

Who gets to adapt? Climate disasters, state capacity, and unequal adaptive governance

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Abstract

Climate change is increasing the need for adaptation, yet it remains unclear why climate disasters translate into adaptive governance in some countries but not others. This paper distinguishes two types of national climate lawmaking: adaptation, which responds to realized hazards, and mitigation, which reduces greenhouse gas emissions. Using national climate lawmaking as an indicator of adaptive governance, it asks how each responds to climate disasters and whether the response depends on institutional capacity. Using a country-year panel of 199 countries from 1995 to 2023, built by linking the Climate Change Laws of the World database with EM-DAT disaster records, we estimate Poisson models with country and year fixed effects and a distributed-lag design. A disaster is followed by about a 60 percent increase in adaptation lawmaking, cumulated over three years, while mitigation shows no clear response. The effect is robust across alternative estimators and specifications, is driven by disaster occurrence rather than severity, and fades within three years. It concentrates in countries with higher institutional capacity, particularly high-income countries and those with stronger rule of law, with little response where capacity is low. Disasters thus act as focusing events, but their translation into adaptive governance through national climate legislation depends on institutional capacity. Rather than generating adaptive governance uniformly, climate disasters reinforce existing inequalities in countries' ability to respond through legislation. This has direct implications for the adaptation finance gap and for inequalities in global climate governance.

1. Introduction

Climate change is, at its core, a problem of unequal protection. As climate-related hazards intensify, governments face increasingly difficult decisions about which populations and assets should be protected from floods, storms, droughts, and heat. These decisions are not merely political or administrative. Rather, they are implemented through legal and regulatory frameworks, including building codes, coastal defense policies, drought-management plans, and heat-action statutes. The capacity to enact and implement such legal measures is distributed as unevenly as exposure to climate hazards itself. Consequently, the question of who adapts cannot be separated from the question of which states possess the institutional and legislative capacity to respond. Understanding why some states convert climate shocks into protective legal and policy responses while others do not is therefore central to understanding adaptive governance under climate change. This is fundamentally a governance challenge at the center of the human dimensions of environmental change, and it motivates the analysis developed in this paper.

Sudden, visible, and costly disasters are widely regarded as the primary catalysts for policy change. Their immediate human and economic consequences create political pressure for governments to respond through legislative and institutional reforms. They are focusing events that concentrate attention and open windows for new legislation (Birkland, 1998; Kingdon, 1984). Yet whether these policy windows translate into adaptive governance remains far less clear. Governments can respond by adapting, through measures that protect the affected jurisdiction against harms it has already suffered, or by

mitigating, through measures that reduce greenhouse gas emissions driving future climate change. The two differ fundamentally in their political economy. Adaptation is close to a local good: it protects those who pay for it. Mitigation is a global public good whose benefits are shared worldwide and realized over decades, weakening any single government's incentive to act (Nordhaus, 2015). A national disaster sharply raises the returns to adaptation within the affected country while leaving the free-riding logic of mitigation largely untouched. Whether these incentives translate into enacted adaptation measures, however, depends not only on the policy window created by the disaster but also on governments' capacity to respond.

Yet a focusing event is only an opportunity. Converting it into enacted law requires administrative, fiscal, and institutional capacity that many of the most exposed states lack (Brooks et al., 2005; Eakin and Lemos, 2006). This raises a possibility with direct implications for climate justice. If only higher-capacity states translate disasters into protective legislation, then disasters may reproduce, rather than narrow, the global gap between those who can adapt and those who cannot. The countries with the greatest need would be precisely those least able to legislate their way to protection. This suggests that climate disasters may reinforce, rather than reduce, inequalities in adaptive governance by conditioning who is able to transform climate shocks into institutional responses. This is a pattern that the adaptation finance literature anticipates but that has not been tested through evidence on national adaptation lawmaking (UNEP, 2024; Barrett, 2013). Understanding whether disasters widen these governance inequalities is therefore central to assessing the institutional dimensions of climate justice.

We examine these questions using a country-year panel of 199 countries spanning 1995 to 2023, linking newly enacted national climate laws from the Climate Change Laws of the World database to climate-related disaster records from EM-DAT, alongside measures of income, emissions, and political institutions (Grantham Research Institute and Sabin Center for Climate Change Law, 2024; EM-DAT, 2024). We use national adaptation lawmaking as an observable indicator of governments' institutional responses to climate shocks. National climate laws of this kind are not merely symbolic. Recent evidence shows that they reduce emissions in practice (Eskander and Fankhauser, 2020). Because the outcomes are annual counts of newly enacted laws, we estimate Poisson models with country and year fixed effects, using a distributed-lag design that traces the legislative response in the disaster year and over the three subsequent years. This design allows us to examine not only whether climate disasters stimulate adaptation lawmaking, but also whether the translation of climate shocks into adaptive governance depends on institutional capacity.

