our treatment in the full sample as well as two sub-samples. In Columns (4) to (6) of Table 6, we split the LCV environmental votes into those that are specifically concerned with climate, and closely-linked issues including air pollution, energy, and transportation, versus the remainder.[18] In Columns (4) to (9) we present the estimates using these two different dependent variables computed for the full as well as the sub-period samples. As expected, the Climate-related Score (Columns 4-6) is responsive to natural disasters and reassuringly the Other Score (Column 7-9) is not. This may suggest that the relevance of issues matters when it comes to changing the voting behaviour of senators.

What is noteworthy is that there appears to be a shift in the response timing between two periods. Before 1990, i.e., the response of the climate related score at $t-2$ is large (4.3) and significant at the 5% level. For the later period, the signal is somewhat

[18]More specifically, the climate-related score is derived from issues that belong to at least one group from (1) Air, (2) Clean Energy, (3) Dirty Energy/Toxins/Public Right to Know issues (excluding Toxins/Public Right to Know subcategories), (6) Transportation, (7) Climate change. Unfortunately we are not able to decompose the LCV variable further since single issues like climate change are absent in many voting years. Given that this may be due to reporting and classification changes over time, one should thus view results from this more stringent score with caution.

mixed. We now find an instantaneous large increase by 4.32 points (significant at the 5% level) followed a weaker increase at $t-2$ by 2.83 points (significant at the 10% level). The earlier response could be explained by the recently increasing awareness of the climate urgency and senators reacting more quickly to natural disasters given increased media coverage and the role of social media and more effective lobbying.

6.2 The roles of geography, public partisanship, economic conditions and personal exposure

We also explore heterogeneity in treatment effects, the results of which are presented in Figure 8. To this end, we interact the (lagged) losses variables in the benchmark regression with relevant indicators to explore the roles of geography, public partisanship, economic conditions, and senator exposure to the event.

[Figure 8 about here]

In Panel (i) of Figure 8, we interact each treatment variable with four indicators of Census regions. Accordingly, the pro-environmental impact in legislation of natural disaster damages at $t-2$ seems to be driven by senators in the Midwest and the Northeast. The coefficients corresponding to these regions are 4.8 (significant at the 5% level) and 3.1 (significant at the 10% level), respectively. One should note that among climate-related disasters that cause injuries and fatalities, the Midwest is particularly vulnerable to tornadoes, while the Northeast is vulnerable to hurricanes and winter weather. The near-zero coefficient of the West could be explained by its lower exposure to climate related disasters and as such the damages are less likely to translate into political pressure. However, a similar argument does not appear to hold for senators from the South given the insignificant impact of disasters on voting behavior despite being exposed to disasters such as tornadoes and hurricanes. This may suggest the influence of other factors that coincide with a general North-South divide.

Panel (ii) examines the role of state partisanship. To this end, we construct yearly estimates of party identification of the voters following Caughey and Warshaw (2018), where, for each year we label a state as 'Blue' if its proportion of Democratic supporters is estimated to exceed Republican supporters, and otherwise the state is labelled as 'Red'. In this regard Blue states may serve as a proxy for the liberalism of a senator's constituency and as such it is correlated with electoral preference for environment protection. The point estimates of the impact at $t-2$ are very similar for either state types (around 2.2-2.3). However the corresponding standard errors of Blue states are much lower than for the Red states (0.9 compared to 1.3). As such the impact in Blue states is estimated more precisely and is significant at the 5% level. Meanwhile, the impact in Red states is significant at the 10% level.

When we look at the role of economic conditions, Panel (iii) suggests that the impact at $t-2$ is qualitatively larger for senators representing high-income states, where a state is classified as 'high income' if it belongs to the top quintile of (average) personal income in a given year. The corresponding coefficient is 3.6 (significant at the 5% level), i.e., double its low income counterpart (significant at the 10% level). This result is consistent with the previous finding that exceptional monetary damages by natural disasters does not support environmental legislation, where a preference for environment stringency is thought to be associated with better economic conditions

(Elliott et al., 1997; Kahn and Kotchen, 2011; Scruggs and Benegal, 2012; Shum, 2012)

Finally, Panel (iv) explores heterogeneous effects according to the personal experience of senators. The impact at $t-2$ is significant (at the 5% level) if the senator held office when the disasters struck (as compared to only becoming senator after the disaster). Otherwise the impact is statistically insignificant.

7 Discussion

One of our main findings is that extreme climate disasters impact senatorial voting on environmental legislation two years after the event. There are several factors that could help explain this result. Arguably, for a senator to change voting behavior requires four conditions: (1) that a senator is aware of salient damages caused by a natural disaster in their constituency and the need to respond to such an event; (2) they are convinced that environmental legislation may appropriately address such a need; (3) their interaction with the environmental legislation at the Senate provides an opportunity to address the issues; and (4) a constituency must want to change. Each of these conditions can be time-consuming for the following reasons.

First, as states are large administrative territories, it may take months and even years to obtain a reliable tally on actual losses. Indeed, it is not an easy task to verify whether a loss is attributable to a disaster or to other causes. In our study, natural disaster shocks are a combination of damages from all extreme climate events rather than a single extremely rare catastrophe. Our underlying assumption is that when designing and voting for laws, legislators are affected by the real socioeconomic consequences of a disaster rather than their physical intensity.

Second, even when a senator is keen to support legislation that aims to reduce the frequency or intensity of disasters, there are challenges to overcome that begins with an ideology that may have been developed over long political career. To this end, external social factors, such as lobbying from interest groups (Yu, 2005; Prieur and Zou, 2018; Pacca et al., 2021), and pressure from media and the families of victims, may mediate against a senator changing their historical stance on environmental legislation.

Finally, throughout the legislative processes, senator voting is a last stage of the process. In the first instance, if a senator wants to demonstrate greater support for environmental legislation they need to either propose a new legislation, amend an existing law, or change their historical voting pattern when new legislation is put to the senate. In this study we were able to only consider voting behavior. In this regard, when we trace 216 roll call votes in the Scorecard database between 1989-2014, we find that it took a prominent piece of environmental legislation up to 472 days (on average 118 days) from beginning (introduced in either chamber of the Congress) to reach a roll call vote at the Senate.[19]

Hence, our finding of a two year effect is entirely consistent with how the voting process in the US operates.

[19] We scraped the Senate website (https://www.senate.gov) to extract the action history of all legislation (bills, amendments, resolutions) related to roll call votes listed in the LCV Scorecard database. Several roll call votes may be related to a same issue if the issue was rejected before but voted again after amendment(s).

8 Conclusions

Ideally elected politicians act in their voters' best interests. The occurrence of natural disasters offers researchers a quasi-natural experiment to investigate how large events affect the way in which a senator votes. More precisely, the often large losses due to natural disasters provide a context for possibly bringing about sudden changes in politicians' support for environmental issues. While, on the one hand, they may be inclined to prioritise the immediate economic needs of their constituents after a disastrous event, casting environmental concerns aside, they may also consider supporting environmental issues more enthusiastically under the belief that legislation will reduce the impact of natural hazards and associated environmental disasters in the future.

