Online Appendix to "Hurricanes, Climate Change Policies and Electoral Accountability," by Stefano Gagliarducci, M. Daniele Paserman and Eleonora Patacchini, July 2025.

A Appendix

A.1 Definition of 'green' bills: further details

A research assistant read the content of all bills identified either by the CBP (see Section 2) as aimed at contrasting climate change, and classified each of them according to the following (non mutually exclusive) flags: "against the environment", related to "noise pollution", providing "relief funds".

Specifically, a bill was considered as "against the environment" if:

- it prohibits, limits, or delays the authority of Federal agencies or other U.S. authorities to issue regulations, decrees, or orders to implement international protocols or agreements;
- it prohibits or limits U.S. contributions to international programs aimed at protecting the environment;[1]
- it prohibits, limits or delays the use of Federal funds to implement environmental friendly regulation (e.g. limitation of carbon dioxide emissions, greenhouse gas emission reductions) or finance grant programs;[2]
- it prohibits, limits, or delays subsidies or credit to households or firms for using renewable energy or environmentally friendly goods;

[1] Examples of international programs are: the *Kyoto Protocol to the United Nations Framework Convention on Climate Change*, and the *Intergovernmental Panel on Climate Change*.

[2] Examples of environmentally friendly regulation are: limitation of carbon dioxide emissions, greenhouse gas emission reductions, ozone standards, greenhouse gas emissions from mobile sources, emissions from fossil fuel-fired electric utility generating units. Examples of grant programs are: EPA *National Clean Diesel Campaign*, EPA *Environmental Justice Program*, *Greenhouse Gas Reporting Program*, *Global Methane Initiative*, *Climate Resilience Fund*, *Climate Resilience Evaluation Awareness Tool*, *Green Infrastructure Program*, *Climate Ready Water Utilities Initiative*.

- it prohibits, limits, or delays the introduction of programs to reduce the effects of emissions;
- it prohibits, limits, or delays the introduction of taxes or fees on emissions (e.g., carbon dioxide emissions);
- it waives the requirements introduced by previous regulation;
- it waives temporarily taxes on traditional energy sources to decrease the price of energy;
- it simplifies the implementation of the Keystone pipeline, against which there was a strong campaign by environmentalist associations;
- it expresses the sense of Congress against taxes or tax increases on traditional energy sources;
- it expresses skepticism on research documenting global warming or climate change.

Finally, a bill was considered as providing "relief funds" if it introduces additional relief funds to the victims of a natural disaster, and related to "noise pollution" if it introduces special regulations or taxes against noise pollution, mainly related to aviation and aeronautic regulations.

Of the initial 968 bills identified by the CBP as aimed at combating climate change, 94 turned out to be "against the environment", 2 providing "relief funds", and 6 related to "noise pollution".

A.2 Robustness checks

In Table A.1, we assess the robustness of the balance tests (Table 2 in the main text) to alternative specifications of the fixed effects. Specifically, in the top panel of the table, we replace state $\times$ decade fixed effects with district fixed effects, while in the middle and bottom panels, we replace state $\times$ decade fixed effects with individual fixed effects. Note that we perform the analysis with district fixed effects on individual characteristics only, as county characteristics do not vary within decades and would eventually be absorbed by district fixed effects (which are defined as a congressional district-decade pair).

Results on individual characteristics are substantially unchanged when using each of the alternative specifications. Results on county characteristics show instead a statistically significant difference in the unemployment rate depending on hurricane occurrence, while the rest of the variables remain statistically balanced. As we discuss in the main text, the difference in the unemployment rate that shows up in this specification might reflect the fact that the incidence of hurricanes is higher in districts close to the coast. Specifications that include district fixed effects are likely to address this potential confounder.

In the top panel Table A.2 we assess the robustness of our main results (Table 3 in the main text) to alternative definitions of the sample, of hurricane incidence, and green bills. We follow our preferred specification, which controls for all individual and district characteristics and district$\times$decade fixed effects (i.e., specification (4) in Table 3). We use the specification in the second panel of Table 3, in which we define a state as hit by a hurricane if it was hit in year $t-1$.

In column (1), we restrict the analysis to states most frequently hit by hurricanes, and from which we derive most of the variation we use to identify the treatment. These are the states on the Atlantic coast and on the Gulf of Mexico (i.e., the Census divisions of New England, Middle Atlantic, South Atlantic, East South Central, and West South Central). The point estimate is slightly smaller than in Table 3, but still positive and statistically significant.

In column (2), we use as the key right hand side variable the share of counties in the district affected by the hurricane (instead of a binary variable indicating whether any county in the district was affected by a hurricane). In column (3), instead, we use the share of the population in counties affected by the hurricane over the total population in the district. Both specifications show that moving from zero to one hundred percent of the district being hit by a hurricane increases the number of green bills by about 0.17. The results of these specifications are essentially indistinguishable from those of Table 3.

One concern with these measures of hurricane incidence is that they are based on FEMA disaster declarations. These declarations, as well as the intensity of FEMA assistance, may be themselves affected by the political environment and therefore not completely exogenous. Therefore, in column (4) we replace the key right-hand side variable with the highest wind speed recorded across all counties affected by the hurricane, measured in 100s mph.[3] This is a potentially more "objective" measure of hurricane incidence, even though it may suffer from some measurement error because of the way wind speed is measured. Specifically, the hurricane's wind speed is measured in the eye of the storm (according to Weather Underground). However, it is not clear how to assess how much a county was affected based on the wind speed measured in the eye of the hurricane (see pp. 8-10 in National Oceanic and Atmospheric Administration, 1999). Because of this, we believe that the FEMA declarations are a better assessment of whether a county deserved assistance because of the damage induced by the hurricane. Reassuringly, the coefficient is still positive and statistically significant. The magnitude of the coefficient is in line with previous ones: the average hurricane has a maximum wind speed of about 45 miles per hour, meaning that going from no hurricane to an average hurricane raises the number of bills by about 0.11.

In column (5), we exclude from the count of green bills the ones related to air pollution. Even though we verified manually that these bills almost always aim to reduce CO2 emissions, one may be worried that they may not be tied specifically to the threat of climate change. The results are again substantively unchanged. Finally, in column (6), we exclude FEMA emergency declarations to address concerns that these declarations primarily help areas prepare for potential future natural disasters, rather than provide assistance to areas that already experienced damage.[4] Our primary coefficient of interest remains substantially unchanged, although it becomes less precisely estimated. This finding indirectly suggests that residents in these areas continue to experience a salient reminder about the possibility of extreme events.

In Table A.3 we explore the robustness of the results to different forms of clustering, and other specification issues. In columns (1)-(4), we replicate the preferred specification

[3] Specifically, we consider the wind speed recorded on the five points on the actual hurricane trajectory that are closest to the county centroid, and weight those values by the inverse of the distance from the county centroid. We assign the maximum of these five recorded speeds as the county's experienced wind speed.

