The impact of climate disasters on climate action: Evidence from a natural experiment in Western Europe

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Abstract

Unexpected climate disasters have been shown to increase public concern for tackling climate change, but not change the attitudes of policy makers. This raises the question of what the overall impact of climate disasters are on mitigation laws and policies? This paper utilises a natural experiment to estimate the impact of extreme and unexpected disaster shocks on mitigation laws and policies for 17 Western European countries. Over the period from 1980 to 2020, we identify 1990 as a particularly extreme year of climate shocks for some countries in Western Europe. Using a simple difference in difference estimation, we show that these countries which experienced the extreme climate disaster shock in 1990 implemented less mitigation laws relative to the control group over the next 10 years. This suggests that while disasters may increase public concern for the environment, they do not necessarily translate into more mitigation laws.

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1 Introduction

Over the coming decades the world is set to experience more frequent and intense climate disasters (IPPC, 2022). Understanding how these disasters shape the incentives for climate action is central for the low-carbon transition. Do climate disasters act as a wake up call for mitigating climate change? The relationship between climate disasters and mitigation is not clear cut. On the one hand, becoming more vulnerable to climate change is likely to lead to an increase in damages and deaths from extreme weather events. Such extreme phenomena can act as “focusing events” (Birkland, 1998) that increase voters’ preferences for green policies (Andre et al., 2024; Hoffmann et al., 2022; Zelin and Smith, 2023). However, recent research shows that this change in voting behaviour does not necessarily lead to changes in the preferences of political parties (Wappenhans et al., 2024). Given these different impacts on the preferences of different actors (voters vs political parties), it is unclear what the overall effect on actual mitigation policies are.[1]

This paper aims to analyse the overall impact of unexpected climate disaster shocks on the number of mitigation laws implemented across 17 Western European countries. Over the period from 1980-2020 (for which we have data), we identify unexpected climate disaster shocks as any year with a particularly high frequency of climate-related disasters relative to what is normally experienced in each country. Only the most extreme disasters will act as focusing events, and so we consider the impact on policies of the most extreme events. Methodologically, we conduct a data-driven identification strategy for extreme climate weather events. We consider deviations (4+ standard deviations) from the country mean, rather than the number of disasters in each country, to avoid potentially confounding factors. For example, geographically larger countries will expect to have more disasters each year by their nature of being larger countries, alongside implementing more climate laws.

A surprising result of this data-driven identification strategy is that almost all the shocks occur in 1990.[2] 1990 was a particularly bad year for climate-related disasters in Western Europe. Over six major storms—Daria, Herta, Judith, Ottilie and Polly, Vivian, and Wiebke—hit the continent, causing historically high levels of damage.[3] These events represent a highly unusual concentration of extreme weather activity, with multiple countries experiencing disaster counts that are more than four standard deviations above their historical average. In the baseline,[4] we identify four countries—Luxembourg, Denmark, the Netherlands, and Finland—as having experienced an unusually high number of disasters in 1990.

These shocks were concentrated within 35 days[5] hence providing an experimental setting that is both

[1]One exception is Rowan, 2022 who analyses fixed effect regressions of climate disasters on overall mitigation policies and finds insignificant effects.

[2]Using the 4sd cutoff the only other shock over the 1980–2020 period is Sweden in 2005.

[3]See Koks and Haer. (2020), which uses OpenStreetMap data to confirm the exceptional nature of damages in 1990.

[4]In the baseline, the 4Std Dev cutoff is used, i.e., shocks are years when the number of climate disasters are four standard deviations above the country mean.

[5]See Appendix for details.

temporally sharp and plausibly exogenous, satisfying the conditions for a valid difference-in-differences design. As climate disasters of this magnitude are unpredictable and outside the scope of normal political planning, we treat the 1990 shock as an exogenous treatment affecting only the identified countries. We estimate the average treatment effect by comparing the cumulative number of mitigation laws passed in the treated countries to those in the control group of Western European countries that did not experience such an extreme shock. Pre-treatment trends in mitigation laws are parallel across treatment and control groups, providing support for the common trends assumption. Importantly, 1990 also precedes major international climate agreements (e.g., the 1992 UNFCCC and 1997 Kyoto Protocol), making it a natural focal point for evaluating how extreme events may have influenced the domestic climate policy trajectory during a critical formative period.

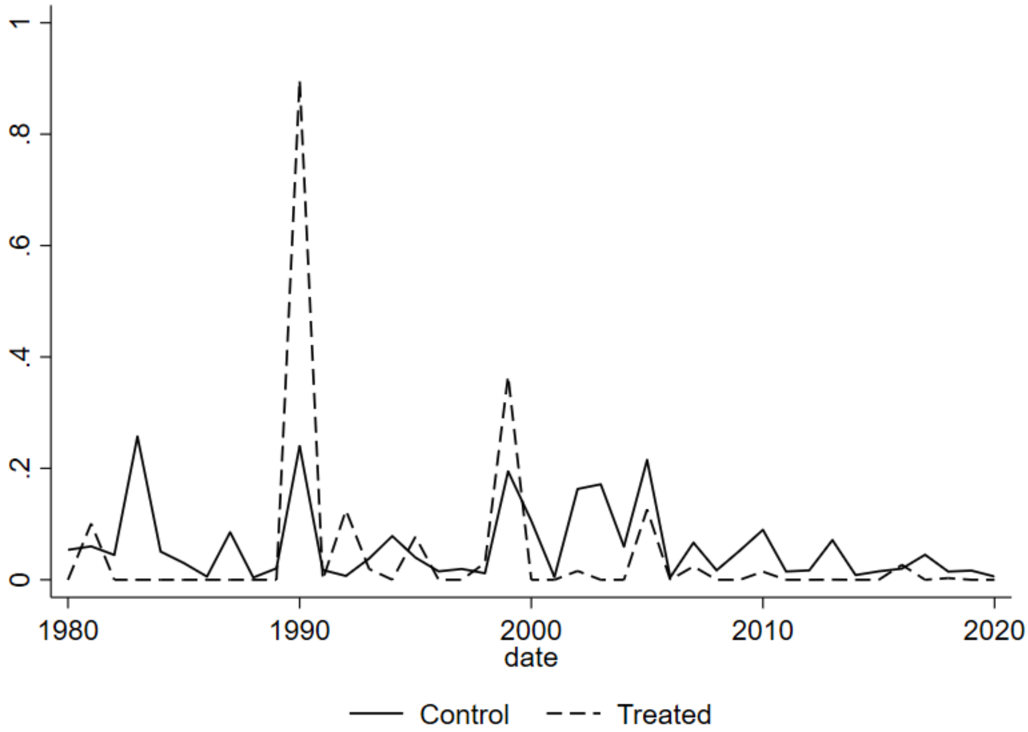
Figure 1 presents the damages to GDP in the treatment and control group respectively. The disasters in 1990 led to the largest increase in damages to GDP in the treatment group (nearly 1% of GDP). This was the highest ever damages in the treatment group over the whole period, and much higher than the damages in the control group. This is important as it suggests that our identification strategy of focusing on the number of extreme events also identifies periods of relatively high actual damages.