Three findings follow. First, a disaster is followed by roughly a 60 percent increase in adaptation lawmaking cumulated over three years, with no pre-existing trend. The response tracks whether a disaster occurred rather than how severe it was, and it fades by the third year. Second, the same disasters produce no clear change in mitigation lawmaking, consistent with the free-riding logic described above. Third, and most importantly, this adaptation response is concentrated in high-income states and those with stronger rule of law, while low-capacity countries show no measurable response. Disasters do not prompt protective lawmaking everywhere. Rather, they do so where the capacity to

legislate already exists, indicating that the translation of climate shocks into adaptive governance is conditioned by institutional capacity, reinforcing existing inequalities in protection from climate change.

The paper makes three contributions. First, it provides cross-national, within-country evidence on a question studied mainly in single-country settings. Second, it distinguishes adaptation from mitigation lawmaking rather than treating climate policy as a single output. Third, it advances understanding of adaptive governance by showing that the policy windows created by climate disasters are institutionally conditioned rather than universally translated into legislative action, linking the focusing-event tradition to the literatures on adaptive capacity, adaptation finance, and climate justice. These results imply that external support for the lowest-capacity, highest-need states is not a supplement to disaster-driven adaptation but a precondition for it. More broadly, the findings suggest that climate disasters do not automatically generate adaptive governance but instead reveal how institutional inequalities shape national responses to environmental change.

2. Literature review

The idea that sudden, harmful, and visible events reshape the policy agenda is well established. Focusing events concentrate public and elite attention on a problem and open windows for new policy (Birkland, 1998; Kingdon, 1984). Disasters are among the clearest examples because they make a latent risk concrete at a particular place and time. Whether these policy windows translate into sustained adaptive governance, however, remains much less well understood. The political response is not neutral across policy types. Healy and Malhotra (2009) show that voters reward governments for visible disaster relief but not for the less visible work of preparedness. As a result, elected officials have stronger incentives to respond to realized harm than to invest in preventing future harm. Studies of legislative behavior point in the same direction. Elliott et al. (2023), examining four decades of environmental voting in the United States Senate, find that legislative support increases with unusual human losses but not with economic losses. They also find that the response is short-lived, lasting about two years. The economic literature likewise documents that disasters generate large and unevenly distributed losses, giving governments incentives to act (Botzen et al., 2019). Taken together, these studies suggest that disasters create political opportunities for policy change, but they provide limited evidence on whether those opportunities translate into enacted climate legislation across countries. Yet this literature has rarely asked whether disasters are followed by adaptation lawmaking, mitigation lawmaking, or neither across countries. Most existing evidence comes from single-country studies, often focused on the United States, and examines attitudes, disaster relief, or public spending rather than the content of enacted climate laws.

Adaptation and mitigation differ in who benefits and when those benefits are realized. Adaptation is closer to a local good. Sea walls, drainage systems, irrigation, and heat action plans primarily protect the jurisdictions that invest in them, and they address hazards that residents have already experienced (Fankhauser et al., 1999; Dittrich et al., 2016). Mitigation, by contrast, is a global public good. The benefits of one country's emissions reductions are shared worldwide and accrue over decades, giving

each country a strong incentive to free-ride on the mitigation efforts of others (Nordhaus, 2015). The two are linked because the amount of adaptation a society undertakes affects the damages that mitigation is intended to avoid. Likewise, decisions about mitigation influence the returns to adaptation (Kane and Shogren, 2000; IPCC, 2022). For the purposes of this paper, however, the central point is the asymmetry in incentives. A national disaster raises the returns to adaptation within the affected country, but it does little to change the free-riding incentives that limit mitigation. This difference in incentives provides a theoretical expectation that climate disasters should influence adaptation and mitigation lawmaking differently.

A substantial cross-national literature examines why some countries enact more climate legislation than others. Domestic factors play a central role. The stock and quality of existing climate laws strongly predict subsequent legislation, and flagship statutes often lead to further lawmaking (Fankhauser et al., 2015). Studies of adaptation policy specifically find that its adoption and diffusion across countries are driven largely by factors internal to each state rather than by external pressure alone (Massey et al., 2014). Political institutions also matter. Democracies generally show stronger climate commitments, while institutional quality shapes environmental outcomes more broadly (Bättig and Bernauer, 2009; Bernauer and Koubi, 2009). Recent work also shows that climate legislation has measurable economic consequences. For example, it influences bank lending in emerging economies (Ghosh, 2023).

A related literature focuses on adaptive capacity, defined as the resources, institutions, and knowledge that enable societies to translate policy intent into effective action (Smit and Wandel, 2006; Adger, 2006). Although its conceptualization and measurement remain contested, adaptive capacity is widely recognized as unevenly distributed and as a key determinant of adaptation outcomes (Engle, 2011; Berrang-Ford et al., 2011). The adaptation finance gap illustrates this inequality, with adaptation needs in developing countries substantially exceeding available financial resources (UNEP, 2024). Likewise, vulnerability indices consistently show that countries facing the greatest climate risks often possess the weakest institutional capacity to respond (Chen et al., 2015). Countries experiencing the largest disaster losses are therefore frequently those least equipped to convert climate risks into effective policy responses (Brooks et al., 2005).