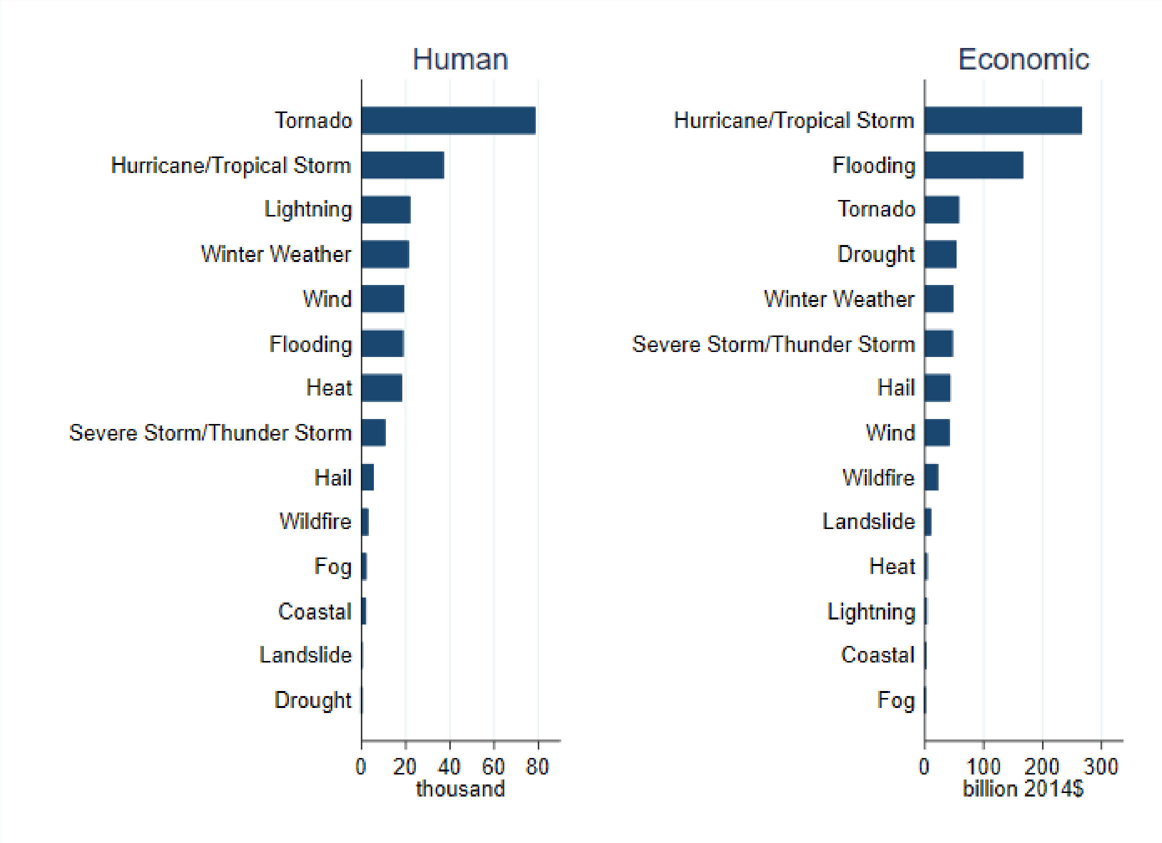
Our study, using a long panel of senatorial environmental voting behavior and state level extreme climate disaster losses, shows that senators are indeed responsive to the losses of their electorates after important natural disasters. However, their reaction in terms of voting for environmental friendly legislation is not immediate and is relatively short-lived. The observed gap of two years between the occurrences of the natural shocks and changes in the environmental friendliness of senators perhaps suggests that such changes are not solely driven by immediate self-reflection and self-motivation of senators themselves, but also through gradual interactions with external pressures, such as media coverage, public opinion, and advocacy groups. We find that the favourable conditions for pro-environmental legislation impact of natural disasters include electoral liberalism, good local economic condition and the senator holding the office when the (past) disasters struck. Geographically, the impact seems to be driven by Midwestern and Northeastern senators. In addition, the fact that any impact appears to be short-lived may reflect bounded rationality: the impact of natural disasters on senators disappears rapidly as other concerns arise.

When considering how senators react to natural disasters we find strong evidence that a senator is more likely to vote favourably on environmentally friendly legislation if there is a large number of injuries and fatalities in their constituency after a climate related natural disaster. In contrast, they do not respond to exceptional monetary losses. Possibly human losses matter more politically, but it could also be that a damaged economy constrains the preference for environmental stringency. It should also be noted that monetary losses are based mostly on insurance claims and thus may be the less accurate measure of extreme damages due to climate events.

Finally, it should be acknowledged that one weakness of our study is that in identifying the important natural disaster event years used to identify any causal effects between extreme climate and senatorial voting behavior we are assuming that the distribution of possible losses has remained stable over our 44 year sample period and that there is no temporal clustering of events. This may be an arguably unrealistic assumption and one could possibly explore this by modelling such changes explicitly, although our sample period may be too short to detect such effects. In addition, it is also interesting to test whether similar voting patterns might be observed at the House of Representatives. A difficulty that may arise in doing so, however, is that the boundaries of congressional districts have not been consistent over time due to redistricting.