[4] The President can declare a major disaster for any natural event that has caused damage of such severity that it is beyond the combined capabilities of state and local governments to respond. A Major Disaster Declaration provides a wide range of federal assistance programs for individuals and public infrastructure, including funds for both emergency and permanent work. Emergency Declarations can be issued for any situation in which the President determines that federal assistance is necessary. They are intended to supplement state and local government efforts in providing emergency services or in mitigating or preventing the threat of a catastrophe.

of Table 3 in the text (third panel, column 4), but cluster the standard errors at either the individual congress member level, the state×decade, the state×year level, or the state and year level. The main coefficient remains statistically significant in all these specifications, including in column (4), where we implement a more demanding (in the case of a short panel like ours) two-way clustering.[5]

We next explore whether our main results are biased because of the inclusion of contemporaneous controls in the regression: these include the total number of other (i.e., "non-green") bills sponsored or cosponsored, the unemployment rate, the committee membership dummies, and the indicator for whether a congress member is a House leader (speaker, minority/majority leader/whip, standing committee chair). One may be concerned that these are "bad controls," because the occurrence of a hurricane may affect not only the support for green legislation but also other aspects of legislative activity, committee assignments, or economic conditions in the district. Therefore, we replace the number of other bills sponsored or cosponsored in year t with the number of bills sponsored or cosponsored in year $t-2$; and similarly, we replace the contemporaneous unemployment rate with the unemployment rate lagged two years. In doing this, we lose observations for which we do not observe the variable at a two-year lag (e.g., observations for rookie congress members and observations at the beginning of the sample period). We drop the committee membership dummies and the indicator for being a leader, as there is no natural way to construct a two-year lag of these variables. Column (5) of Table A.3 replicates the main specification (with controls for contemporaneous number of bills sponsored or cosponsored), but using just the sample of observations in which we observe all the relevant variables lagged two years. Even though we lose more than 20% of the observations, the coefficient is almost indistinguishable from that of the main specification. The results of our estimation without any potentially bad controls are presented in column (6). The point estimate drops from 0.170 to 0.137, and

[5] Standard errors increase (nontrivially) when they are two-way clustered by state and year, rather than just by state. To tell whether this is because of a reduction in the degrees of freedom or because two-way clustering genuinely captures correlation in the residual that simpler clustering schemes miss, we ran a placebo exercise. Specifically, we randomly drew (w/o replacement) new years for hurricanes to hit (i.e., we reshuffled the dates of occurrence of hurricanes); then, we estimated our preferred specification and recorded the coefficient of interest and its standard error (clustered at the state level), and repeated this procedure 2000 times. The results, reported in Figure A.3, show that the null of no effect is rejected for less than 5% of the placebo estimates, which implies that clustering by state was conservative enough.

remains statistically significant. It does not appear that including the contemporaneous controls introduces substantial bias.

Finally, we address recent concerns on two-way fixed effects models with staggered treatment adoption and treatment effect heterogeneity (for a summary, see Roth et al., 2023). The main concern is that in any two-way fixed effects estimate of Diff-in-Diff, already-treated units are kept as controls – something that might introduce bias in the presence of heterogeneous effects across groups experiencing treatment at different points in time. To address this concern, we implement the Cengiz et al. (2019) stacked-by-event strategy, which ensures that we use as comparison units to districts hit by a hurricane only districts that were never hit, or those that where hit later (see also section 5 and Appendix B for more details on the construction of thee sample used in this analysis). The results are presented in the last two columns of Table A.3: column 7 uses only never-treated units as the comparison group, while column 8 includes both never-treated and not-yet-treated units. In both cases, the coefficients remain positive and statistically significant. They are slightly larger in magnitude than those in the main analysis.

Overall, these results indicate that our estimates are robust to different forms of clustering and econometric specifications.

Table A.1: Balancing tests - Robustness analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Controlling for district fixed effects							
Individual characteristics:	Leader (0/1)	Republican (0/1)	Minority (0/1)	Margin (perc.)	Female (0/1)	Tenure (terms)	Age (years)	
Hit by hurricane	0.002	-0.009	0.026	0.662	-0.004	0.088	0.334	
	(0.002)	(0.013)	(0.018)	(1.507)	(0.006)	(0.107)	(0.255)	
Avg. outcome	0.0105	0.492	0.447	35.76	0.146	4.520	55.72	
N. year/districts	13,684	13,684	13,684	13,438	13,684	13,005	13,680	
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
	Controlling for individual fixed effects							
District characteristics:	Pop. (log)	Income (log)	Land area (log)	Over 65 share	Black share	Foreign share	Urban share	Unemp. (perc.)
Hit by hurricane	-0.000	0.004	-0.002	-0.066	0.029	-0.016	0.083	-0.294***
	(0.002)	(0.002)	(0.013)	(0.040)	(0.099)	(0.114)	(0.255)	(0.090)
Avg. outcome	13.31	10.17	15.61	13.17	12	9.597	73.80	7.894
N. year/districts	13,347	13,347	13,347	13,347	13,347	13,347	13,347	13,669
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics:	Leader (0/1)	Republican (0/1)	Minority (0/1)	Margin (perc.)	Female (0/1)	Tenure (terms)	Age (years)	
Hit by hurricane	-0.001	0.000	0.006	-0.099		-0.053	0.000	
	(0.002)	(0.002)	(0.022)	(1.563)		(0.035)	(0.003)	
Avg. outcome	0.0104	0.486	0.550	34.45	0.146	4.521	55.72	
N. year/districts	13,669	13,669	13,669	13,362	13,669	12,990	13,665	
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	

Notes. *Hit by hurricane* is a dummy equal to 1 if at least one county in the district was hit by a hurricane. *Leader* is a dummy for being House speaker, minority/majority leader/whip, or standing committee chair. *Minority* is a dummy indicating whether the representative is in the minority party in the House. *Mg. victory* is the relative margin of victory w.r.t. the second candidate. *Tenure* is the number of terms served in Congress. Standard errors clustered by state in parentheses. ***, **, *: denote significant at 1, 5, and 10 percent level, respectively.

Table A.2: Hurricanes and support for green bills - Robustness to alternative measures of hurricane incidence

	(1)	(2)	(3)	(4)	(5)	(6)
	N. green bills					
					No air pollution	No emergency declarations
Hit by hurricane $(t-1)$	0.124**				0.132***	0.122**
	(0.055)				(0.047)	(0.055)
Share counties (t-1)		0.173**				
		(0.065)				
Share population (t-1)			0.175***			
			(0.062)			
Maximum wind intensity (t-1)				0.239*		
				(0.128)		
Avg. outcome	0.908	1.053	1.053	1.053	0.806	1.053
N. year/districts	9,381	13,253	13,253	13,253	13,253	13,253
States	Highly exposed	All	All	All	All	All
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
District × Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes. *Hit by hurricane* is a dummy equal to 1 if at least one county in the district was hit by a hurricane. *N. green bills* sponsored or cosponsored per year/district, as defined by the CBP. In column (1), the highly exposed states are the ones belonging to the Census divisions of New England (ME, NH, VT, MA, CT and RI), Middle Atlantic (NY, NJ, PA), South Atlantic (DE, MD, VA, WV, NC, SC, GA and FL), East South Central (AL, MS, TN and KY), and West South Central (LA, AR, TX and OK) only. In column (5), we exclude from the count of green bills those related to air pollution. In column (6), we consider FEMA major disaster declarations (not emergency declarations) only. *Share counties* is the share of counties in the district affected by the hurricane. *Share population* is the share of population in counties affected by the hurricane over the total population in the district. *Wind* intensity in 100 mph. For a description of *Individual controls* and *District controls* see Table 3 in the text. Standard errors clustered by state in parentheses. ***, **, *: denote significant at 1, 5, and 10 percent level, respectively.