Taken together, these two strands of literature point to an unresolved question. While focusing-event theory explains how disasters create opportunities for policy change, and adaptive-capacity research explains why countries differ in their ability to respond, they have rarely been integrated to examine whether climate disasters translate into adaptive governance through national climate legislation. It is this intersection that the present study addresses.

2.1 Research gap

Taken together, the literature points to a common unresolved question: under what conditions do climate disasters translate into adaptive governance? Three related gaps emerge. First, the focusing-event literature shows that disasters can reshape the policy agenda. However, it has not examined across

countries whether the resulting legislation focuses on adaptation or mitigation. Which type of climate law follows a disaster, and how this shapes adaptive governance, remains largely unexplored in cross-national research. Second, the climate policy literature explains why countries differ in the amount of climate legislation they enact. However, it rarely considers disasters as within-country shocks that may alter the composition of climate policy. It also seldom distinguishes between adaptation and mitigation lawmaking. Third, the literatures on adaptive capacity and adaptation finance argue that the ability to respond depends on national institutional capacity. Yet this proposition has rarely been tested by examining whether institutional capacity moderates the legislative response to climate disasters across countries at different levels of development. Addressing these gaps requires integrating insights from focusing-event theory, climate policy, and adaptive-capacity research within a common empirical framework. These gaps motivate the following research questions.

Research Question 1

Do climate disasters increase national adaptation lawmaking, and what is the dynamic pattern of that response?

Research Question 2

Do climate disasters affect national mitigation lawmaking, and does the mitigation response differ from the adaptation response?

Research Question 3

Does the adaptation response depend on institutional capacity, measured by income and governance quality, with climate vulnerability examined as a secondary dimension?

3. Conceptual framework and hypotheses

The legislative response to a climate disaster is the product of two factors: the incentive a disaster creates to legislate and the capacity of the state to act on that incentive. The first determines whether a policy window opens and which kind of law it favors. The second determines whether that window is used. Together, these factors explain why climate disasters may generate adaptive governance in some countries but not in others. Separating these factors clarifies why the same disaster can produce protective law in one country and no legislative response in another.

A disaster increases the salience of realized, local harm. Because adaptation directly addresses that harm and protects the jurisdiction that funds it, the perceived returns to adaptation rise within the affected country. Policymakers also gain a concrete problem to act on while public attention is high (Birkland, 1998; Kingdon, 1984). Political incentives reinforce this. Governments are rewarded for visible response more than for less visible preparedness (Healy and Malhotra, 2009). Because legislating takes time, the response should build over several years rather than appear at once. It should then fade as attention moves on.

Hypothesis 1

Climate disasters increase adaptation lawmaking, with the response emerging after the disaster, concentrated in the first few years, and declining thereafter.

Mitigation follows a different logic. Its benefits are global and slow to materialize, so a national disaster does little to alter the free-riding incentive that constrains it (Nordhaus, 2015). A disaster may raise the general salience of climate change and prompt some mitigation legislation. However, any such effect should be weaker than the adaptation response and harder to attribute to the disaster itself.

Hypothesis 2

Climate disasters produce a weaker mitigation response than adaptation response; any mitigation effect is expected to be small and not robust.

A policy window is necessary but not sufficient. Turning attention into enacted law requires capacity that is distinct from incentive and unevenly distributed across states. We conceptualize this as institutional capacity comprising three complementary dimensions. The first is administrative capacity, the bureaucratic ability to draft, coordinate, and implement legislation. The second is fiscal capacity, the resources needed to make commitments credible. The third is rule-of-law capacity, the rule-bound, predictable governance that allows new statutes to be passed and enforced (Brooks et al., 2005; Engle, 2011; Eakin and Lemos, 2006). We proxy these with national income and the quality of the rule of law, the two dimensions most consistently associated with the capacity to produce and enforce policy. Climate vulnerability is examined as a secondary dimension because need and capacity need not coincide. Where capacity is low, a disaster may make the need for adaptation obvious without enabling a legislative response. As a result, the same focusing event yields protective law in high-capacity states and little or nothing in low-capacity ones. Climate disasters therefore do not simply reveal differences in exposure; they also reveal inequalities in governments' ability to convert climate shocks into adaptive governance. If so, disasters reproduce the global adaptation gap rather than closing it. This distributional consequence places the result within debates over climate justice and adaptation finance (UNEP, 2024; Barrett, 2013).

Hypothesis 3

The adaptation response to climate disasters increases with institutional capacity: stronger in higher-income countries and those with stronger governance, and weak or absent where capacity is low.