Using this natural experiment, we show that countries which experienced this unexpected climate disaster shock implemented less mitigation laws. Our findings show that the shock significantly reduces the cumulative number of mitigation laws in the immediate 10 years post-treatment period (1990-2000) by -0.303 compared to the control group. The average number of cumulative mitigation laws in this period across both groups is 0.295. This finding is robust to several alternative specifications. It suggests that while disasters may increase the environmental concern of voters, the actual legislative and policy impact of a disasters is a reduction in the number of mitigation laws.

One possible explanation for this is that unexpected disasters may favour a shift towards adaptation and away from mitigation. Adaptation offers more immediate and certain benefits, while mitigation requires global coordination and long-term commitment, often hindered by free-riding and collective action problems (Galanis, Ricchiuti, and Tippet, 2025). Moreover, post-disaster fiscal constraints—due to reconstruction costs and economic disruption—may lead governments to prioritise adaptation, crowding out mitigation efforts (S. Hsiang et al., 2017; Burke, S. M. Hsiang, and Miguel, 2015; Zhang et al., 2018; Shew et al., 2020). While we would like to test this mechanism directly, reliable cross-country data on adaptation laws are lacking: for the 1980–2000 period, the Climate Laws of the World database records just one adaptation law, making any formal estimation infeasible. Our interpretation remains one plausible explanation for the observed decline in mitigation efforts.

Section 2 outlines our argument and how it relates to existing literature. Section 3 discusses the data. Section 4 outlines the methodology and identification strategy. Section 5 presents the results and section 6 concludes.

Figure 1: Damages to GDP from Extreme Weather Events Treatment and Control Group (% of GDP)



Note: 1990 is identified as the only year where the number of climate damages 4 Std Dev above the country mean. Treated group = Finland, Netherlands, Luxembourg and Denmark. Control group = Austria, Belgium, France, Germany Ireland, Italy, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom.

2 Background

Climate disasters are known to promote pro-climate attitudes and voting behaviours. Utilising state level variation in temperature in the US, Bergquist and Warshaw (2019) find that climate concern at the individual level is modestly responsive to changes in state-level temperatures. Similar results for the US have been found by Egan and Mullin (2012) and Brooks et al. (2014) regarding temperature and Konisky, Hughes, and Kaylor (2016) for other extreme weather events. Globally, being more vulnerable to climate change, of which exposure to disasters is a key factor, is associated with more individual support for climate action (Andre et al., 2024). Exposure to climate disasters is also known to promote voting for pro-environmental parties and policies (Baccini and Leemann, 2021; Hoffmann et al., 2022). Combining climatological, survey and electoral data at the sub national level, Hoffmann et al. (2022) find a significant and sizeable effect of temperature anomalies, heat episodes and dry spells on voting for Green parties in the European Parliament. Similar findings are found by Baccini and Leemann (2021) in Switzerland on voting behaviour.

The key explanation for these findings is that climate disasters have the ability to act as "focusing events", that make future risks concrete alongside helping pro-environmental groups implement political change (Birkland, 1998), particularly in the face of better resourced fossil fuel interest groups (McAdam, 2017). However, even if climate disasters influence public opinion and voting behaviour, this influence does not necessarily translate into legislative and policy change. Recent work suggests that climate disasters do not shape the preferences of policy makers (Wappenhans et al., 2024).

Given these competing actors, this paper therefore aims to empirically estimate what the overall impact of disasters are on mitigation laws and policies. This is the first paper to utilise a natural experiment framework to test this question, Rowan (2022) using fixed effect regressions find insignificant effects, although they do not use a causal inference framework.

3 Data

For climate related disasters, we use the Emergency Events Database (EM-DAT). Following the IMF, we define a climate related disaster to be any disaster from drought, extreme temperature, flood, landslide, storm and wildfire. We take a count of the total number of annual natural disasters within a country. To report a disaster in the EM-DAT database, there must be at least one of the following criteria: ten or more killed, 100 injured or more, a state of emergency declared, or a formal request for international assistance. This relatively high bar is important for our analysis, given that we only want to identify the most extreme disasters that have the ability to act as focusing events to shift policy and laws. One limitation of the EM-DAT database is the under-reporting of disasters. Under-reporting however is primarily an issue outside of Western Europe, in countries without the infrastructure needed to report and investigate such events. Moreover, the most severe disasters (with which this paper is concerned) are likely to be those captured in the database. Following the IMF, we use data from 1980 to 2020. In the appendix, Figure A1 shows the average number of disasters each year for each country.

Domestic climate mitigation laws and policies are from the Climate Change Laws of the World database. For our baseline definition of mitigation laws (policies) we consider laws (policies) that only include a mitigation aspect (i.e. do not also include adaptation, disaster response etc), although we broaden this definition for robustness tests. Figure A2 presents the average number of mitigation laws and policies for each country.

National climate legislation has been shown to have a significant negative impact on both consumption and production emissions (Sam, 2023). To show that domestic mitigation laws for our sample is a decent proxy for effective mitigation policies, we estimate a fixed effects regression of the frequency of domestic mitigation laws on GHG emissions (annual GHG emissions in tons CO2 equivalent) over the 1980 to 2020 period in Table A2 in the appendix. Both the contemporaneous and the first lag of mitigation laws are significant associated with a reduction in GHG emissions. One law reduces -10.04m tons annual GHG emissions in C02 equivalents contemporaneously, and -9.46m tons in after 1 year.

Following the existing literature, we control for fossil fuel rents (the sum of coal, oil and gas rents as a share of GDP from the World Bank), GDP per capita (2017 PPP dollars $1000s from World Bank), population (millions people from World Bank) and a measure of Democracy (University of Gothenburg's Varieties of Democracy database) and the left-right political orientation of the chief minister (Peterson et al., 2023; Tørstad, Sælen, and Bøyum, 2020; de Silva and Tenreyro, 2021). Countries endowed with fossil fuels are less willing to take action given the significant economic costs of transitioning away from fossil fuels (Brulle, 2018; Dolphin, Pollitt, and Newbery, 2020; Victor, Lumkowsky, and Dannenberg, 2022), while countries with more democratic political institutions less susceptible to the influence of special interests are more likely to take action (Finnegan, 2022; Keohane, 2001; Bättig and Bernauer, 2009).