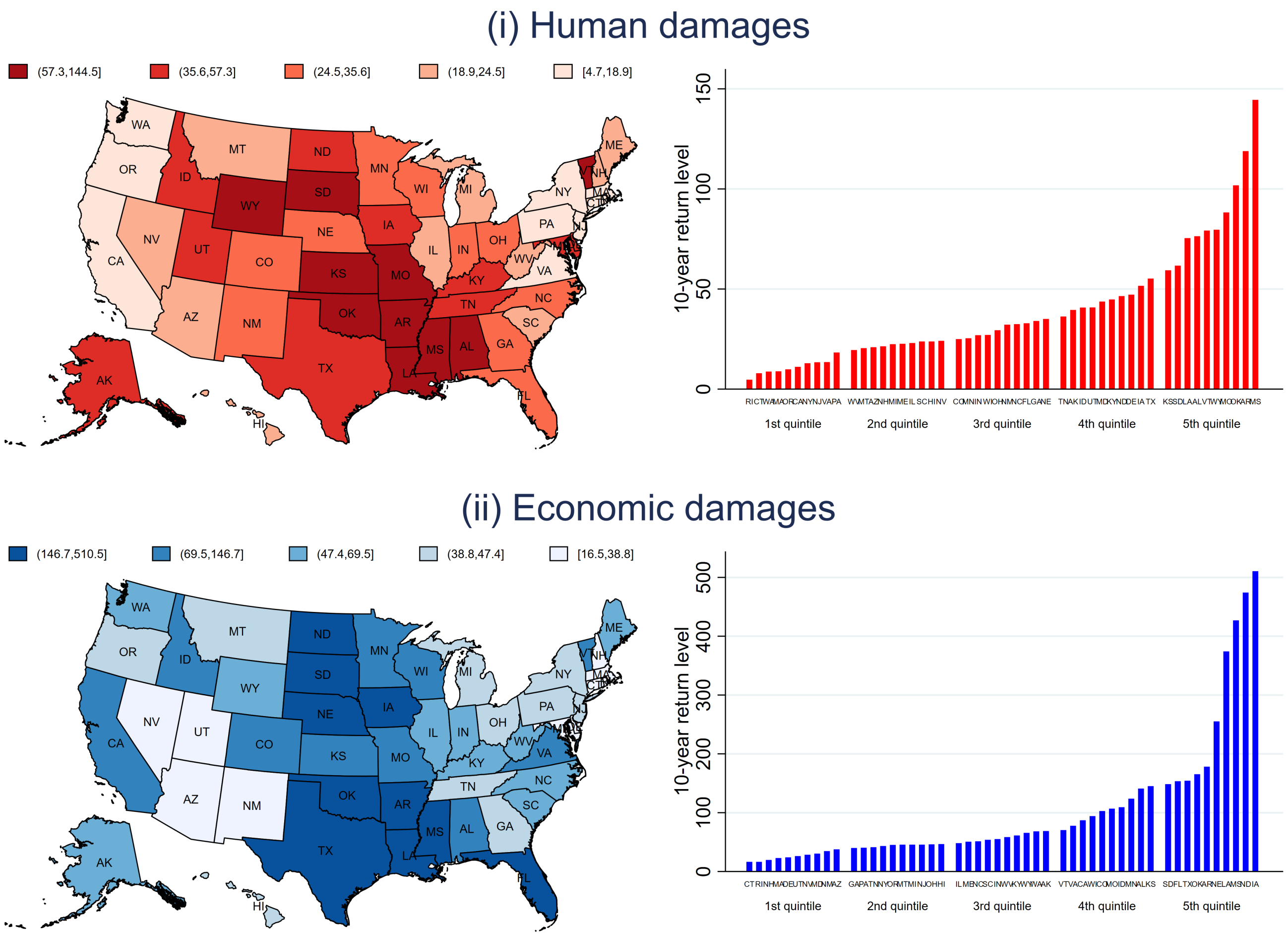
Figures and Tables

Figure 1: Cumulative damages by climate-related natural disasters in the US (1960-2014)



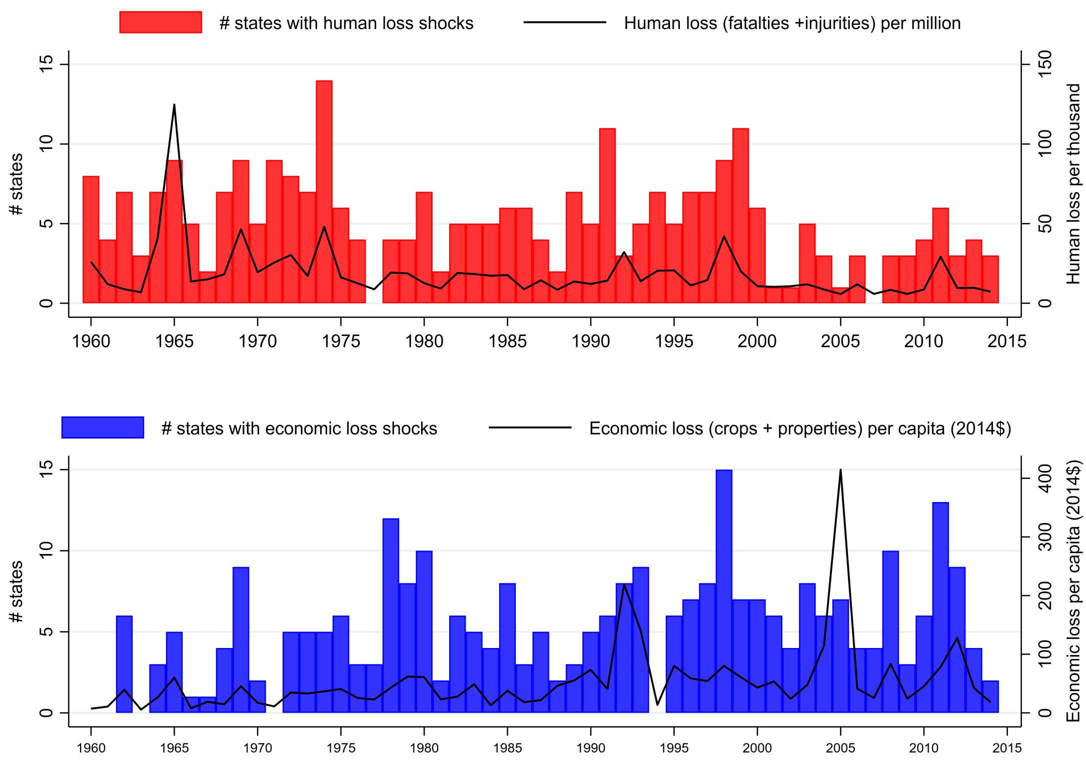
Source: Authors aggregated SHELDUS data between 1960-2014 excluding disasters not related to climate (namely Avalanches, Tsunami/Seiches, Volcanoes, and Earthquakes). Human damages are calculated as the total number of people injured or killed. Economic damages are calculated as the total damages to crops and properties (in 2014 USD).

Figure 2: Estimates of the 10-year return level for annual damages caused by climate-related natural disasters by state (1960 - 2014)