Table A.3: Hurricanes and support for green bills - Robustness to alternative econometric specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	N. green bills							
							Stacked event study Cengiz et al. (2019)	
Hit by hurricane (t-1)	0.170***	0.170***	0.170***	0.170*	0.175**	0.137*	0.270***	0.246***
	(0.045)	(0.056)	(0.058)	(0.097)	(0.065)	(0.074)	(0.100)	(0.098)
Avg. outcome	1.053	1.053	1.053	1.053	1,083	1.083	1.198	1.198
N. year/districts	13,253	13,253	13,253	13,253	10,405	10,405	44,453	49,646
S.e. clustering	Individual	State × Decade	State × Year	State & Year	State	State	State	State
Comparison group: never-treated							Yes	Yes
Comparison group: not-yet-treated							No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls (t-2)					Yes	No		

Notes. *Hit by hurricane* is a dummy equal to 1 if at least one county in the district was hit by a hurricane. *N. green bills* sponsored or cosponsored per year/district, as defined by the CBP. Column (6) includes only controls predetermined at time t-2: it removes controls for committee membership and the Leader indicator; it replaces the n. of other non-green bills sponsored or cosponsored with those at t-2 (only available for congressmen already in office at t-2); and it replaces the contemporaneous unemployment rate with the unemployment rate at time t-2. For a description of *Individual controls* and *District controls* see Table 3 in the main text. Estimates in columns (7) and (8) are based on a stacked event study design as in Cengiz et al (2019). Standard errors clustered by congress member in column (1), by state per decade in column (2), by state per year in column (3), by state and year in column (4), and by state in columns from (5) to (8) in parentheses. ***, **, *: denote significant at 1, 5, and 10 percent level, respectively.

A.3 Additional evidence on main results

In this Section, we provide additional evidence on the effect of other extreme weather events on the support for green legislation and on whether there are spillover effects on geographic areas adjacent to those hit by the hurricane. We also examine alternative potential mechanisms that may explain the increase in support for environmental legislation in the aftermath of a hurricane.

Other weather events. In Table A.4 we look at whether support for green legislation is also tied to the occurrence of other extreme weather events, which are not necessarily perceived to be as strongly linked to climate change as hurricanes. In fact, we find little evidence that the occurrence of snow storms is associated with increased green legislation. Severe coastal storms (unrelated to hurricanes) also do not trigger a response in terms of green legislation.[6] However, the occurrence of tornadoes leads to an increase in the number of green bills, possibly because they catalyze similar media attention. The effect size is similar to that of hurricanes.

Spillover effects. In Table A.5, we look at whether hurricanes have spillover effects on neighboring districts or states. In general, it appears that the effect of the hurricane on support for green bills is concentrated among congress members representing districts directly hit by the hurricane. The response of representatives in adjacent districts is positive, but small and not statistically significant.[7]

Logrolling. Politicians in districts hit by a hurricane may be in a position to leverage the increased visibility of their district to extract policy concessions from their peers, in a quid pro quo bargain. We hypothesize that these exchanges of favors may be more prevalent among representatives who share a tight connection with other congress members with strong environmental preferences. If this hypothesis is correct, we would expect to see that the response to hurricanes is stronger for representatives who have stronger social ties to a large number of other "green" legislators. Following Battaglini and Patacchini (2018), we measure

[6]Hurricanes have distinctive atmospheric features that set them apart from other storms. Nevertheless, the difference between hurricanes and non-hurricane-related storms or other disasters may be just a matter of scale, and not due to differences in how strongly these phenomena are associated with climate change.

[7]This result suggests that if the impact of a hurricane extends to control districts, our baseline estimates likely represent a lower bound of the true effect.

social ties using the network of alumni connections, i.e., those who graduated from the same institution within four years.[8] The advantage of this approach is that it measures social ties that are likely predetermined, and not influenced by shared geography, expertise (as, for example, if we had used networks based on committee membership), or political preferences. We then identify as "green friends" the alumni whose League of Conservation Voters (LCV) lifetime environmental score at time t - 2 was above the median of the Congress.[9] Based on this measure, more than 40% of the representatives have at least 1 "green friend" and a maximum of 16. Table A.6 reveals that the estimated coefficient on the interaction with the number of "green friends" (column 1) is small and, if anything, it is negative. Similar results hold in column (2), where we use the number of representatives from the same state whose LCV lifetime environmental score was above the median of the corresponding year as a measure of network. Therefore, there appears to be little support for the "exchange of favors" hypothesis.

Lobbying. We next explore whether politicians' response to hurricanes is merely driven by capture from environmental lobbying groups. For this purpose, we use the yearly sum of campaign contributions to individual representatives received from environmental PACs and PACs related to the automotive and energy industry. We identified as environmental all PACs classified by the Center for Responsive Politics (CRP) as "Environmental"; as automotive those classified as "Transport"; and as energy those classified as "Oil & Gas", "Electric utilities" and "Coal mining".[10]

In columns (3) and (4) of Table A.6, we use campaign contributions as the dependent variable, and show that representatives of districts hit by a hurricane receive more contributions from environmental PACs and fewer contributions from Energy and automotive PACs, even though the effects are not statistically significant. In column (5), we use the number

[8]While Battaglini and Patacchini (2018) construct networks for the 109th-113th Congresses, we have extracted information on the educational institutions attended by all the congressmen from the 101st to the 113th Congress. The data source is the Biographical Directory of the United States Congress, which is available online (http://bioguide.Congress.gov/biosearch/biosearch.asp).

[9]The LCV lifetime score assigns to each congress member a score between 0 and 100, equal to the share of pro-environment votes cast out of the total number of votes scored.

[10]Note that this definition excludes other subcategories of the energy and natural resources sector, such as the "Miscellaneous Energy" sector, which includes many PACs associated with wind, solar and other renewable energy sources.

of green bills sponsored and cosponsored as the dependent variable, and we include in the regression an interaction between the "hit by hurricane" dummy and log(1+contributions) from each of the three sources (environmental, energy and automotive, and others). [11]

According to the capture theory, we should observe a stronger response to hurricanes in terms of green bills for representatives who received large amounts of campaign contributions from environmental PACs. This is not the case: if anything, the response to hurricanes is smaller for politicians who receive more contributions from environmental PACs. Contributions from the energy and automotive industries do reduce support for green legislation, but this effect reflects in large part the fact that representatives who receive large contributions from the energy and automotive industries are also less likely to have supported environmental causes in the past. In fact, when controlling for the politician's pro-environmental score (as measured by the LCV score) in Column (6) and its interaction with the "hit by hurricane" dummy, the coefficient on the interaction term becomes significantly smaller.