The paper focuses on countries in Western Europe for two reasons.[6] Firstly, by limiting the sample to Western European the treatment and the control groups are likely to be similar, as countries share similar political, economic and geographical factors which may influence attitudes to climate change. Secondly, climate change was a politically salient issue in Western Europe over the period of analysis, for example all countries were signatories of the 1997 Kyoto agreement - the first major international agreement on climate change.

[6] We define Western Europe geographically to include Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Luxembourg, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland and the United Kingdom

4 Identifying the impact of extreme climate related disasters

The empirical literature on the impacts of climate disasters tends to exploit exogenous variation in disasters over time within a country or region, to causally identify effects on key economic outcomes (see Dell, Jones, and Olken, 2014 for a literature review). Identification requires analysing the within variation, given that cross sectional variation in disasters is likely to be determined by other factors that are correlated with key variables of interest (i.e. larger countries have more disasters and more laws).

Climate disasters will only impact climate legislation if they are outside of the normal experience of the country. If a climate disaster falls within what is normally experienced within a country, it is less likely to focus attention in a way that can actually change public attitudes and by extension climate legislation. While several papers use a fixed effects panel regression to estimate the effects of climate disasters on economic outcomes (Dell, Jones, and Olken, 2014; Rowan, 2022), such models do not demarcate between normal and extreme shocks, as all variation from the mean is included in the estimation. It is not clear how normal shocks would act as “focusing events" that shift attention as discussed in the theoretical literature. In other words, the shocks need to be sufficiently out of the ordinary for the story to make sense. To deal with this issue, we identify extreme shocks as cases where there is a high number of climate related disasters in a given year relative to the country average. Such a shock is highly exogenous as countries may anticipate a certain number of climate related disasters year on year, and may plan for these, but significant deviations from the average are impossible to predict, particularly given the uncertain nature of extreme climate related disasters.

The question then is where should the line be drawn that demarcates a significant deviation from the mean? The existing literature provides some indication of what can be defined as an extreme event. Rahmstorf and Coumou (2011) argues that an extreme event can be defined as those that are more than four standard deviations above the mean, while Chaurasia, Verma, and Sinha (2020) use a 10 standard deviation cutoff. We take the lower value of these (4 standard deviations) as our baseline but consider higher cutoffs as robustness tests.

Figures 2 and 3 shows the frequency of climate related disasters (solid line) and the cutoff of four standard deviations above the average (dashed line) in each country. Figure 2 presents the countries where the number of disasters is more than 4 Std Dev in at least one year (our treatment group), while figure 3 presents the countries which never pass the cut-off point (our control group). A surprising result is that all the shocks occur in one year - 1990. 1990 was a particularly bad year for disasters in Western Europe: over 6 storms hit the continent that year (Daria, Herta, Judith, Ottilie and Polly, Vivian, Wiebke). While no other paper has used this 1990 shock in an identification strategy, Koks and Haer. (2020) shows that these storms in 1990 caused historically high damages using an alternative dataset (OpenStreetMap data), showing that this coincidence is not being driven by a data colletion issue in the EM-DAT dataset.

Given that the identified shocks all occur in one year, it is possible to estimate a simple difference

Figure 2: Frequency of climate disasters and 4 standard deviation cutoff - Treatment Group