Together, these hypotheses conceptualize disaster-driven adaptation as a governance process conditioned by institutional capacity. They map directly onto the three research questions. The first hypothesis addresses the first research question by examining whether disasters increase adaptation lawmaking. The second hypothesis addresses the second research question by testing the adaptation-mitigation contrast. The third hypothesis addresses the third research question by examining whether

capacity moderates the legislative response, with income, governance, and, secondarily, vulnerability as its dimensions.

4. Data

This study constructs a panel dataset linking national climate lawmaking to climate disasters and a range of development, emissions, and political measures. The dataset merges six data sources using country (ISO3) and year identifiers. The analysis covers the period from 1995 to 2023, during which all core variables are available. The full panel includes up to 199 countries. The estimation sample used for the summary statistics comprises 4,886 country-year observations across 170 countries. The dataset includes the outcome variables (new adaptation laws and new mitigation laws), the treatment variable (climate disaster occurrence), and a set of economic, environmental, and political covariates. Detailed information on the outcome variables (Appendix Section A.1), treatment variable (Appendix Section A.2), covariates and sample (Appendix Section A.3), and measurement and validity (Appendix Section A.4) is provided in the Appendix. Table 1 presents summary statistics. The sample contains substantial variation in both climate disasters and climate lawmaking, providing the within-country variation required for identification.

Table 1
Summary statistics

Variable	Mean	SD	Min	Max	N
New adaptation laws (count/yr)	0.220	0.676	0.000	24.000	4,886
New mitigation laws (count/yr)	1.308	3.25	0.000	39.000	4,886
Climate disaster (0/1)	0.604	0.489	0.000	1.000	4,886
Severe disaster (0/1)	0.312	0.463	0.000	1.000	4,886
Tropical cyclone, IBTrACS (0/1)	0.136	0.343	0.000	1.000	4,886
GDP per capita (const. 2015 USD)	11,895.716	17,582.176	211.008	112,417.877	4,886
Population (millions)	40.541	142.929	0.075	1,438.070	4,886
CO2 per capita (t)	4.686	6.574	0.00	76.624	4,886
Vulnerability (ND-GAIN)	0.439	0.094	0.251	0.675	4,886
Electoral democracy (V-Dem)	0.519	0.262	0.014	0.92	4,886
Notes: Adaptation and mitigation are annual counts of new laws. The IBTrACS storm indicator is the reporting-independent tropical cyclone measure described in Section S5 of the Supplementary Material.					

5. Methods

The outcome variables are sparse, non-negative counts of new laws, and most country-years record no new laws. Accordingly, the analysis uses Poisson pseudo-maximum likelihood (PPML) as the primary estimator. PPML accommodates zero outcomes without transforming the dependent variable and remains consistent under relatively weak assumptions in the presence of heteroskedasticity (Santos Silva and Tenreiro, 2006). To assess the robustness of the findings, we also estimate negative binomial and linear fixed-effects models, which produce the same qualitative conclusions.

5.1 Specification

Let $L_{c,t}$ denote the number of new laws of a given type, either adaptation or mitigation, adopted by country c in year t . The baseline specification is

$$E(L_{c,t}) = \exp(\alpha_c + \delta_t + \beta_0 D_{c,t} + \beta_1 D_{c,t-1} + \beta_2 D_{c,t-2} + \beta_3 D_{c,t-3}) \quad (1)$$

where $D_{c,t-k}$ is the climate disaster indicator for country c in year $(t-k)$. Country fixed effects α_c absorb time-invariant differences across countries, including legal systems, climate exposure, and the underlying propensity to legislate. Year fixed effects δ_t absorb common global shocks and long-run trends in climate lawmaking and disaster reporting. The estimated disaster effects are therefore identified from within-country variation over time. Because climate disasters are recurring rather than absorbing treatments, this framework is more appropriate than staggered difference-in-differences designs developed for one-time treatment adoption.

Two specifications are estimated. The first includes only the contemporaneous disaster indicator, $D_{c,t}$. The second estimates the distributed-lag specification in Eq. (1), including the contemporaneous disaster and its three annual lags. Climate legislation takes time to develop because laws must be drafted, debated, and enacted. A one-year specification may therefore miss much of the policy response. The distributed-lag model follows the logic of tracing the dynamic response to recurring shocks rather than to a single event (Jordà, 2005).

The main parameter of interest is the cumulative disaster effect, defined as $\theta = \beta_0 + \beta_1 + \beta_2 + \beta_3$. The cumulative effect is reported as a proportional change, $100 \times [\exp(\theta) - 1]$. Statistical significance is assessed using a linear combination test based on the cluster-robust covariance matrix.

The baseline specification includes only country and year fixed effects. Time-varying controls for development, emissions, and political institutions are introduced as robustness checks. The disaster treatment is estimated first as a binary indicator and then using the alternative treatment measures described in Section 4.2.