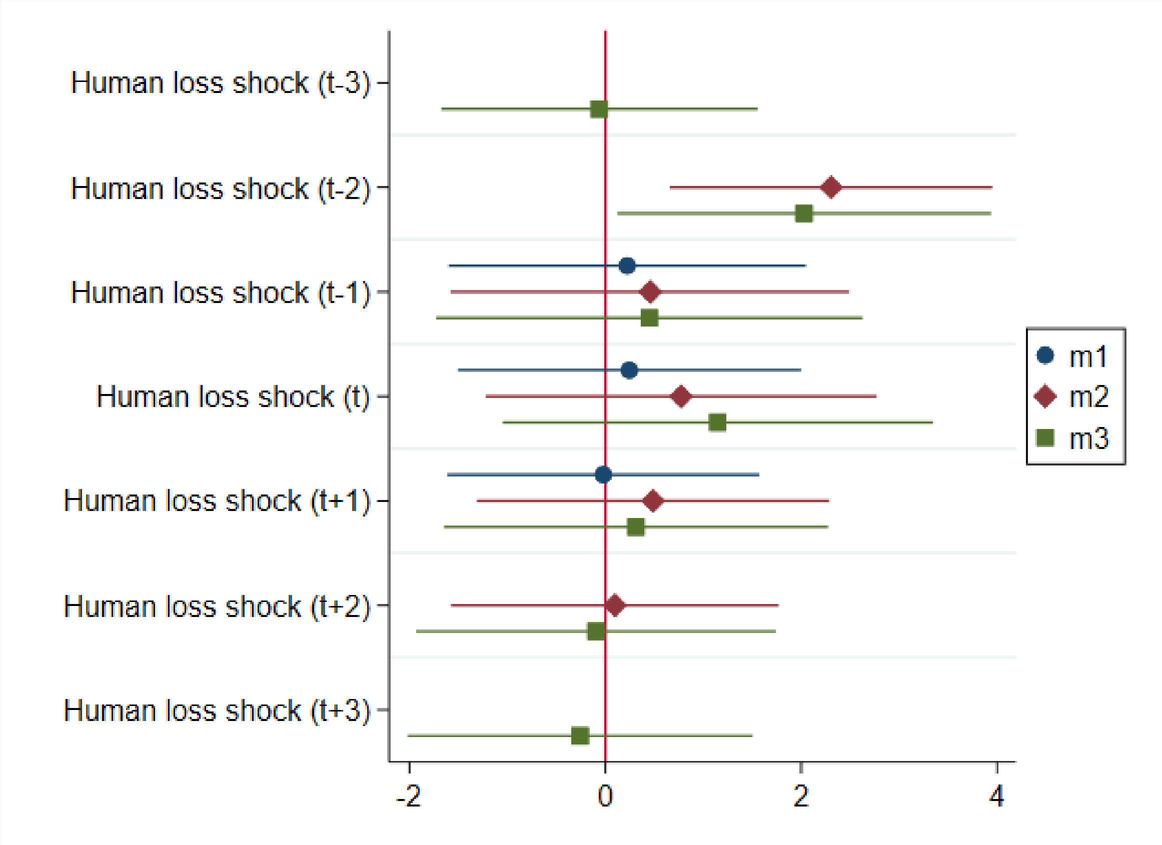
Source: Authors calculations using SHELDUS data between 1960-2014 for 50 states that have Senators. Human damages are calculated as the total number of people injured or killed per one million people. Economic damages are calculated as the total damages to crops and property (in 2014 USD) per capita. Our calculations only include climate-related disasters (Figure 1). See text for details. The x-axes of the bar charts are 2-letter state postal abbreviations.

Figure 3: Natural disaster shocks over time



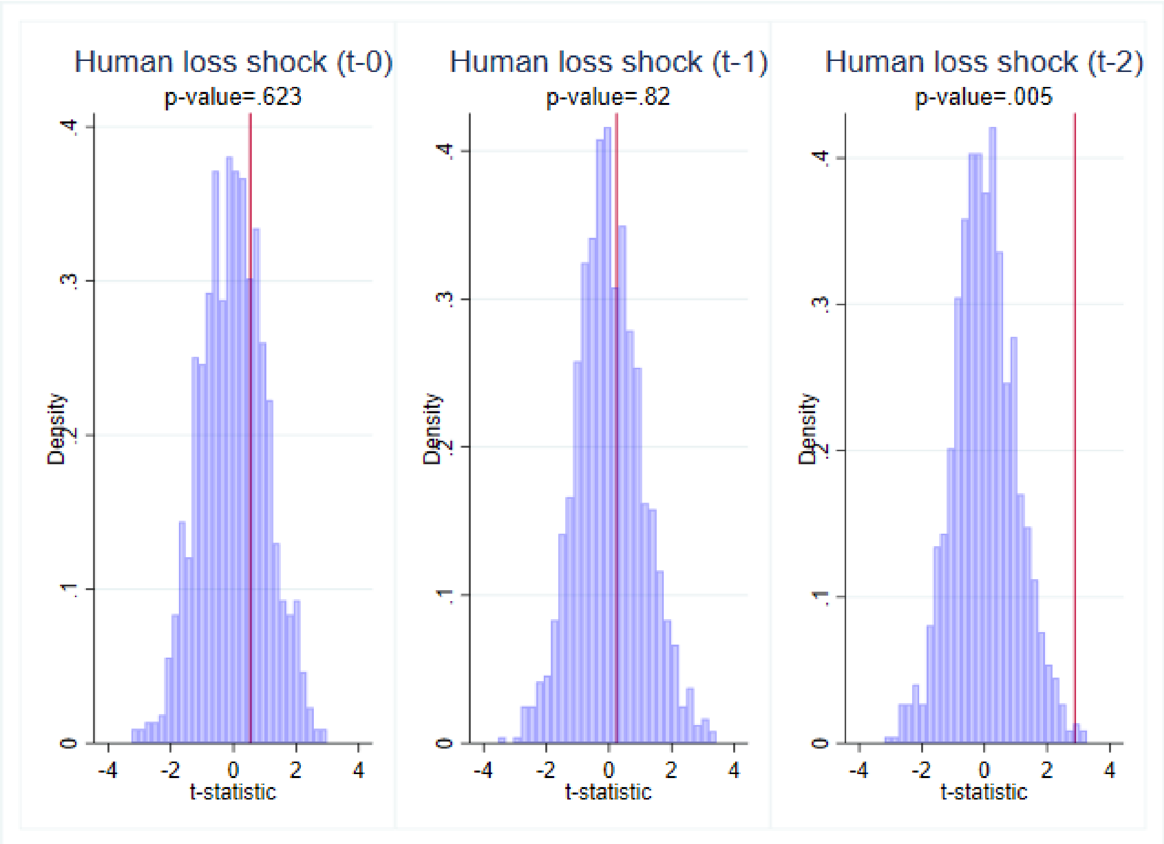
Source: Authors calculations using the SHELDUS data for 50 states that have Senators. Human losses are the sum of fatalities and injuries. Economic losses are the sum of damages (in 2014 dollars) to crops and properties caused by natural disasters. Our calculations only include climate-related disasters (Figure 1). Economic/human loss shocks are the years that economic/human losses (per capita) of a particular state exceed their 10-year return levels as detailed in Figures 2.