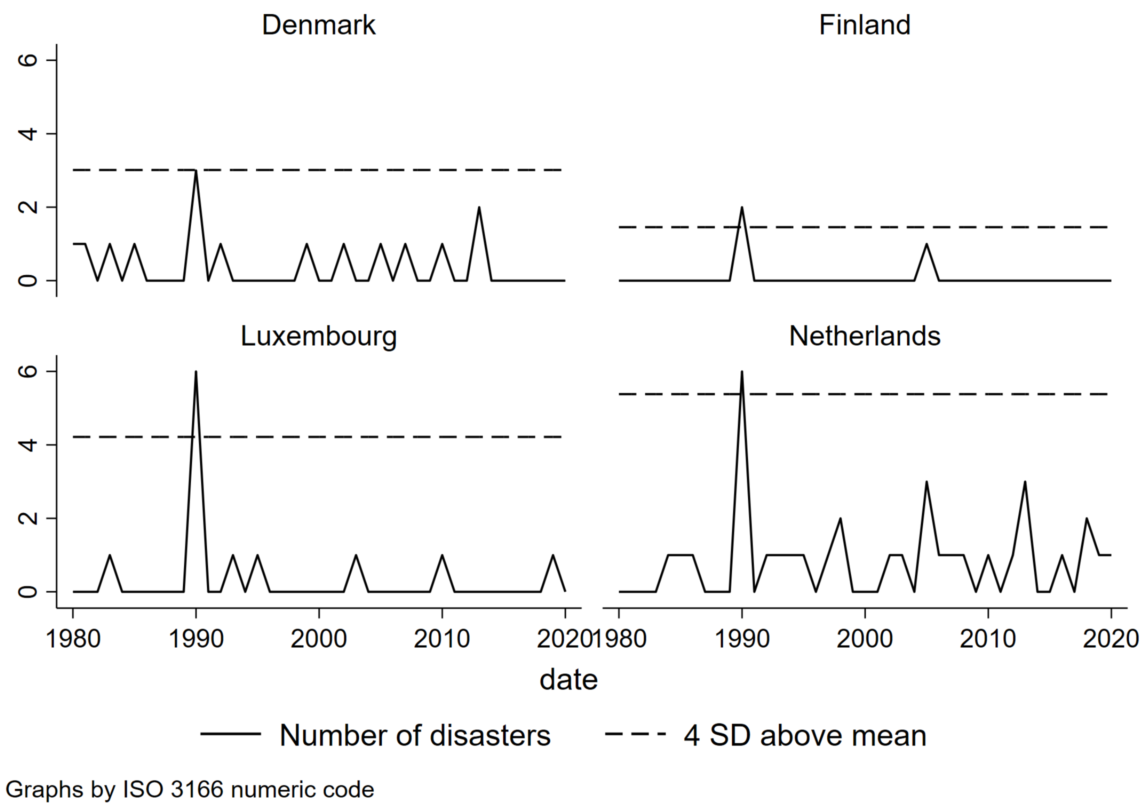
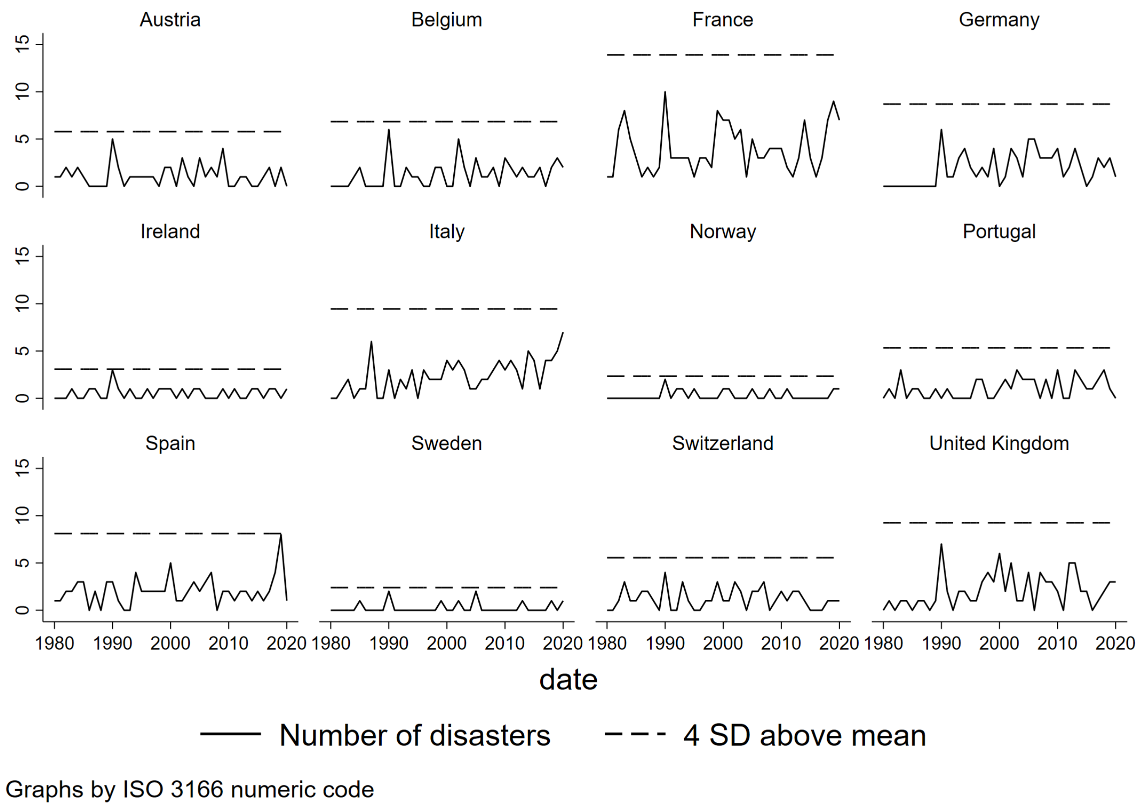


Figure 3: Frequency of climate disasters and 4 standard deviation cutoff - Control Group



in difference where the treated units are all treated at the same time. The treatment group consists of Finland, Netherlands, Luxembourg and Denmark. The control group consists of the other countries in Western Europe that do not experience such a significant shock: Austria, Belgium, France, Germany, Ireland, Italy, Norway, Portugal, Spain, Sweden, Switzerland and the United Kingdom.

As a robustness test we also present results using cutoffs above 4 Std Dev above the mean. Table A4 shows the shocks when 5 Std Dev cutoff is used. Only Finland and Luxembourg in 1990 are identified. When 6 Std Dev cutoffs are used, no shocks are identified.

Equation 1 outlines the baseline specification to be estimated

$$freqlaw_{it} = \beta_0 post_t + \beta_1 treatment_i + \beta_2 interaction_{it} + \beta_3 C_{it} + FE_i + T_t + e_{it}, \tag{1}$$

where $freqlaw_{it}$ is the cumulative number of either mitigation climate laws since 1950,[7] $post_t$ is a dummy that equals 1 if year is 1990 or after, $treatment_i$ is equal to 1 if country is Finland, Netherlands, Luxembourg and Denmark, $interaction_{it}$ is the interaction between the treatment and the post dummy, C_{it} is a vector of control variables including fossil fuel rents as a percentage of GDP, population, the left/right leanings of the government and the level of democracy, FE_i captures the country fixed effects and T_t is the global shocks. Global shocks include the major international agreements (Kyoto 1997, Copenhagen 2008 and Paris 2015), given that all countries in the sample are signatories of these agreements. We use the cumulative frequency of laws in order to properly see the overall effect of the shock on the trends pre and post the 1990 shock. We employ the cumulative number of mitigation laws as our primary outcome variable, rather than the annual frequency, for both empirical and theoretical considerations. First, the legislative process is characterized by persistence and path-dependence; policy responses to exogenous shocks such as climate disasters are often delayed and dispersed across multiple years. Consequently, annual frequency measures are likely to understate the medium-run policy response and generate excessive measurement noise due to the sparsity of climate-related legislation within single-year intervals. Second, the cumulative specification mitigates concerns related to zero-inflated outcomes and high year-to-year variance, which can bias inference in standard linear panel models (see Figure 4). Third, from an identification perspective, the cumulative formulation enhances the interpretability of the treatment effect in the difference-in-differences setting, allowing us to recover the integrated effect of the disaster shock over the post-treatment horizon. Finally, we validate this choice by conducting robustness checks using count models (Poisson fixed-effects) based on annual frequency, confirming the direction and significance of the main result.

We consider the the period from 1980 to 2000 for the baseline regressions. This is because the impact of a disasters on mitigation laws should occur within a relatively short time horizon. For example, it is not reasonable to assume that a shock in 1990 should have an impact on mitigation laws much beyond 10 years into the future. As a robustness test we also consider a longer time horizon of 20 years in the

[7] We define a mitigation law to be a law in the database that is only classified as mitigation. For example, laws that are passed for both mitigation and adaptation are ignored in order to identify the effect of shocks on the type of law.

future.

Damages to GDP is another potential variable to capture the impact of extreme weather events. However, as damages depends on several potential confounding factors (such as adaptation investments in the past, the GDP of each country etc), such an analysis would suffer from potential endogeneity. The frequency of climate disasters is not determined by the policies of the specific country, beyond the long term effect that global mitigation has on the overall global frequency of climate disasters. It is therefore a highly exogenous variable, unlike damages to GDP. That said, the identified shocks in 1990 did cause unprecedentedly high damages to GDP in the treatment group (see Figure 1.