Bills reaching the floor. In Table 7, we documented the relationship between the occurrence of a hurricane and the probability that a bill became law. The precision of the estimates was limited by the small number of bills that actually became law. In Table A.7, we replicate the analysis but use as the dependent variable the number of bills that reached the floor for consideration. The results are broadly in line with those of Table 7, confirming the notion that support for green bills in the aftermath of a hurricane is not pure posturing.

[11] We use the log transformation because contributions from PACs sum to zero for many representatives (from 80% for the environmental contributions, to 10% for the energy and automotive contributions) but they are often very large for some others (up to about 1 million dollars per year for environmental contributions, and to about half million dollars per year for the energy and automotive contributions).

Table A.4: Other disasters and support for green bills

	(1)	(2)	(3)
		N. green bills	
Hit by snow (t-1)	0.030		
	(0.062)		
Hit by non-hurricane related storm (t-1)		-0.017	
		(0.044)	
Hit by tornado (t-1)			0.196***
			(0.073)
N. year/districts	13,253	13,253	13,253
Year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
District controls	Yes	Yes	Yes

Notes. *Hit by hurricane* is a dummy equal to 1 if at least one county in the district was hit by a hurricane. *N. green bills* sponsored or cosponsored per year/district, as defined by the CBP. *Snow* includes snowfalls, freezings, and severe ice storms. *Non-hurricane related storm* includes severe storms and coastal storms not related to hurricanes. For a description of *Individual controls* and *District controls* see Table 3 in the main text. Standard errors clustered by state in parentheses. ***, **, *: denote significant at 1, 5, and 10 percent level, respectively.

Table A.5: Hurricanes and support for green bills - Spillover effects

	(1)	(2)	(3)
	N. green bills		
Hit by hurricane (t-1)	0.178***	0.173***	0.178***
	(0.059)	(0.059)	(0.059)
Adjacent district hit by hurricane (t-1)	0.053		
	(0.068)		
District in state hit by hurricane (t-1)		0.022	
		(0.079)	
Adjacent state hit by hurricane (t-1)			0.030
			(0.055)
Avg. outcome	1.053	1.053	1.053
N. year/districts	13,253	13,253	13,253
Year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes
District controls	Yes	Yes	Yes

Notes. *Hit by hurricane* is a dummy equal to 1 if at least one county in the district was hit by a hurricane. *N. green bills* sponsored or cosponsored per year/district, as defined by the CBP. *Adjacent district*, *District in state*, and *Adjacent state* are dummies equal to 1 if at least one adjacent district, or one non-adjacent district in the state, or one district in an adjacent state was hit by a hurricane (but not the district itself), respectively. For a description of *Individual controls* and *District controls* see Table 3 in the main text. Standard errors clustered by state in parentheses. ***, **, *: denote significant at 1, 5, and 10 percent level, respectively.

Table A.6: Logrolling and lobbyists' pressure

	(1)	(2)	(3)	(4)	(5)	(6)
			Contributions (log)			
	N. green bills		Green	Energy/ automotive	N. green bills	
Hit by hurricane (t-1)	0.194***	0.208***	0.112	-0.044	0.214***	0.255***
	(0.055)	(0.053)	(0.137)	(0.069)	(0.074)	(0.077)
Main effects						
N. green friends	-0.002					
	(0.018)					
N. green same state		-0.003				
		(0.021)				
Green contributions (log)					-0.013**	-0.011
					(0.005)	(0.007)
Energy/automotive contr. (log)					-0.043**	-0.029
					(0.017)	(0.018)
Other contributions (log)					0.039	0.033
					(0.035)	(0.040)
Green score (t-2)						0.304*
						(0.163)
Hit by hurricane (t-1) ×:						
N. green friends	-0.005					
	(0.021)					
N. green same state		0.006				
		(0.006)				
Green contributions (log)					-0.022	-0.040**
					(0.014)	(0.019)
Energy/automotive contr. (log)					-0.155***	-0.080*
					(0.051)	(0.047)
Other contributions (log)					0.172***	0.107*
					(0.057)	(0.056)
Green score (t-2)						0.134*
						(0.071)
Avg. outcome	1.082	1.082	1.565	9.671	1.067	1.097
N. year/districts	10,703	10,703	7,630	7,630	7,630	6,489
Congress members	All	All	Re-run	Re-run	Re-run	Re-run
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes. *Hit by hurricane* is a dummy equal to 1 if at least one county in the district was hit by a hurricane. *N. green bills* sponsored or cosponsored per year/district, as defined by the CBP. *N. green friends* is the number of representatives who graduated from the same university and whose LCV lifetime environmental score, as observed at $t-2$, was above the median of the corresponding year. *N. green same-state* is the number of representatives from the same state whose LCV lifetime environmental score, as observed at $t-2$, was above the median of the corresponding year. *Green contributions (log)* and *Energy/automotive contr. (log)* are the log of the yearly amount of campaign funds, in thousand USD, received from PACS classified as environmental or as energy and automotive industry by the CRP, both defined only if the incumbent is running for re-election (*Re-run*). *Other campaign funds (log)* is the residual of the campaign funds not classified as either green or oil. All continuous interaction variables are demeaned. *Green score (t-2)* is the LCV lifetime environmental score as observed at $t-2$, and it not available for rookies. Column (5) also controls for the interaction of *Green score (t-2)* with *Hit by hurricane (t-1)*. In all columns, *Controls* include the level of the corresponding interacted variable. For a description of *Controls* see Table 3. Standard errors clustered by state in parentheses. ***, **, *: denote significant at 1, 5, and 10 percent level, respectively.

Table A.7: Hurricanes and green bills' outcomes: reaching the floor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	N. green bills that reached the floor									
Hit by hurricane (t-1)	0.307	0.113	-0.009	1.582***	0.408	0.251	0.188	-0.441	0.961	0.918
	(0.695)	(0.745)	(0.714)	(0.574)	(1.203)	(0.654)	(0.952)	(0.791)	(0.843)	(1.925)
Hit by hurricane (t-1) ×:										
Unsafe district		0.713								1.936
		(1.162)								(1.757)
Tenure ≥ 5			0.795							0.314
			(0.649)							(0.752)
Republican				-2.341**						-1.175
				(1.049)						(2.455)
Centrist					0.005					-2.387*
					(1.089)					(1.265)
Green score (t-2)						1.036				1.420
						(0.667)				(1.492)
Unemp. rate (t-2) above median							0.393			2.486***
							(0.960)			(0.856)
Coastal								1.389		1.461
								(0.967)		(1.056)
Post-2007									-1.220	-1.781
									(1.453)	(1.591)
Avg. outcome	3.931	3.957	3.931	3.931	3.890	3.767	4.210	3.931	3.931	4.000
N. year/districts	13,253	13,067	13,253	13,253	13,137	10,990	11,544	13,253	13,253	9,424
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. *Hit by hurricane* is a dummy equal to 1 if at least one county in the district was hit by a hurricane. *N. green bills* sponsored or cosponsored per year/district, as defined by the CBP. For a description of all the interacted variables, see Table 5. For a description of *Individual controls* and *District controls* see Table 3.*Individual controls* also include *Green score (t-2)* in column (6). *District controls* also include *Unemp. rate (t-2)* in column (7), and *Coastal* in column (8). All continuous interaction variables are demeaned. Standard errors clustered by state in parentheses. ***, **, *: denote significant at 1, 5, and 10 percent level, respectively.