5.2 Estimation sample

In Poisson models with country fixed effects, countries that record no laws of a given type throughout the sample do not contribute to the estimation of the slope coefficients because their fixed effects are not identified (Santos Silva and Tenreyro, 2010). Accordingly, such countries are excluded from the corresponding estimation sample. For the adaptation models, 15 of the 199 countries enact no adaptation laws during the study period and are therefore excluded. All 199 countries are retained in the mitigation models. The excluded countries contain only zero outcomes, so their omission does not affect the interpretation of the estimated coefficients. They are also distributed across income groups rather than concentrated among low-income countries, making it unlikely that their exclusion explains the estimated capacity gradient. The estimation sample includes all remaining country-years for which the outcome variable, disaster treatment, and fixed effects are defined.

5.3 Identification and dynamic effects

The estimand is the average within-country change in climate lawmaking following a disaster, conditional on country- and year-fixed effects. Identification requires that, after accounting for these fixed effects, the timing of disasters is unrelated to the underlying trajectory of climate lawmaking. This assumption is plausible because floods, storms, and droughts are driven by weather rather than by the

policy process. Reverse causation from lawmaking to disaster occurrence is therefore unlikely. Because climate disasters are recurring rather than absorbing treatments, the empirical strategy follows a panel distributed-lag framework rather than staggered-adoption difference-in-differences designs developed for one-time treatment adoption (Jordà, 2005; de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

To assess the identifying assumption, the analysis estimates an event-study specification that tests for pre-existing trends in climate lawmaking prior to disaster occurrence. The model includes leads of the treatment for years $(t + 1)$ to $(t + 3)$, together with the contemporaneous disaster and lags through $(t - 4)$. Event time zero denotes the disaster year. Negative event times represent pre-disaster leads, and positive event times represent post-disaster lags. Because future disasters cannot affect current lawmaking, the lead coefficients should equal zero. A joint test of the lead coefficients provides the placebo and pre-trend test (Rambachan and Roth, 2023). The event-study specification is estimated using ordinary least squares with the same two-way fixed effects because the linear specification is more stable when many event-time indicators are included.

5.4 Heterogeneity analysis

The main heterogeneity question is whether the legislative response to climate disasters depends on institutional capacity. Income provides the primary dimension of comparison, while governance quality provides a complementary measure of administrative capacity. The distributed-lag model is estimated separately for World Bank income groups, defined using constant-2015 GDP per capita. Disaster effects are also interacted with governance quality using the V-Dem rule-of-law and corruption indices. These interactions distinguish administrative capacity from income alone. A secondary analysis compares countries above and below the median ND-GAIN vulnerability index. Because institutional capacity cannot be observed directly, income, governance quality, and climate vulnerability are treated as complementary empirical proxies capturing different dimensions of countries' ability to respond to climate disasters. The formal interaction model interacts each distributed-lag term with a high-income indicator. The cumulative interaction effect is evaluated using a cluster-robust linear combination test. The same approach is applied to the governance measures. Sample splits provide transparent comparisons across groups, whereas interaction models retain the full sample and therefore provide greater statistical power.

5.5 Inference and robustness

Standard errors are clustered by country throughout the analysis. This allows arbitrary serial correlation within countries and heteroskedasticity across countries (Cameron and Miller, 2015). The panel includes up to 199 country clusters, well above the range where cluster-robust inference becomes unreliable. The cumulative disaster effects and interaction effects are evaluated using linear combinations of the estimated coefficients based on the same clustered covariance matrix. As additional robustness checks for statistical inference, the main specification is re-estimated using two-way clustering by country and

year and a country-level cluster bootstrap. These procedures account for potential dependence across countries within the same year and provide a more conservative assessment of statistical significance.

6. Results

6.1 *Disasters raise adaptation lawmaking*

Table 2 reports the main estimates. In the distributed-lag specification, a climate disaster is associated with an approximately 60 percent cumulative increase in new adaptation laws over the disaster year and the following three years. The cumulative effect is statistically significant at the 5 percent level. The response does not occur immediately. The contemporaneous coefficient is small and statistically insignificant. The effect builds through the first and second lag before fading by the third year. This pattern is consistent with the time required to draft, debate, and enact new legislation and with the hypothesis that disasters create policy windows whose legislative effects emerge gradually rather than instantaneously.

The results differ for mitigation lawmaking. The same disaster is associated with a cumulative increase of about 18 percent. However, the estimate is statistically significant only at the 10 percent level. These findings provide initial evidence that climate disasters stimulate adaptation lawmaking much more consistently than mitigation lawmaking, consistent with the different incentive structures underlying the two forms of climate policy.