Figure 4: Granger-style causality test



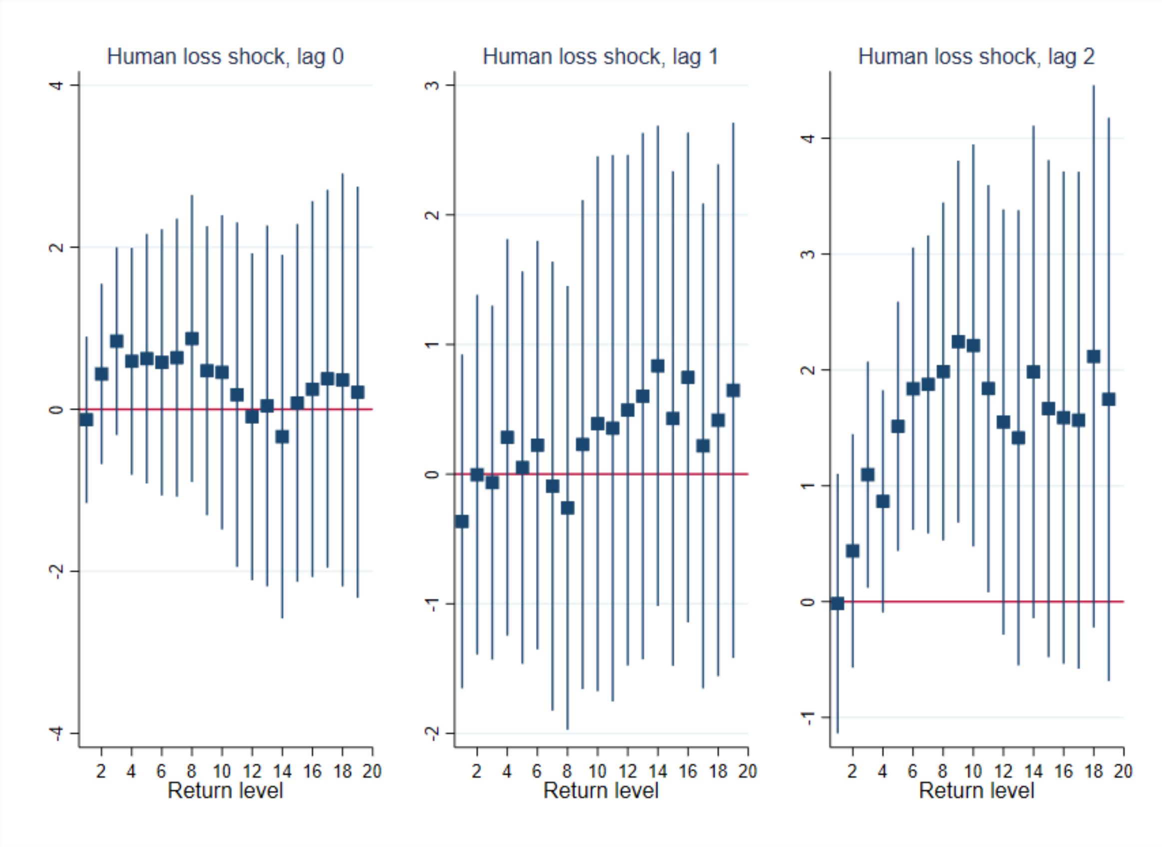
Note: Figure 4 presents the coefficients and 95% confidence intervals of Granger-style causality tests that regress LCV score of senators on contemporary term of human loss shock and its lags and leads, senator-by-term FE, year FE, and other control variables identical to the baseline specifications (Table 2). Models m1, m2, m3 respectively set the length of lags and leads at 1, 2 and 3. Standard errors are clustered at the state level.

Figure 5: Monte-Carlo permutation Test



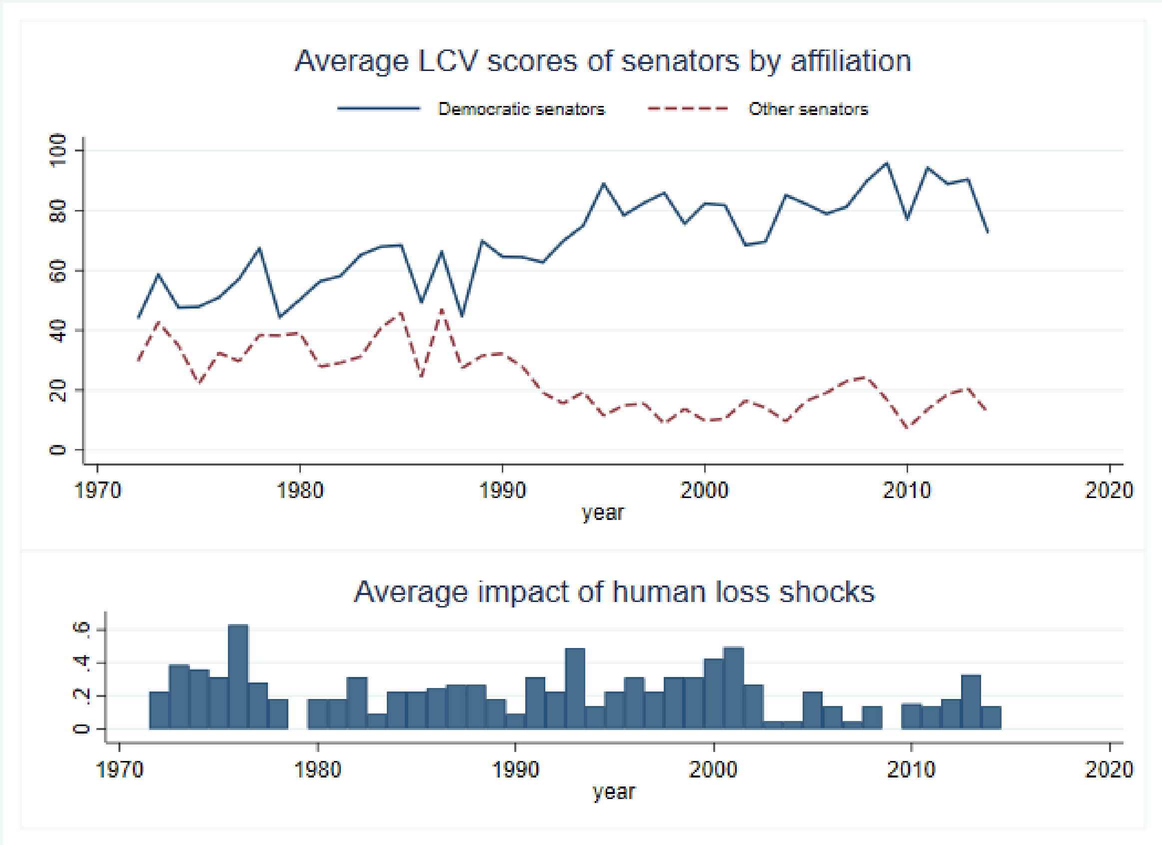
Note: Figure 5 illustrates the t-statistics (vertical red lines) of natural disaster shocks and their lags in the benchmark regression (column (7) of Table 2) and the 2-tail p-values of these statistics derived from a Monte Carlo permutation test. The distribution of t-statistics (blue histogram) is constructed from the 1,000 repetitions of the benchmark regression with natural disaster shocks randomly reassigned to each state-year pair by a binomial distribution with a probability of 10%. The p-values are calculated as the percentage of t-statistics in the distribution that are more extreme than the benchmark values.