A.4 Electoral costs of green legislation

The analysis in the main text highlighted that congress members shielded from electoral competition are more likely to promote green legislation in the aftermath of a hurricane. This suggests that politicians are aware of the potential costs of green legislation, and promoting it may result in a weaker electoral showing and a decrease in other measures of support.

We therefore proceed to examine the relationship between green legislation and various measures of voter support and electoral outcomes. For this analysis, we collapse the original data at the Congress level and separate green bills sponsored or cosponsored before (or in the absence of) a hurricane from those sponsored or cosponsored after a hurricane. We then look at whether these two measures affect various measures of support.[12] The results are presented in Table A.8. We find that congress members who respond to a hurricane by supporting green legislation receive fewer campaign contributions. On the other hand, we find negative, but small and statistically insignificant effects on the share of votes in the subsequent election and in the probability of re-election, possibly because voting choices are based on a variety of issues, while campaign contributions come from a much smaller sample of voters and are possibly more sensitive to the public debate over single specific issues.[13] Finally, we also look at whether hurricanes affect the probability of running for re-election (column 4). We find that there is no such effect, even though members of Congress who sponsor or cosponsor more green bills (both before and after the occurrence of a hurricane) are more likely to run again.

[12] We exclude uncontested races and races where the incumbent is not running for re-election.

[13] The results documented in this table should be viewed with some caution as they do not necessarily represent causal effects. A politician's response to hurricanes may itself be affected by the expected vote share and re-election probability.

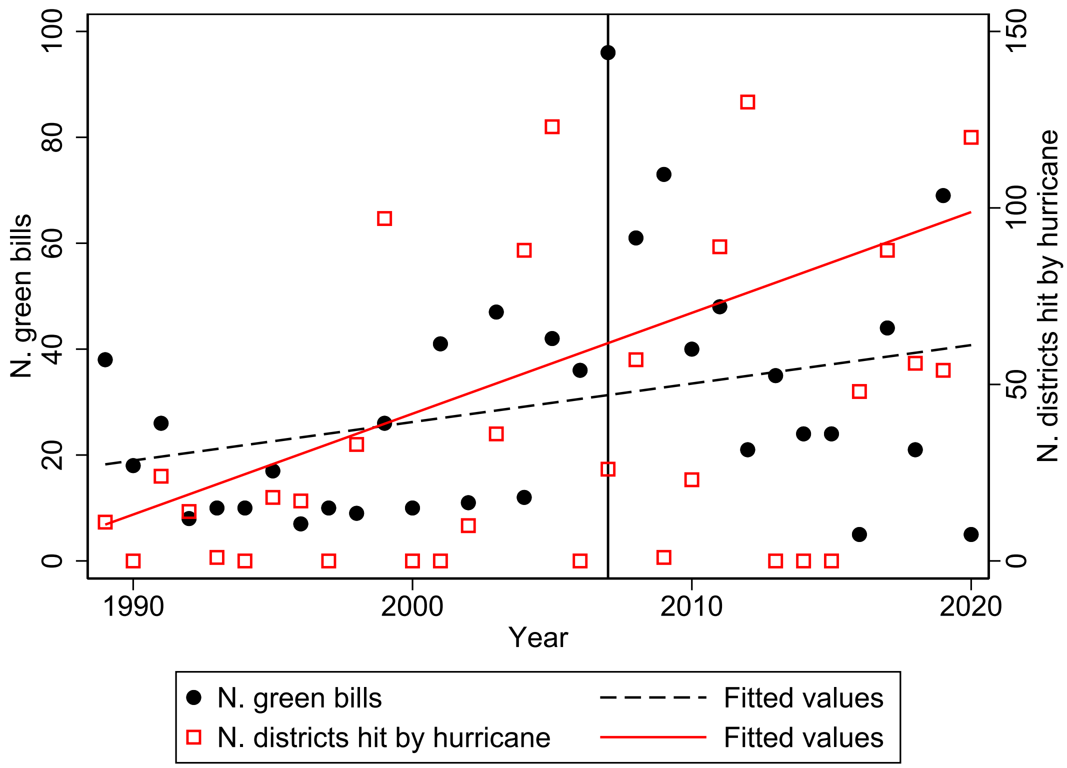
Table A.8: Support for green bills, campaign contributions and electoral outcomes

	(1)	(2)	(3)	(4)
	Individual contributions (log)	Perc. votes next	Re-election probability	Running for re-election
Hit by hurricane	0.105***	-1.167	0.068***	-0.037
	(0.038)	(0.994)	(0.021)	(0.031)
N. green bills before	-0.003	0.015	0.002	0.007**
	(0.005)	(0.095)	(0.001)	(0.003)
N. green bills after	-0.073***	-0.597	-0.008	0.055***
	(0.026)	(0.606)	(0.007)	(0.017)
Avg. outcome	12.33	65.61	0.944	0.806
N. congress/districts	3,793	3,951	3,998	4,966
Congress members	Re-run	Re-run	Re-run	All
Congress FE	Yes	Yes	Yes	Yes
State × Decade FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes

Notes. *Individual contributions* is the sum of direct contributions, earmarked contributions, or contributions through a joint fund raising committee to the candidate (in thousand USD), *Perc. votes next* is the vote share of the incumbent in the subsequent election, and *Re-election probability* is a dummy for the incumbent being re-elected for another term, all defined only if the incumbent is running for re-election (*Re-run*), except in column 4. *Running for re-election* is a dummy for the incumbent running for re-election. Inter-census races (Congress 102^{nd}, 107^{th}, and 112^{th}), and Congress 114^{th} excluded. *N. green bills before* and *N. relief bills before* is the number of green and relief bills sponsored or cosponsored per congress/district before (or in the absence of) a hurricane. *N. green bills after* and *N. relief bills after* equal to zero if no hurricane. For a description of *Individual controls* and *District controls* see Table 3 in the main text. Standard errors clustered by state in parentheses. ***, **, *: denote significant at 1, 5, and 10 percent level, respectively.