Table 2
Climate disasters and national climate lawmaking (PPML, country and year fixed effects)

Disaster (t)	(1)	(2)	(3)	(4)
	Adaptation, static	Adaptation, dist- lag	Mitigation, static	Mitigation, dist- lag
	0.094 (0.110)	0.095 (0.108)	0.023 (0.047)	0.026 (0.046)
Disaster (t-1)		0.165* (0.099)		0.055 (0.036)
Disaster (t-2)		0.213** (0.105)		0.058 (0.037)
Disaster (t-3)		-0.001 (0.084)		0.024 (0.040)
Cumulative (t to t-3)		0.472** (0.208)		0.163* (0.097)
Implied change		+ 60.3%		+ 17.7%
Country and year FE	Yes	Yes	Yes	Yes
Observations	5,336	5,336	5,771	5,771
Countries	184	184	199	199
Notes: Poisson pseudo-maximum-likelihood estimates for 1995–2023. The outcomes are annual counts of new national adaptation and mitigation laws. The disaster variable is a binary indicator equal to one if a climate-related disaster occurred in a given year. Columns 1 and 3 include only the contemporaneous disaster term. Columns 2 and 4 additionally include three annual lags. Country-clustered standard errors are reported in parentheses. $p < 0.10$, $p < 0.05$, $p < 0.01$. The cumulative effect is the sum of the contemporaneous and lagged coefficients. The implied percentage change is calculated as $(100 \times [\exp(\theta) - 1])$. The adaptation models exclude 15 countries that enacted no adaptation laws during the study period.				

Figure 1 provides descriptive context. Adaptation and mitigation lawmaking both increase over the sample period. Recorded climate disasters also become more frequent. These common trends are absorbed by the year fixed effects.

6.2 Dynamics and the pre-trend test

Figure 2 presents the event-study estimates. Event time zero denotes the disaster year, with leads shown to the left and lags to the right. For adaptation, the lead coefficients are close to zero. A joint test fails to reject the null hypothesis that the three lead coefficients are jointly equal to zero. This indicates that

adaptation lawmaking was not increasing before disasters occurred. The response emerges only after the disaster, supporting the identifying assumption that disaster timing is unrelated to pre-existing trends in adaptation lawmaking.

The pattern differs for mitigation. The lead coefficients are jointly significant, indicating that mitigation lawmaking was already trending upward before disasters occurred. The mitigation effect reported in Table 2 cannot therefore be cleanly attributed to the disaster and is not interpreted as a causal response. The adaptation results remain stable across event windows ranging from two to five years on either side of the disaster. As an additional falsification test, current adaptation lawmaking shows no response to disasters occurring four or five years in the future. A joint test fails to reject the null hypothesis that these future-disaster coefficients are jointly equal to zero. This pattern is consistent with a response to disasters rather than an underlying trend, strengthening confidence that the estimated adaptation effects reflect legislative responses to climate disasters rather than underlying policy dynamics. Complete event-study estimates are reported in the Supplementary Material.

6.3 Adaptation compared with mitigation

A statistically significant adaptation effect alongside a statistically insignificant mitigation effect does not, by itself, imply that the two responses differ significantly. Because the adaptation and mitigation models are estimated separately, the difference between their cumulative effects is tested using per-country influence functions with country-level clustering (see Supplementary Material, Section S2). The estimated adaptation response exceeds the mitigation response by 0.31 log points. However, the difference is not statistically significant at conventional levels ($p = 0.12$). These results indicate that the evidence is consistent with a stronger legislative response for adaptation than for mitigation, although the difference between the two responses cannot be established statistically. The results therefore support two separate conclusions: disasters are followed by a clear increase in adaptation lawmaking, while no comparable increase is observed for mitigation lawmaking. They do not provide statistical evidence that the two responses differ from one another. The interpretation of the mitigation estimates is further qualified by the event-study results. Specifically, mitigation lawmaking exhibits a significant pre-trend. It is therefore treated as a comparison outcome rather than as a clean causal response to climate disasters.

6.4 The response depends on capacity

Table 3 reports the heterogeneity analysis and presents the central finding of the paper. Panel A reports the formal interaction test, in which the four distributed-lag terms are interacted with a high-income indicator. The additional adaptation response in high-income countries is large and statistically significant, indicating that the legislative response to climate disasters is systematically conditioned by institutional capacity.

Table 3
Heterogeneity in the cumulative adaptation response

Panel A. High-income interaction	Cumulative effect	Implied change
Lower and middle-income	0.196 (0.251)	+ 21.6%
High-income, additional (interaction)	0.942*** (0.330)	
High-income, total	1.138*** (0.232)	+ 212.1%
Panel B. Income-group splits		
Low income	-0.063 (0.710)	-6.1%
Lower-middle income	0.169 (0.307)	+ 18.5%
Upper-middle income	0.569 (0.466)	+ 76.6%
High income	0.632** (0.314)	+ 88.2%
Notes: Poisson pseudo-maximum-likelihood estimates. The reported effects are cumulative adaptation effects over the disaster year ((t)) and the subsequent three years ((t) to (t-3)). All specifications include country and year fixed effects. Country-clustered standard errors are reported in parentheses. $p < 0.10$, $p < 0.05$, $p < 0.01$. The interaction effect is the sum of the four high-income interaction terms, evaluated using a cluster-robust linear combination test. Income groups follow the World Bank classification based on constant-2015 GDP per capita and are assigned in each country-year. Complete sample-split results are reported in the Supplementary Material.		