Figure 6: Return period sensitivity for human loss shocks



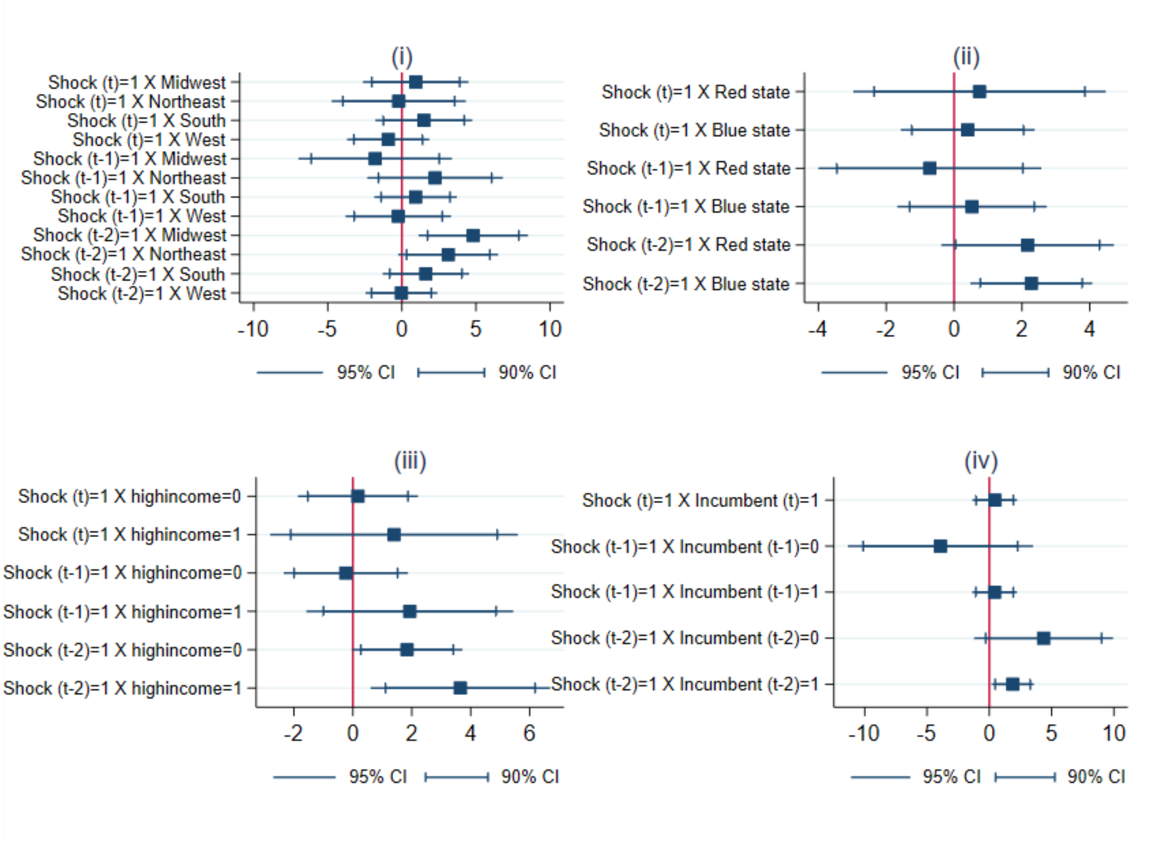
Note: Figure 6 illustrates the coefficients and 95% confidence intervals of human loss shocks and their 2 lags in the benchmark regression using FE (Column 3 of Table 2), where the shocks are defined by varying values of return periods in the EVT model.

Figure 7: Counterfactual analysis: Baseline regression



Note: Figure 7 illustrates the average LCV scores of (modelled) senators by affiliation (upper panel) and the yearly average of cumulative impact of human loss shocks and their lags (lower panel) using the benchmark specification.

Figure 8: Heterogeneous Treatment Effects



Note: Figure 8 presents coefficients and confidence intervals (at 95% and 90% levels) of regressions that explore the heterogeneity in treatment effects. Panel (i) compares responses of senators from different regions. Panel (ii) compares senators from constituencies with different state partisanship. Using time-varying estimates of party identification of mass public by Caughey and Warshaw (2018), we code a pair of state-year as "Blue state" if estimated proportion of Democratic identifiers exceeds Republican identifiers, otherwise "Red state". Panel (iii) analyses the role of economic condition with high income states defined as those at the top quintile of (average) personal income. Panel (iv) analyses responses of senators who held the office when the past shocks occurred versus those did not. The dependent variable is LCV score of senators. All regressions include senator-by-term FE, year FE, and other control variables identical to the baseline specification (Table 2). Standard errors clustered are at state level.

Tables

Table 1: Summary statistics

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Annual legislators' score (0-100 scale)	4,414	47.02	34.54	0	100
Human loss shocks (10-year return)	4,414	0.100	0.301	0	1
Economic loss shocks (10-year return)	4,414	0.118	0.322	0	1
NOMINATE dim. 1 (Economic/Redistributive)	4,414	0.00264	0.354	-0.762	0.919
NOMINATE dim. 2 (Other Vote)	4,414	-0.0663	0.449	-1	1
Democrat [binary]	4,414	0.519	0.500	0	1
First year at the Senate [binary]	4,414	0.0662	0.249	0	1
Senator in election year [binary]	4,414	0.167	0.373	0	1
Age	4,414	58.55	10.37	31	100
Male senator [binary]	4,414	0.927	0.260	0	1
Share of black voters	4,414	0.0998	0.0936	0.00186	0.380
Share of 18-29 voters	4,414	0.257	0.0438	0.178	0.420
Share of 30-44 voters	4,414	0.285	0.0360	0.213	0.443
Share of 45-64 voters	4,414	0.293	0.0386	0.203	0.395
Personal income, $2014	4,414	32.58	9.403	13.96	66.86
Real growth rate, %	4,414	3.030	4.050	-28.18	43.55
Average temperature (^0C)	4,414	11.11	5.020	-5.403	24.56
Precipitation (inches)	4,414	37.89	14.90	6.240	94.31
Share of Air issues, %	4,414	7.369	13.15	0	66.67
Share of Clean Energy issues, %	4,414	10.73	12.16	0	50
Share of Climate change issues, %	4,414	5.833	11.97	0	40
Share of Dirty Energy issues, %	4,414	35.77	13.94	0	66.67
Share of Land issues, %	4,414	33.57	19.94	0	85.71
Share of Other issues, %	4,414	19.60	13.42	0	50
Share of Transportation issues, %	4,414	8.000	11.30	0	53.85
Share of Water issues, %	4,414	28.74	13.90	0	71.43

Note: Dirty Energy issues is short for Dirty Energy/Toxics/Public Right to Know issues. Water issues is short for Water, Oceans and Drilling issues. Land issues is short for Lands/Forest/Wildlife issues.