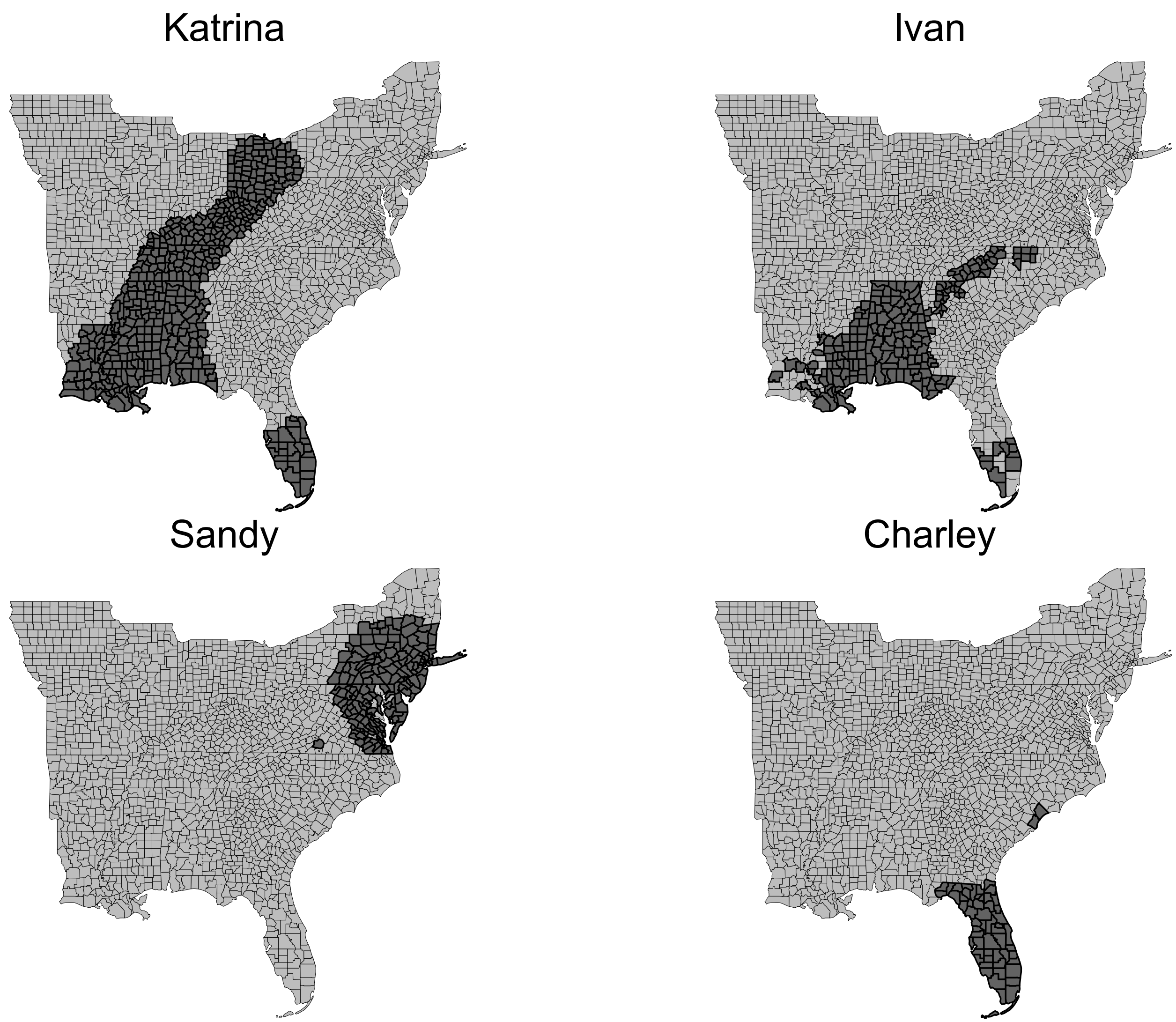
A.5 Additional Figures

Figure A.1: Hurricanes and green bills over time



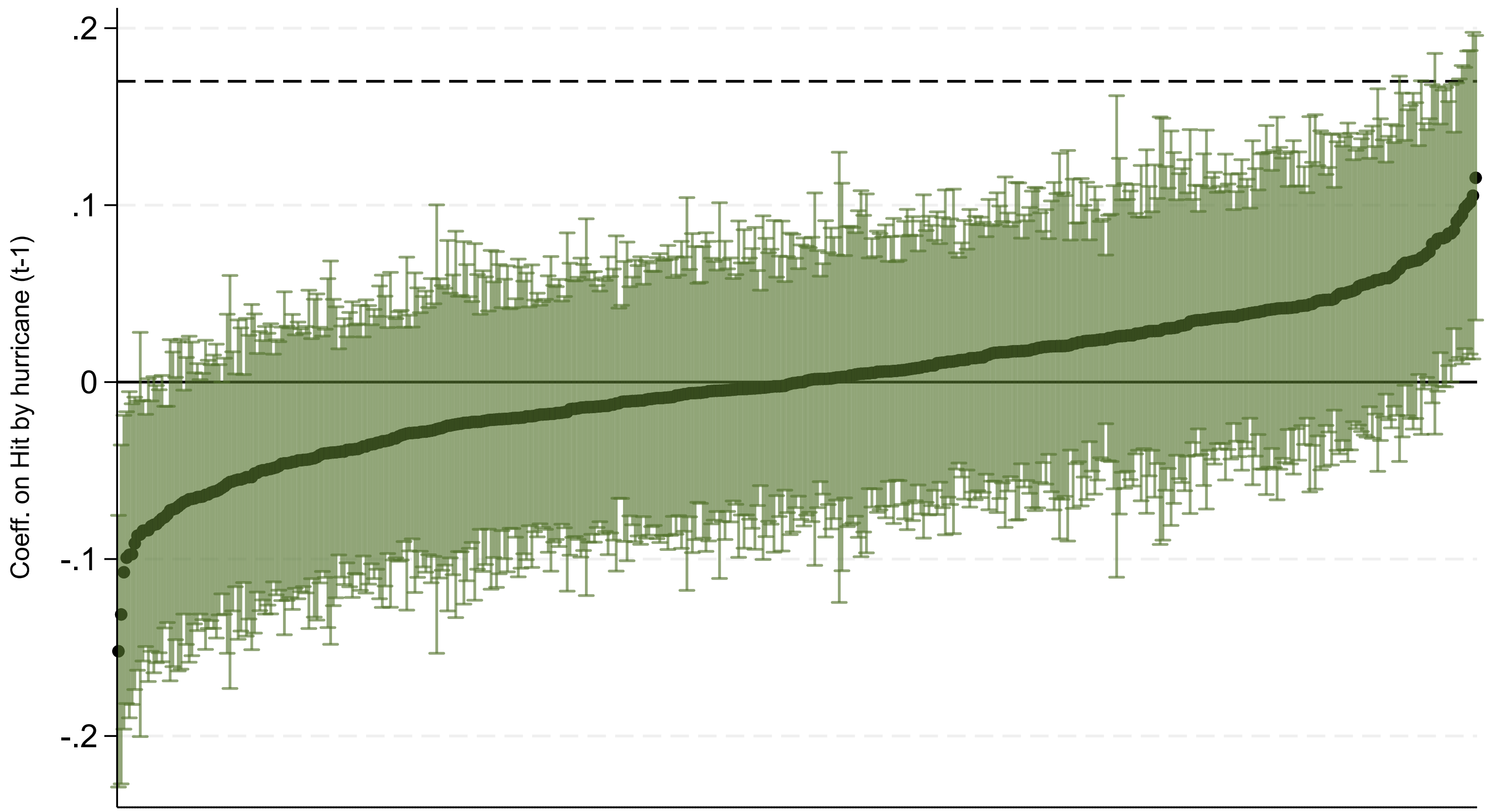
Notes. *N. green bills* sponsored or cosponsored per year/district, as defined by the CBP. The vertical line corresponds to the release of the 4$^{\text{th}}$ IPCC Assessment Report in 2007.