5 Results

5.1 Baseline results

Figure 4 presents the change in the cumulative number of mitigation laws between the treated and control groups. As discussed above, the climate disaster shock occurs in 1990 which is indicated by the dark vertical line. The parallel trends assumption holds for the pre-treatment period, as both groups have the same flat trends. In 1990, the control group starts to implement mitigation laws, while the treated group does not implement laws until the 1997[8]. The parallel trends assumption is that both groups would have followed the same trajectory had the treated group not suffered from the disaster shock in 1990.

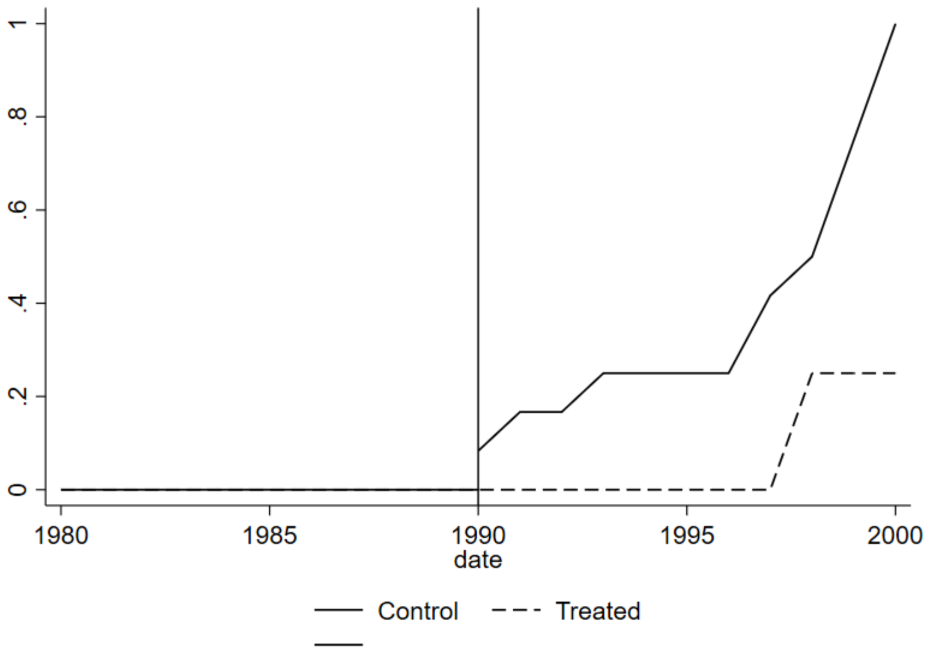
Column 1 of table 1 presents the coefficient and significance level of the treatment effect (i.e. β_2 in equation 1), without controlling for potential confounding factors. The treated group had -0.303 fewer cumulative mitigation laws implemented in the post treatment period (1990 to 2000) than the control group. The average number of cumulative mitigation laws in this period across both groups is 0.295, and therefore an average reduction of around -0.303 is larger than the mean.

Column 2, both in tables 1 includes control variables. The effect of disasters on the number of mitigation laws remains significant with the same sign, although the magnitude of the effect on the number of mitigation laws is relatively smaller (-0.297) and only significant at the 10% level. None of the controls are significant, and we therefore drop these for further specifications.

Lastly, column 3 changes the dependent variable to include the cumulative frequency of laws and policies, in order to capture a potentially broader definition of mitigation action beyond just laws. As can be seen, the shock negatively impacts the number of mitigation laws and policies (by -0.606) and this remains significant at the 10% level. The average number of mitigation laws and policies implemented each year for all countries in the post treatment period was 0.705, and so the effect is slightly smaller than the average.

[8] The increase in mitigation laws for both the treatment and the control group in 1997 is driven by all countries signing the 1997 Kyoto Accords

Figure 4: Impact of climate disaster shock on cumulative no. mitigation laws



Note: 1990 is identified as the only year where the number of climate damages 4 Std Dev above the country mean. Treated group = Finland, Netherlands, Luxembourg and Denmark. Control group = Austria, Belgium, France, Germany Ireland, Italy, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom.

Table 1: The impact of climate disaster shock on frequency mitigation laws

	(1) Cum Freq Laws	(2) Cum Freq Laws	(3) Cum Freq Laws & Policies
Interaction	-0.303**	-0.297*	-0.606*
	(0.026)	(0.075)	(0.089)
Controls			
Fossil rents/GDP		-0.002	
		(0.941)	
Population		-0.000	
		(0.515)	
Left-right index		-0.033	
		(0.412)	
GDP/capita		-0.000	
		(0.532)	
N	336.000	336.000	336.000

P-value in parentheses | * 0.10 **0.05 ***0.01 | Country and Year fixed effects are included | 1980-2000

5.2 Robustness tests

We perform seven robustness tests, with the first 6 estimation results presented in table 3. We do not include control variables in the robustness test as they are insignificant. The first robustness test changes the time frame to include a longer period going from 1980 to 2010, rather than our initial shorter period from 1980 to 2000. Using a longer time period than 2010 does not make much sense, as we should not expect a shock in 1990 to have an impact on laws more than 20 years into the future. As can be seen the coefficient on the interaction term remains significant at the 5% significant level.

The second robustness test changes the identification strategy. It identifies the extreme weather shock as years where there are more than 5 Std Dev above the country mean. Table A4 lists these shocks by country and year. Using the 5 Std Dev cutoff decreases the number of countries that experience a shock to just Finland and Luxembourg. Figure A3 in the appendix presents the plots of the treated and control group. Columns 2 of table 3 presents the results. As can be seen, the coefficient is -0.338, almost the same as when the 4 Std Dev cutoff is used, and is significant at the 1% significant level.

The third robustness test changes the identification strategy so that the cutoff is 3 standard deviations above the mean. Table A5 lists these shocks by country and year. Sweden is dropped from the sample as it experiences two shocks over the time period. All the other shocks also occur in 1990, however when defined in this way, the coefficient becomes insignificant. We interpret this results as suggesting that the shocks need to be sufficiently strong enough in order to have the impact on mitigation laws.