Table 2: Senatorial votes for environment related issues in response to natural disaster(s)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE
Human loss shock (t)	0.17	0.12	0.48	0.81					0.052	0.0098	0.43	0.73
	(0.82)	(0.81)	(0.89)	(0.92)					(0.84)	(0.83)	(0.91)	(0.94)
Human loss shock (t-1)		0.086	0.23	0.27						0.051	0.19	0.25
		(0.90)	(0.94)	(0.98)						(0.90)	(0.94)	(0.98)
Human loss shock (t-2)			2.24***	2.06**							2.42***	2.20**
			(0.78)	(0.86)							(0.79)	(0.89)
Human loss shock (t-3)				-0.19								-0.18
				(0.74)								(0.71)
Economic loss shock (t)					0.67	0.69	0.50	0.67	0.67	0.69	0.42	0.55
					(0.79)	(0.81)	(0.82)	(0.87)	(0.82)	(0.83)	(0.85)	(0.91)
Economic loss shock (t-1)						0.20	0.065	0.064		0.19	0.015	0.019
						(0.78)	(0.81)	(0.84)		(0.78)	(0.81)	(0.84)
Economic loss shock (t-2)							-0.69	-0.56			-1.06	-0.89
							(0.69)	(0.75)			(0.72)	(0.78)
Economic loss shock (t-3)								-0.11				-0.072
								(0.83)				(0.79)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4414	4414	4314	4214	4414	4414	4314	4214	4414	4414	4314	4214

Note: The dependent variable is the annual LCV score of senators. Besides senator-by-term fixed effects and year fixed effects, the regressions also include: (1) controls for senator characteristics such as ideology, party affiliation, tenure characteristics, age, gender (2) lags of state characteristics just prior to the earliest modelled shocks (demographic and macroeconomic conditions of constituency), (3) contemporaneous terms and lags of weather controls and (4) controls for issue composition. Standard errors clustered at state level are in parentheses. *,**,*** are estimates significant at 10%, 5% and 1%, respectively.

Table 3: Robustness check: Alternative specifications

	Senator-by-term FE							Senator FE	State FE	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Human loss shock (t)	0.48	0.43	0.31	0.54		-0.33	0.13	1.07	0.77	1.07
	(0.89)	(0.89)	(0.96)	(0.89)		(0.84)	(0.87)	(0.79)	(0.87)	(0.91)
Human loss shock (t-1)	0.23	0.18	-0.73	0.29		0.26	0.052	0.67	0.65	0.81
	(0.94)	(0.93)	(0.94)	(0.93)		(0.95)	(0.93)	(1.02)	(0.89)	(0.93)
Human loss shock (t-2)	2.24***	2.17***	1.76**	2.26***	2.13***	1.86**	2.10**	2.16**	1.99**	2.17**
	(0.78)	(0.78)	(0.82)	(0.79)	(0.73)	(0.79)	(0.88)	(0.85)	(0.97)	(1.01)
Senator Score (t-1)						-0.066***	-0.096***			
						(0.022)	(0.026)			
Senator Score (t-2)							-0.13***			
							(0.019)			
Ideology & Party controls	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ADA score control (lagged)	No	No	Yes	No	No	No	No	No	No	No
Other senator char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic controls (lagged)	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Other constituency controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Issue controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4314	4314	3318	4314	4314	4028	3659	4314	4314	4314

Note: The dependent variable is annual LCV score of senators. Column (1) of Table 3 replicate the benchmark specification in column (3) of Table 2 (including senator-by-term FE, year FE and other controls). Other columns modify this specification as indicated in the table. See text for more details. Standard errors clustered at state level are in parentheses. *,**,*** are estimates significant at 10%, 5% and 1%, respectively.

Table 4: Compound Shocks

	10-year return level shocks				5-year return level shocks			
	(1) From t-1	(2) From t-2	(3) From t-3	(4) From t-4	(5) From t-1	(6) From t-2	(7) From t-3	(8) From t-4
Number of human loss shocks	-0.095	0.99	0.54	0.44	0.29	0.58	0.29	-0.087
	(0.65)	(0.61)	(0.59)	(0.49)	(0.52)	(0.44)	(0.39)	(0.37)
Observations	4314	4314	4314	4314	4314	4314	4314	4314

Note: The dependent variable is annual LCV score of senators. The variable of interest is cumulative number of human loss shocks from the year specified in column title. Human loss shocks are defined using either 10-year return level or 5-year return level. All regressions also include senator-by-term FE, year FE and other controls identical to Table 2. Standard errors clustered at state level are in parentheses. *,**,*** are estimates significant at 10%, 5% and 1%, respectively.

Table 5: Spillover effects

	(1) Senator Score	(2) Senator Score	(3) Senator Score
Human loss shock (t)	0.48	0.50	0.47
	(0.89)	(0.88)	(0.88)
Human loss shock (t-1)	0.23	0.26	0.30
	(0.94)	(0.95)	(0.95)
Human loss shock (t-2)	2.24^{***}	2.25^{***}	2.24^{***}
	(0.78)	(0.77)	(0.78)
Human loss shocks in any neighboring state (t)		-0.31	
		(0.59)	
Human loss shocks in any neighboring state (t-1)		-0.80	
		(0.73)	
Human loss shocks in any neighboring state (t-2)		-0.064	
		(0.67)	
Average human loss shocks in neighboring states (t)			-0.065
			(1.79)
Average human loss shocks in neighboring states (t-1)			-2.81
			(2.09)
Average human loss shocks in neighboring states (t-2)			0.024
			(1.76)
Observations	4314	4314	4314

Note: The dependent variable is annual LCV score of senators. Column (1) of Table 5 replicates the benchmark specification in column (3) of Table 2 (including senator-by-term FE, year FE and other controls). Other columns modify this specification by adding different measures of human loss shocks in neighbouring states. *,**,*** are estimates significant at 10%, 5% and 1%, respectively.

Table 6: Different Periods and Alternative Scores

	Full Score			Climate-related Score			Other Score		
	(1) Full sample	(2) Before 1990	(3) Since 1990	(4) Full sample	(5) Before 1990	(6) Since 1990	(7) Full sample	(8) Before 1990	(9) Since 1990
Human loss shock (t)	0.48	0.032	1.27	3.03*	2.43	4.32**	-1.09	-2.25	-0.039
	(0.89)	(1.20)	(1.15)	(1.61)	(3.16)	(1.82)	(1.03)	(2.18)	(1.22)
Human loss shock (t-1)	0.23	0.26	0.64	1.16	2.28	1.38	-0.96	-3.34	0.89
	(0.94)	(1.60)	(0.98)	(1.32)	(2.15)	(1.49)	(1.39)	(2.46)	(1.31)
Human loss shock (t-2)	2.24***	2.86**	2.08**	3.28**	4.30**	2.83*	1.09	1.09	1.20
	(0.78)	(1.37)	(0.82)	(1.30)	(1.99)	(1.46)	(1.20)	(1.88)	(1.27)
Observations	4314	1798	2516	4311	1798	2513	4302	1796	2506

Note: The dependent variable is indicated in the first row. Full Score is annual LCV score of senators calculated from all issues. Climate-related Score is calculated fro subset of LCV issues concerning climate change, air, energy, and transportation. Other Score is calculated from the rest of the recorded LCV issues. All regressions also inc senator-by-term FE, year FE and other controls identical to Table 2. Standard errors clustered at state level are in parentheses. *,**,*** are estimates significant at 10%, and 1%, respectively.