Figure A.2: Selected Hurricanes and counties included in FEMA declarations



Notes. Source: FEMA. Counties hit by a hurricane in dark green, excluding those hosting evacuees from Hurricane Katrina. Black lines denote state boundaries.

Figure A.3: Placebo on s.e. clustering



Notes. The figure displays 500 estimated placebo coefficients (randomly drawing, w/o replacement, the year of occurrence of a hurricane) with a 95 percent confidence interval (standard errors clustered by state) on the *N. green bills* sponsored or cosponsored per year/district, as defined by the CBP. Estimates include all the controls as in column (4) of Table 3. The dashed horizontal line represents our baseline estimate (0.172), as in column 4 of Table 3.

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B Event study analysis

In this appendix, we describe how we implemented the event-study analysis used in the construction of Figure 3 in the main text. Because of the particular structure of our data, we could not use any of the pre-existing packages for the event-study analysis (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; de Chaisemartin and d'Hautefœuille, 2020; Borusyak, Jaravel, and Spiess, 2022). Instead, we implemented our own code, which is an adaptation of the stacked-event study method that is suitable for our data.

B.1 Data structure

We start by introducing the concept of a *congressional decade*. Seats in the House of Representatives are reapportioned among the 50 states following the decennial Census.[14] After congressional seats are allocated, states can redraw the maps of congressional districts (the process of *redistricting*). A *congressional decade* is the 10-year period between the reapportionment of congressional seats. Because the Census is conducted in years ending in "0", and the apportionment and redistricting process typically takes about two years, the first year of a congressional decade is the year ending in "3". Our data, which spans between 1989 and 2020, therefore spans four congressional decades: 1983-1992, 1993-2002, 2003-2012, and 2013 to 2022.

The key observation is that congressional districts cannot be linked across congressional decades. This is obvious for states that gain or lose congressional seats; but it is also true for states that maintain the same number of seats, because the redistricting process means that the boundaries of congressional districts can (and do) change substantially across decades.[15]

As a result, our data should be viewed as *four* distinct panels of congressional districts. Available statistical software for the analysis of event studies does not easily accommodate

[14]For more details on this process, see "Apportionment and Redistricting Process for the U.S. House of Representatives," `https://www.congress.gov/crs-product/R45951`

[15]For example, the southern tip of the Delmarva peninsula was part of Virginia's first congressional district between 1993 and 2002, but became part of Virginia's second congressional district between 2003 and 2012. Specific places may also shift from one congressional district to another. For example, Montgomery, AL was part of Alabama's 2nd congressional district between 1983 and 1992, the 7th Congressional District between 1993 and 2002, and the 3rd Congressional District between 2013 and 2022.

this data structure. Therefore, we have implemented our own estimator, which still follows the principles of the recent literature on staggered difference-in-differences designs, but is suitable for our particular data structure.

B.2 Implementation of event-study analysis

For implementing the event-study analysis, we adopt a stacked difference-in-difference approach (Cengiz et al., 2019) that is suitable for our data structure. We proceed in the following steps. For each congressional decade d, we first keep only those districts hit by a hurricane no more than once in that decade. Let g_i be the year in which district i is hit by a hurricane. By convention, districts not hit by a hurricane in decade d have $g_i = \infty$. Let G_d be the set of all years within congressional decade d in which at least one district is hit by a hurricane. For each element g in G_d, we construct a separate sub-experiment, i.e., a separate difference-in-difference data set that includes only valid comparisons between treated districts and comparison districts that either are never treated, or are only treated after the focal cohort (that is, the cohort that is first treated at time g). This ensures that our difference-in-difference analysis does not include any "forbidden comparisons" between units that are treated late and those that are treated early (Goodman-Bacon, 2021). We consider two specifications for constructing the comparison group. In the *never-treated only* specification, the comparison group includes only those districts never hit by a hurricane within the congressional decade. In this specification, for each sub-experiment $g \in G_d$, we define the treatment indicator $D_{it,g}$ as follows:

$$D_{it,g} = \begin{cases} 1 & \text{if } g_i = g \text{ and } t \geq g \\ 0 & \text{if } (g_i = g \text{ and } t < g_i) \text{ or } (g_i = \infty). \end{cases}$$

In the *never-treated and not-yet-treated* specification, the comparison group for cohort g includes also those districts hit by a hurricane in congressional decade d, but not yet hit by a hurricane by year g. That is, the treatment indicator $D_{i,t}$ is defined as as follows:

$$D_{i,t} = \begin{cases} 1 & \text{if } g_i = g \text{ and } t \geq g \\ 0 & \text{if } (g_i = g \text{ and } t < g_i) \text{ or } (g_i > g \text{ and } t < g_i) \text{ or } (g_i = \infty). \end{cases}$$

We then stack together each different data set within a congressional decade d, and then stack together the four congressional decades in our sample. We define relative time $r_{it,g} = t - g_i$ for ever-treated districts, and $r_{it,g} = 0$ for never-treated districts. Our final sample consists of each of the different data sets, indexed by sub-experiment $g \in G = \bigcup_{d=1}^{4} G_d$.

In the *never-treated only* specification, the event-study graph plots the estimates of β_s, for $s = -5, ..., 5$ obtained by the following regression:

$$Y_{it,g} = \sum_{s=-6, s\neq -1}^{6} \beta_s D_{it,g} \times 1\{r_{it,g} = s\} + X'_{it,g}\gamma + \delta_{i,g} + \delta_{t,g} + u_{it,g},$$

where the subscript it, g denotes an observation in district i at time t and sub-experiment g. Note that the regression includes a full set of sub-experiment-by-district and sub-experiment-by-time fixed effects. We have verified that in the case of a single congressional decade, this strategy yields estimates that are numerically identical to those obtained by Stata's `stackedev` command.

In the *never-treated and not-yet-treated* specification, the regression equation is modified to also include the event-time dummies not interacted with the treatment indicator for the pre-treatment period:

$$Y_{it,g} = \sum_{s=-6, s\neq -1}^{6} \beta_s D_{it,g} \times 1\{r_{it,g} = s\} + \sum_{s=-6}^{-2} \alpha_s \times 1\{r_{it,g} = s\} + X'_{it,g}\gamma + \delta_{i,g} + \delta_{t,g} + u_{it,g}.$$

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C Text analysis of congressional speeches: technical appendix

C.1 Data

Our main data source is the open-access U.S. Congressional Record (`www.congress.gov/congressional-record`), which provides information on every single speech given on the floor of the U.S. House of Representatives and Senate. We focus on speeches delivered in the House between the 101st Congress (1989-1990) and the 113th Congress (2013-2014). Data on speeches include the Congress number, the date, the speech itself, a speech identifier, a speaker identifier, and the word count, for a total of 904,413 observations. We merge these data with information on roll-call votes (`www.congress.gov/roll-call-votes`) to retrieve the date when each bill that reached the floor was voted on.