The estimated adaptation response differs markedly across income groups. For low- and middle-income countries, the cumulative effect is about 22 percent and is not statistically significant. For high-income countries, the cumulative effect is approximately three times larger and highly significant. Panel B shows the same pattern. The estimated response increases steadily with income, from no measurable effect in low-income countries to a strong response in high-income countries. Together, these results suggest that disasters create opportunities for new climate legislation across countries, but the ability to translate those opportunities into enacted adaptation laws depends on institutional capacity rather than disaster occurrence alone. This pattern is consistent with the paper's theoretical expectation that policy

windows generated by climate disasters yield legislative change only where sufficient institutional and administrative capacity exists.

To distinguish institutional capacity from income alone, the disaster terms are also interacted with the rule-of-law measure. The additional response in countries with stronger rule of law is about 0.49 log points and is statistically significant. This finding suggests that administrative and institutional capacity, rather than income alone, shapes the legislative response to climate disasters, indicating that the observed capacity gradient cannot be attributed solely to differences in economic resources. Additional analyses based on corruption, OECD membership, region, and climate vulnerability show the same general pattern. These results indicate that the capacity gradient is not driven by a single region or group of countries and is robust across alternative measures of institutional capacity and country characteristics. The full set of estimates is reported in the Supplementary Material.

6.5 Robustness

Table 4 summarizes the robustness analyses. The main adaptation result remains stable across alternative sample periods, from a 1990 start through a 2005 start. The estimated effect also becomes stronger when small states are excluded using population thresholds. It remains robust after adding time-varying controls for development, emissions, democracy, and globalization, indicating that the baseline results are unlikely to be driven by omitted time-varying factors. The positive adaptation response is therefore robust to alternative sample definitions, model specifications, and control sets.

Table 4
Robustness of the cumulative adaptation effect Panel A. Alternative specifications

Specification	Cumulative effect	Implied change	Obs.	Countries
Any disaster (baseline)	0.472**(0.208)	+ 60.3%	5,336	184
Severe disaster	0.371*(0.224)	+ 44.9%	5,336	184
Disaster count	0.028(0.049)	+ 2.8%	5,336	184
Log people affected	-0.014(0.045)	-1.4%	1,263	91
Window 1990–2023	0.529**(0.206)	+ 69.8%	6,256	184
Window 2000–2023	0.511**(0.209)	+ 66.6%	4,392	183
Window 2005–2023	0.511**(0.226)	+ 66.8%	3,458	182
Population > 1 million	0.541**(0.231)	+ 71.8%	4,197	147
Population > 5 million	0.578**(0.243)	+ 78.2%	3,157	115
Excluding EU members	0.333(0.230)	+ 39.5%	4,524	156
Adding time-varying controls	0.498**(0.208)	+ 64.6%	4,583	160
Panel B. Alternative estimators				

Estimator	Cumulative effect	Implied change
Poisson FE (PPML)	0.472**(0.208)	+ 60.3%
Negative binomial FE	0.448**(0.207)	+ 56.5%
Linear FE (OLS)	0.062(0.039)	—
Notes: Panel A reports robustness checks based on alternative disaster measures, sample restrictions, and model specifications. Panel B reports estimates from alternative estimators. All Poisson and negative binomial models estimate the cumulative adaptation effect over the disaster year ((t)) and the subsequent three years. All specifications include country and year fixed effects. Country-clustered standard errors are reported in parentheses. The baseline sample covers 1995–2023. The controls specification additionally includes GDP per capita, population, the urban population share, carbon dioxide emissions per capita, the Electoral Democracy Index, the regime-type classification, and the KOF Political Globalization Index. The linear fixed-effects model is estimated in levels; therefore, no implied percentage change is reported. $p < 0.10$, $p < 0.05$, $p < 0.01$.		

The findings are also robust to alternative estimators. The negative binomial model produces estimates that are very similar to the baseline PPML results, while the linear fixed-effects model yields the same positive relationship, although with lower precision, as expected for count outcomes with many zero observations. Together, these results indicate that the main findings do not depend on the choice of estimator. Likewise, the absence of robust effects for disaster counts and disaster severity indicates that the legislative response is associated primarily with the occurrence of a climate disaster rather than its measured magnitude.

The robustness checks also clarify which aspect of disasters matters. The adaptation response is strongest when disasters are measured by their occurrence. A weaker response is observed for the severe-disaster indicator, whereas no clear relationship is found for the number of disasters or the number of people affected. This pattern suggests that legislative responses are driven more by the occurrence of a salient disaster than by its measured magnitude, consistent with disasters acting as focusing events that trigger legislative attention rather than responses that scale proportionally with disaster severity.