A Appendix A: Extreme Value Theory (EVT) and Peaks Over a Threshold model (POT)

Appendix A outlines how we apply the Extreme Value Theory (EVT) and a Peaks Over a Threshold model (POT) to construct our measures of natural disaster shocks. Our approach is to examine the sequence of intensity of annual damages (in terms of either economic or human cost) $\{X_{j1}, X_{j2}, ...X_{jt}\}$ caused by ND in state j (among 50 states), in year t of which, thanks to the SHELDUS database, we have observations over 55 years (between 1960 and 2014). We are interested in modelling the unknown 'fat-tail' distribution of the maximum of the intensity sequence, which is assumed to be independent and identically distributed (i.i.d): $M_{jt} = \max\{X_{j1}, X_{j2}, ..., X_{jt}\}$. If we can normalise $\{M_{jt}\}$ by the appropriate sequences of constants $\{a_{jt} > 0\}$ and $\{b_{jt}\}$ we can obtain a non-degenerate distribution function G_j for all $y \in \mathbb{R}$:

$$Pr\left\{\frac{M_{jt} - b_{jt}}{a_{jt}} \leq y\right\} \rightarrow G(y) \text{ as } t \rightarrow \infty \tag{A.1}$$

then by the Extremal Types Theorem (Fisher and Tippett, 1928), $G(y)$ must fall into one of three extreme value distribution families: Fréche, Gumbel or Weilbell. Furthermore, these families are specific cases of a single parametric distribution, namely the Generalised Extreme Value (GEV) (Jenkinson, 1955). This allows us to approximate the distribution of its maximum $\{M_{jt}\}$ by a GEV distribution, which is characterised by three parameters: location (μ_j), scale (σ_j), and shape (ξ_j):[20]

[20] We use subscription j for the GEV distribution function and its parameters here to hightlight the fact that we model state j separately. As revealed by its name, the parameter ξ_j controls the shape of the GEV. It falls into either the Fréche, Gumbel or Weilbell family if $\xi > 0$, $\xi \rightarrow 0$ and $\xi < 0$, respectively.

$$Pr\{M_{jt} \leq z\} \approx G_j(z) = \exp\left\{ -\left[1 + \xi_j\left(\frac{z - \mu_j}{\sigma_j}\right)\right]^{-1/\xi_j}\right\}, \quad -\infty < z < \infty \tag{A.2}$$

Then the positive excesses over a threshold u_j are large enough ($y_{jk} = X_{jk} - u_j$ conditional on $X_{jk} > u_j$) to approximate a generalised Pareto distribution (GPD) (Pickands, 1975):

$$Pr\{X_{jk} - u_j \leq y | X_{jk} > u_j\} \approx H_j(y) = 1 - \left(1 + \frac{\xi_j y}{\sigma_j + \xi_j(u_j - \mu_j)}\right)^{-1/\xi_j} \tag{A.3}$$

Intuitively, as we are modelling the upper tail of an i.i.d distribution, only observations on the right matter. Hence, the approach requires the selection of a threshold u_j to determine the observations to be used for the approximation. The GPD $H_j(y)$ includes 2 parameters: a shape parameter ξ_j, which is the same as the shape parameter of the GEV in (A.2) and a scale parameter $\tilde{\sigma}_j = \sigma_j + \xi_j(u_j - \mu_j)$, both of which can be estimated using a Maximum Likelihood Estimator (MLE) and observed positive threshold excesses. The approach is sensitive to the choice of threshold u_j as there is a trade off between unbiasedness and accuracy of the estimates. As only observations that exceed the threshold enter the estimation, a lower threshold retains more observations, yielding lower standard errors but at the cost of possible violation of the asymptotic basis of the model, which may cause biasness. In practice, a small threshold is preferable as long as the limit model can provide a reasonable approximation (Coles, 2001).

The selection of such a threshold can be aided by the use of relevant visualising tools. From the GPD (A.3), we have:

$$E\{X_{jk} - u_j | X_{jk} > u_j\} = \frac{\sigma_j + \xi_j(u_j - \mu_j)}{1 - \xi_j} \tag{A.4}$$

Note that (μ_j, σ_j, ξ_j) are the fixed parameters of the GEV $G_j(z)$ in (A.2). Thus, (A.4) is obviously a linear function of threshold u_j as long as u_j is large enough to validate the asymptotic approximation behind the EVT. This motivates the use of the mean residual life plot (MRL), which visualises the relationship between the average of positive excesses corresponding to threshold u_j and the threshold:

$$MRL = \left\{ \left(u_j, \frac{1}{n_{u_j}} \sum_{X_{jk} > u_j} (X_{jk} - u_j) \right) : u_j < X_j^{\max} \right\} \tag{A.5}$$

where n_{u_j} is the number of all exceedances defined by the threshold u_j in the dataset (all X_{jt} exceeding the threshold u_j). The strategy is to pick the lowest threshold u_j, above which the MRL illustrates a reasonable line, taking into account the sample variations (i.e., incorporating the confidence intervals derived from the approximate normality of sample means). Once a reasonable threshold is obtained, the log-likelihood functions as detailed in Coles (2001) were adopted to estimate the pair of parameters $\hat{\xi}_j$ and $\hat{\tilde{\sigma}}_j$. Note that as long as the asymptotic approximation is valid (u_j is large enough), the shape parameter ξ_j and the transformed scale parameter $\tilde{\sigma}_j^* = \tilde{\sigma}_j - \xi_j u_j = \sigma_j - \xi_j \mu_j$ are independent of the threshold u_j. This enables a post-estimation strategy to assess the selection of threshold u_j: to plot the estimates of ξ_j and the transformed $\tilde{\sigma}_j$ against a range of corresponding u_j and validate whether the estimates become stable when u_j increases above the chosen threshold.

Finally, the estimation can also be validated by a number of diagnostic plots including probability plots, quantile plots, return level plots, and density plots (Coles, 2001). The first three plots compare the model-based and empirical estimates of the distribution function. The last one compares the density function of the fitted model with a histogram of the data.

Once we obtain the satisfactory ML estimates of the parameters $(\hat{\xi}_j; \hat{\sigma}_j)$, the N-year return level is estimated by:

$$\hat{z}_j(N) = u_j + \frac{\hat{\sigma}_j}{\hat{\xi}_j}[(N\hat{\zeta}_{u_j})^{\hat{\xi}_j} - 1] \qquad \text{for } \hat{\xi}_j \neq 0 \tag{A.6}$$

$$\hat{z}_j(N) = u_j + \hat{\sigma}_j log(N\hat{\zeta}_{u_j}) \qquad \text{for } \hat{\xi}_j = 0 \tag{A.7}$$

where ζ_{u_j} is the probability of an individual observation X_{jt} greater than the threshold u_j and can be estimated by the ratio between the number of observations in the sample that exceed the chosen threshold (k_j) and the total observation number (n_j): $\hat{\zeta}_{u_j} = k_j/n_j$. The standard errors and confidence intervals of the return levels can be computed by the delta method, taking into account the uncertainty due to ζ_{u_j} (Coles, 2001).

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