The fourth and fifth robustness test estimates a two-way fixed effects regression equation, given its use in the empirical literature. Given that we are now estimating the impact of multiple shocks over many years, we change the dependent variable to the frequency (rather than the cumulative frequency) of laws. This is different to the diff in diff, where there is one shock and we want to understand its total cumulative effect. Given that the dependent variable is a count variable with many potential zeros, we two types of fixed effects regression, a Poisson fixed effects regression (columns and a zero-inflated negative binomial regression. In the Poisson regression we have the dependent variable as the frequency of laws implemented in each period rather than the cumulative number of laws in order to not add a trend to the data. We include 5 lags given the potential in the baseline for the shock to have an impact on laws into the future. Column 4 and 5 presents the results. The fifth lag is significant and negative at 5% level for both estimations. All other lags are insignificant. Increasing the number of climate disasters a country experiences by 1, leads to a decline in the number of climate laws passed by -0.238/-0.266 after 5 years. The number of observations increases for this specification as we estimate this now over the whole period from 1980 to 2017, as we include all extreme weather events and not just extreme shocks. While these effects are more muted, there is still some evidence that any shock can lead to a decline in the number of mitigation laws (Dell, Jones, and Olken, 2014).

In column 6 we keep the baseline identification strategy but we change the dependent variable to be the frequency of mitigation laws, rather than cumulative frequency. As can be seen, the coefficient on the interaction term remains significant and negative at the 5% level.

The final set of robustness checks is presented in Table 4, which estimates a placebo test by varying the start year of the treatment from 1990. Specifically, column 2 examines the effect of a placebo shock beginning in 1992 on the cumulative number of mitigation laws, column 3 considers the impact if the shock commenced in 1993, and so forth. The primary objective of this placebo test is to assess whether the observed impact of the 1990 shock genuinely drives the results. If the placebo tests (columns 2 to 7) yield statistically significant coefficients, it would imply that the 1990 shock may not be responsible for the observed differences between the treated and control groups.

The results yield two notable observations. First, the dependent variable used in these tests is the cumulative number of laws and policies enacted. Second, the coefficients become statistically insignificant after 1993, with the pattern of insignificance persisting through 1996. While the results suggest that 1990 is not the sole year exhibiting significance, they also indicate that the further the hypothetical shock deviates from 1990, the less significant the coefficients become. This finding underscores that the 1990 shock likely captures a genuine effect, as demonstrated through the placebo tests.

To summarise, we find that unexpected climate disasters significantly reduce the number of climate mitigation laws implemented. This result is robust across several identification strategies and controls. One possible explanation is a shift in legislative priorities: disasters may heighten public pressure for action, but this pressure often favours adaptation over mitigation. Adaptation offers more immediate and certain benefits, while mitigation requires global coordination and long-term commitment, often hindered by free-riding and collective action problems (Galanis, Ricchiuti, and Tippet, 2025). Moreover, post-

Table 2: Robustness 1: The impact of climate disaster shock on frequency mitigation laws

	(1)	(2)	(3)	(4)	(5)	(6)
main						
r1vs0.treat_post	-1.051***	-0.338***	0.205			-0.068**
	(0.001)	(0.003)	(0.228)			(0.032)
No. Disasters t-1				-0.006	-0.018	
				(0.949)	(0.856)	
No. Disasters t-2				0.065	0.073	
				(0.508)	(0.464)	
No. Disasters t-3				0.098	0.093	
				(0.317)	(0.352)	
No. Disasters t-4				0.128	0.144	
				(0.186)	(0.156)	
No. Disasters t-5				-0.238**	-0.266**	
				(0.022)	(0.028)	
N	496.000	336.000	315.000	576.000	576.000	336.000

P-value in parentheses | * 0.10 **0.05 ***0.01 | Country and Year fixed effects are included | 1980-2000

Table 3: Robustness 1: The impact of climate disaster shock on frequency mitigation laws

	(1)	(2)	(3)	(4)	(5)	(6)
r1vs0.treat_post	-0.893**	-0.338***	0.205			-0.011
	(0.011)	(0.003)	(0.228)			(0.750)
No. Disasters t-1				-0.006	-0.018	
				(0.949)	(0.856)	
No. Disasters t-2				0.065	0.073	
				(0.508)	(0.464)	
No. Disasters t-3				0.098	0.093	
				(0.317)	(0.352)	
No. Disasters t-4				0.128	0.144	
				(0.186)	(0.156)	
No. Disasters t-5				-0.238**	-0.266**	
				(0.022)	(0.028)	
N	496.000	336.000	315.000	576.000	576.000	336.000

P-value in parentheses | * 0.10 **0.05 ***0.01 | Country and Year fixed effects are included | 1980-2000

Table 4: Placebo test: impact of shock on cumulative laws and policy with different shock start dates (1990 to 1996)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Start treatment year	1990	1991	1992	1993	1994	1995	1996
Interaction	-0.606*	-0.619*	-0.611*	-0.615	-0.595	-0.586	-0.590
	(0.089)	(0.089)	(0.099)	(0.116)	(0.152)	(0.203)	(0.269)
N	336.000	336.000	336.000	336.000	336.000	336.000	336.000

P-value in parentheses | * 0.10 **0.05 ***0.01 | Country and Year fixed effects are included

disaster fiscal constraints—due to reconstruction costs and economic disruption—may lead governments to prioritise adaptation, crowding out mitigation efforts (S. Hsiang et al., 2017; Burke, S. M. Hsiang, and Miguel, 2015; Zhang et al., 2018; Shew et al., 2020).

Case studies support this interpretation. For example, following Hurricane Sandy, climate policy in New York City shifted from decarbonisation to defensive adaptation (Cohen, 2021; McCraine and Surminski, 2019). Similarly, Giordono, Boudet, and Gard-Murray (2020) show that after 15 extreme weather events in the US, only adaptation policies were enacted at the local level. While we would like to test this mechanism directly, reliable cross-country data on adaptation laws are lacking: for the 1980–2000 period, the Climate Laws of the World database records just one adaptation law, making any formal estimation infeasible. Our interpretation remains one plausible explanation for the observed decline in mitigation efforts.

6 Conclusion

As climate disasters intensify, understanding how they influence mitigation efforts is crucial. While disasters often raise public concern, our findings suggest they may paradoxically reduce legislative action on mitigation. What impacts do climate disasters have on climate mitigation? Given expected increase in climate disasters, the IPCC has called for improved modelling on the relationship between impacts and mitigation arguing that a "necessary first step will be improved modelling of feedback from impacts, which is currently immature in most long-term global integrated assessment modelling" (Klein et al., 2007) [9]. The existing literature shows that climate disasters can promote public concern for the environment by focusing attention on the risks of climate change but this does not necessarily translate into changes in behaviour by policy makers (Wappenhans et al., 2024).