C.2 Methodology

We adopt a combined *Supervised Machine Learning* and *Dictionary Learning* technique (Dun et al., 2020; Ke et al., 2020). The first method is used to teach the algorithm how to distinguish between speeches related to short-run costs or long-run benefits, adopting a secure training set. The second method is used to validate the first results in our set of interest, implementing both a close, external, dictionary and two open, newly generated ones (Eichstaedt et al., 2020).[16] The use of text as data in order to better analyze public policies and public agents is an established technique (Gentzkow et al., 2019a; Enke, 2020; Isoaho et al., 2021), and using dictionaries is commonly used in other similar studies (Gentzkow and Shapiro, 2010; Gentzkow et al., 2019b).

Using a training set to create a new dictionary is the recommended method when an already established dictionary does not exist, and the topic is very specific (Correa et al., 2017). A new dictionary is a set of words (or word roots) that are more relevant in a specific

[16] Alternatives to our combined method would have been to use just either *Supervised Machine Learning*, or a simple dictionary. However, the former method turned out to be time-intensive and low in accuracy, since political speeches tend to be full of nuances and references to peculiar cases; as for the second method, there was no ready-made dictionary available for our research question.

set of text data. We created a dictionary for words associated with short-run costs (SRC) and long-run benefits (LRB) using the *Bag of Words* method, which turned out to be both efficient and accurate.[17] In practice, we created the open dictionary using the Stata package `txttool` (Williams and Williams, 2014).

The empirical strategy is composed of three stages: a) identification of the training set; b) definition of the dictionaries from the training set; and c) validation of the hypothesis through dictionary learning.

Training set

Our approach to constructing a training set consists of identifying a number of bills related to environmental issues, extracting their relevant debate speeches, and labeling them. We start with the "green bills" (as defined in Section 2) that reached the floor for a roll-call vote. These bills are: HR1633-112, HR3030-101, HR3585-111, HR3644-111, HR3880-107, HR5325-112, HR6899-110, HR5534-109, HR6052-110, HR6190-112. We used the debate surrounding these bills as a training set of green speeches. We focused on all the speeches made on the day a bill first went to the floor for a roll call. We then eliminated speeches containing references to other bills discussed on that day or to other frequently discussed non-environmental topics (e.g., terrorism, Iraq, Vietnam, disability, etc.). This left us with 971 potential speeches from the debate surrounding these bills. We then had a research assistant read the full text of these speeches and determine whether it was indeed an environmental speech. After excluding some very short speeches (i.e., those with fewer than 200 words), we ended up with 584 speeches. From this set, we selected only those speeches that were likely to express more representative views on the subject, i.e., the ones made by the sponsor and the cosponsors, and the ones made by representatives voting against the bill but affiliated with their sponsor's party. We ended up with a training set of 162 speeches.

Once all the relevant speeches (by a sponsor, cosponsor, or an opponent from the same party) were extracted, we manually assigned to each speech a label of "short-run cost" (SRC) and/or "long-run benefit" (LRB). Examples of sentences that label the speech as SRC are:

[17] Alternative methods such as the *N-gram* or the *Term Frequency-Inverse Document Frequency* (*Tf-Idf*; Burnap and Williams, 2016) were less accurate.

"All economic impact studies show that between 400,000 and 4 million jobs are going to be lost. I think that is bad", "For every $1 million in regulations, it creates 1.5 jobs", "How can we justify increasing spending?". To be labelled as LRB, instead, the speech has to refer to some benefits that will arise later in time. Examples of sentences that label the speech as LRB are: "Better understanding of our air quality dilemma will invariably help us define appropriate remediation technologies", "Clean air has got to be our goal in this amendment", "We owe it to our constituents and our country to promote legislation that will stimulate the economy, which our environmental bills do, and protect and promote human health and the environment." These labels are not mutually exclusive, i.e., a training speech can relate to both concepts, or it might have no reference to them.

Dictionaries

This categorization process led to identifying 96 SRC and 111 LRB speeches, with some of the initial 162 speeches having no label, and some having both. We keep only speeches identified either as SRC or as LRB, for a total of 143 speeches. To identify the occurrences of each word in every speech, we use a *Bag of Words* technique. The algorithm selects the words that are most strongly associated with SRC and LRB speeches. Specifically, for each of the 4493 unique words identified in the set of speeches, we calculate Kendall's τ coefficient between an indicator for whether the word appears in the speech and the indicator for whether the speech had been identified as a SRC (or LRB) speech. We then retain only the words with $\tau > 0.25$. Both dictionaries are then tested through a linear discriminant analysis, giving significant results for both dictionaries. This test shows that the frequency distribution of SRC or LRB dictionary words in an SRC or LRB speech was not present in non-SRC or non-LRB speeches with 90% accuracy.[18] We finally removed words that are too general or too specific for the bills discussed in the training set, giving the following final dictionaries:

- Short Run Costs: busi*, cost*, creat*, critic*, job*, prevent*, process*, save, work*.

- Long Run Benefits: American*, develop*, final*, futur*, health*, nation*, past*, re-

[18] Other accuracy tests, like Principal Component Analysis, show similar results.

spons*, sourc*.

Test set

These two dictionaries are not necessarily ready to be implemented in a universal speech set because they originated from a training set of speeches related to environmental issues, and they are valid only in the same semantic space. To overcome this problem, all speeches are screened with an external dictionary aimed at identifying "environmental" speeches only. This stage is a pure dictionary learning process, implementing an external dictionary taken from the SMART vocabulary database of Cambridge Dictionary (`https://dictionary.cambridge.org/topics/earth-and-outer-space/environmental-issues/`). Such dictionary contains these words: environ*, climat*, sustaina*, pollut*, ecology*, energy*, ecosyst*, emission*, ecyclese*, ecycle*, renewabl*. To avoid coincidental speeches, we only defined as "environmental speeches" those containing at least two different words from this dictionary.

The test data set is composed of all environmental speeches given on the floor by representatives from districts hit by a hurricane, and does not include speeches from the training set. The final data set is made of every single environmental speech given by these representatives. To check for relevant speeches, we dropped speeches with fewer than 200 words and more than 5,000 words, which resulted in a final set of 14,273 speeches.

The main indicator we use to assess the magnitude of their awareness of short-run costs and long-run benefits is the count of all SRC or LRB words used by a single representative in any relevant "environmental" speech during the year. This indicator is similar to the one adopted in Gentzkow and Shapiro (2010, p.46) for their dictionary validation. For robustness analysis, we also use as an alternative indicator the share of politicians making speeches with at least 25% of the words contained in the SRC and LRB dictionaries.

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