[9]See p. 756

Utilising a natural experiment, this paper analyses the effect of an unexpected and extreme climate disasters on the implementation of climate laws in Western Europe from 1980-2020. It is the first paper to analyse the impact of climate disasters on mitigation laws using causal analysis for a broader set of countries outside of specific extreme weather events in the US. Our findings show that a climate disaster shock significantly reduces the cumulative number of mitigation laws in the post treatment period (1990-2020) by -0.303 compared to the control group. As the average number of cumulative mitigation laws in this period across both groups is 0.295, the reduction of -0.303 laws is relatively larger than the mean. We show that unexpected extreme weather events can delay or suppress climate mitigation legislation—highlighting a troubling policy feedback loop in the face of rising climate risk.

This study raises several further research questions. Firstly, the findings are only for Western Europe and further analysis is needed to see if these findings hold for other regions of the world, where data is less comprehensive. Secondly, regional data could be utilised to see whether the same trade offs are found at the sub-national level. We have focused on the national level here given that this is where most of the legislative influence on climate change rests. Thirdly, a more qualitative case study approach could be used to see how these 1990 shocks influence policy makers in the treated countries. As climate disasters become more frequent, this research raises a sobering possibility: rather than galvanizing global mitigation, these events may deepen the adaptation–mitigation divide. This underscores the urgent need for institutional frameworks that can convert public concern into durable, forward-looking policy.

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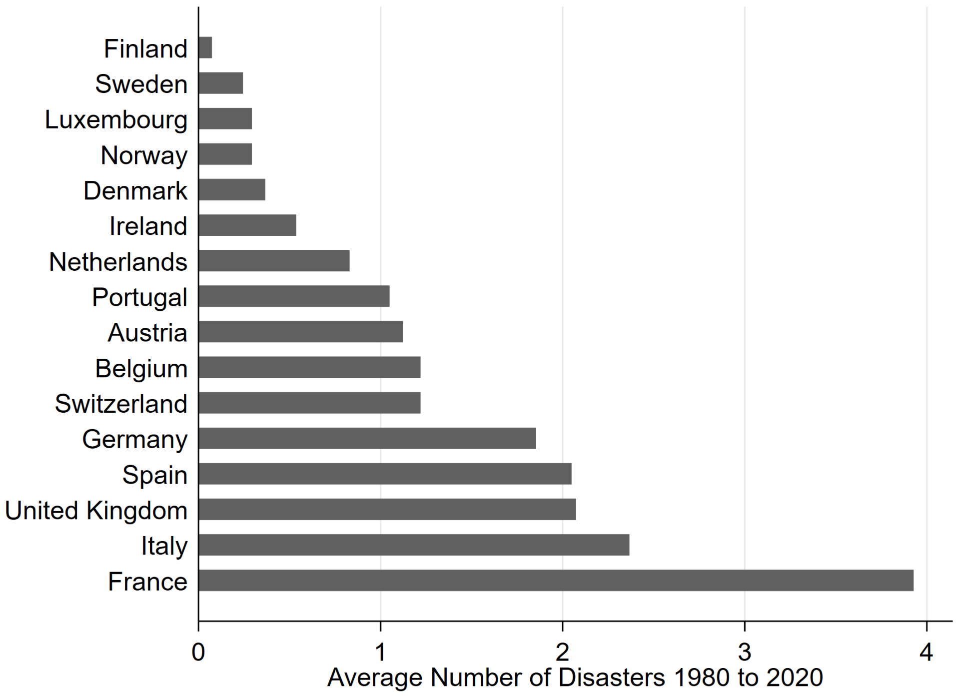
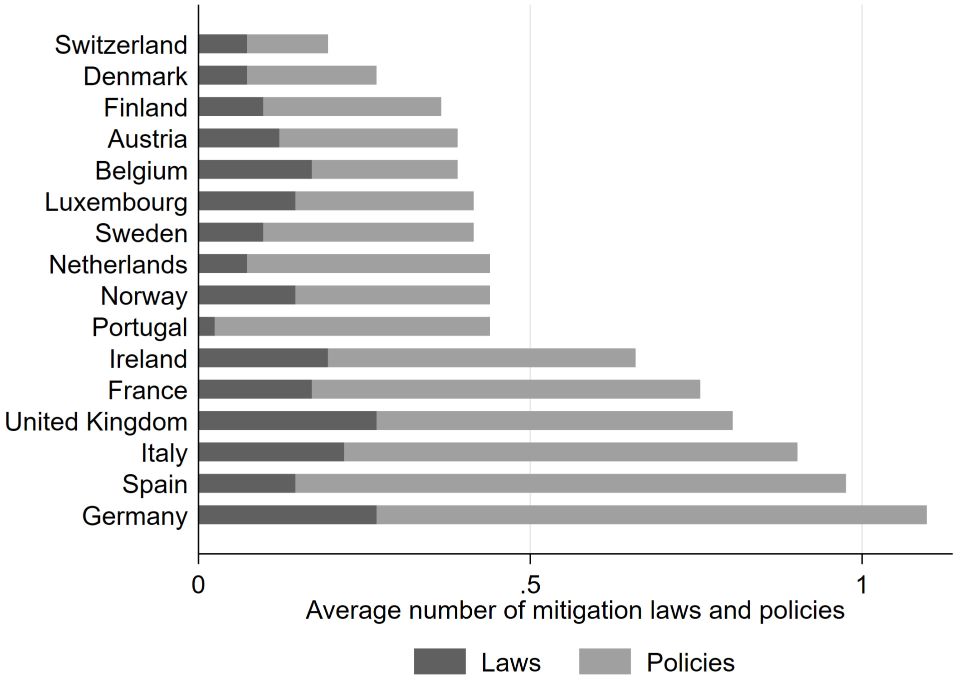
Appendix

Figure A1: Average number of climate related disasters 1980-2020



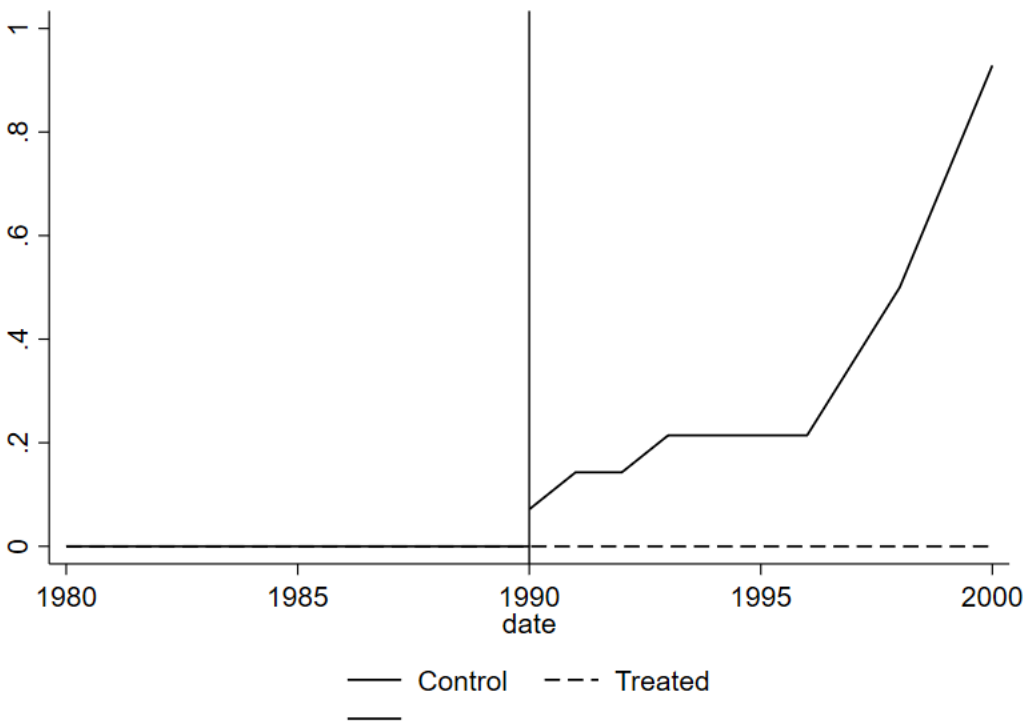
Note: Data from EMDAT database. Climate related disasters are defined as drought, extreme temperature, flood, landslide, storm and wildfire.

Figure A2: Average number of mitigation laws and policies 1980-2020



Note: Data from Climate Laws of the World Database.

Figure A3: Robustness: Impact of climate disaster shock on cumulative no. mitigation laws with 5 Std Dev cutoff



Note: Shock is identified as years where the number of climate damages 5 Std Dev above the country mean. Full list of shocks is in table A4.

Table A1: Major European Windstorms in 1990

Storm Name	**Dates**	**Main Countries Affected**	**Notable Impacts**
Burns' Day Storm (Daria)	25–26 Jan 1990	UK, Netherlands, Denmark, Luxembourg, Germany	Winds up to 230 km/h; 97 fatalities across Europe; severe damage to infrastructure and forests; widespread power outages.
Herta	1–6 Feb 1990	UK, France, Germany, Belgium, Netherlands	Significant wind damage; insured losses estimated at $1.5 billion (2012 USD); disruption to transport and utilities.
Judith	7–8 Feb 1990	Western Europe	Moderate storm with localized damage; limited widespread impact.
Nana	11–12 Feb 1990	Western Europe	Part of the 1990 storm sequence; caused minor structural and tree damage.
Ottilie	13–14 Feb 1990	Western Europe	Minor to moderate impacts; damage to roofs, trees, and power lines.
Polly	14–15 Feb 1990	Western Europe	Localized wind damage; part of prolonged storm activity in February.
Vivian	25–28 Feb 1990	Netherlands, Finland, Germany, Switzerland	Winds up to 268 km/h; 64 fatalities; extensive forest damage (esp. in Germany and Switzerland); transport disruptions.
Wiebke	28 Feb – 1 Mar 1990	Germany, Netherlands, France, Switzerland	Major forest destruction; severe economic damage; insured losses of $1.4 billion (2012 USD); followed closely after Vivian.

Sources: EM-DAT – The International Disaster Database; European Windstorm Database; ESWD – European Severe Weather Database

Table A2: The impact of mitigation laws on GHG emissions

	(1) Annual greenh b/p
No. Mitigation laws	-1.036e+07**
	(0.047)
No. Mitigation law 1	-9460582.247*
	(0.060)
No. Mitigation law 2	-9915163.278
	(0.153)
N	624.000

P-value in parentheses | * 0.10 **0.05 ***0.01 | Country and Year fixed effects are included

Table A3: Periods where frequency of disasters is more than 4 Std Dev above country mean

Country	Date	No. Disaster	Mean No. Disaster	St D No. Disaster
Denmark	1990	3	.372093	.6554989
Finland	1990	2	.0697674	.337734
Luxembourg	1990	6	.3023256	.9644856
Netherlands	1990	6	.8604651	1.125069

Table A4: Periods where frequency of disasters is more than 5 Std Dev above country mean

Country	Date	No. Disaster	Mean No. Disaster	St D No. Disaster
Finland	1990	2	.0697674	.337734
Luxembourg	1990	6	.3023256	.9644856

Table A5: Periods where frequency of disasters is more than 3 Std Dev above country mean

Country	Date	No. Disaster	Mean No. Disaster	St D No. Disaster
Sweden	1990	2	.255814	.5386502
Norway	1990	2	.3023256	.5133867
Austria	1990	5	1.116279	1.13828
Belgium	1990	6	1.255814	1.39886
Ireland	1990	3	.5348837	.6305265
Netherlands	1990	6	.8604651	1.125069
Denmark	1990	3	.372093	.6554989
Finland	1990	2	.0697674	.337734
Luxembourg	1990	6	.3023256	.9644856
Sweden	2005	2	.255814	.5386502
Spain	2019	8	2.069767	1.486372

Table A6: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Cumulative freq mitigation laws	777	1.796654	2.683443	0	15
Cumulative freq adaptation laws	777	.1827542	.4681188	0	2
Freq mitigation laws	777	.1441441	.4275558	0	4
Freq adaptation laws	777	.01287	.1236854	0	2
Freq mitigation laws and policies	777	.5160875	1.185201	0	11
Freq adaptation laws and policies	777	.0720721	.3328337	0	4
Frequency of disasters	736	1.164402	1.577524	0	10
Democracy index	777	.8656023	.0442886	.594	.924
Total rents/GDP	777	.5223525	1.665277	0	12.24815
Population	777	20741.84	24868.64	316.645	83160.87
Left-Right Index	721	-55.0749	231.9422	-999	3
Damages from disasters (%GDP)	777	.0518993	.196719	0	2.895422