Finally, excluding European Union member states reduces the precision of the estimates but does not change their direction, consistent with the concentration of the adaptation response among higher-capacity countries. Additional robustness analyses, including legislative versus executive acts, hazard-specific estimates, and the adaptation share of new climate laws, are reported in the Supplementary Material and support the same overall conclusion. Taken together, the robustness analyses indicate that the principal findings are not sensitive to alternative measures of disasters, estimation methods, sample restrictions, or model specifications.

6.6 Inference and sample robustness

Table 5 reports additional robustness checks for statistical inference, identification, and sample selection. The main adaptation result is robust to alternative methods of statistical inference. The conclusions are unchanged when standard errors are clustered by both country and year and when

inference is based on a country-level cluster bootstrap. These results indicate that the statistical significance of the main finding does not depend on the choice of inference procedure, providing additional confidence that the estimated adaptation response is not an artifact of the baseline variance estimator.

The findings are also robust to an alternative identification strategy. A heterogeneity-robust estimator that compares newly affected countries only with countries remaining untreated over the same four-year window reproduces the same dynamic pattern as the distributed-lag model (de Chaisemartin and D'Haultfœuille, 2020). The response is close to zero in the disaster year, peaks after two years, and then fades. The similarity in both timing and magnitude indicates that the main findings are not driven by heterogeneous treatment effects in the two-way fixed-effects estimator, strengthening confidence that the estimated relationship reflects the legislative response to climate disasters rather than features of the estimation approach.

Table 5
Additional robustness and identification checks

Robustness check	Estimate
Baseline PPML	0.472 **
	(0.208)
Two-way clustering (country and year)	0.472 **
	(0.219)
Cluster bootstrap	0.472 **
	95% CI [0.067, 0.886], $p = 0.022$
Heterogeneity-robust estimator	0.179 †
	(0.076)
Physical storm measure (IBTrACS)	0.263
	(0.353)
EM-DAT storm measure	0.236
	(0.224)
Notes: The baseline estimate is the cumulative adaptation effect over the disaster year and the following three years from the distributed-lag PPML model. Standard errors are shown in parentheses unless otherwise indicated. The cluster bootstrap reports the 95 percent confidence interval. The heterogeneity-robust estimator reports the cumulative increase in adaptation laws rather than the log coefficient. IBTrACS provides a reporting-independent measure of tropical cyclone exposure based on physical storm tracks. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. † Statistical significance is based on the reported bootstrap z-statistic.	

The adaptation response is also unlikely to reflect reporting bias in the disaster data, which are known to be less complete in some lower-income countries (Jones et al., 2022). A reporting-independent measure based on tropical-cyclone tracks from the International Best Track Archive produces results that closely match those obtained from the EM-DAT storm indicator (Knapp et al., 2010; Gahtan et al., 2024). Although both estimates are imprecise, their agreement suggests that reporting bias is unlikely to explain the main findings, providing additional support for the validity of the disaster measure used in the baseline analysis.

Finally, the estimation sample does not drive the heterogeneity results. The countries excluded because they enacted no adaptation laws during the study period are distributed across income groups rather than concentrated among low-income countries. In addition, the high-income interaction is estimated using a common sample, so the heterogeneity results do not depend on changes in sample composition across specifications. Taken together, these additional checks indicate that the central findings are robust to alternative inference procedures, identification strategies, disaster measures, and sample definitions.

7. Discussion

The central finding is that climate disasters are followed by a clear increase in adaptation lawmaking but no clean response in mitigation lawmaking. This pattern is consistent with the long-standing view that sudden, visible, and harmful events act as focusing events that push problems onto the policy agenda and open windows for policy change (Birkland, 1998; Kingdon, 1984). A disaster concentrates harm in one place and time, and adaptation responds directly to that harm. It is therefore the part of the climate policy agenda that moves most clearly after a disaster. More broadly, the findings suggest that climate disasters primarily reshape domestic adaptive governance rather than governments' willingness to provide the global public good of emissions mitigation.

Two features of the adaptation response receive support from earlier research. First, the response tracks the occurrence of a disaster rather than its measured size. It is present for a simple indicator that a disaster occurred but disappears when disasters are measured by their number or by the number of people affected. Second, the response is short-lived. It builds during the first two years after a disaster and fades by the third. Both patterns are consistent with Elliott et al. (2023), who find that support for environmental legislation in the United States Senate responds to human rather than economic losses and that the effect is temporary. This parallel is supportive rather than definitive. The occurrence-rather-than-magnitude pattern is also consistent with an attention-based view of agenda setting. Legislative attention is limited, and a salient disaster is more likely to capture attention than a larger but less visible accumulation of losses (Baumgartner and Jones, 1993). The evidence therefore points to attention as one mechanism through which disasters influence adaptation lawmaking, although it should not be interpreted as a general theory of policymaking. Instead, the results suggest that focusing events create temporary opportunities for legislative action, the extent of which depends on the institutional conditions examined in the following discussion.

The contrast between adaptation and mitigation also aligns with the economics of the two policy types. Adaptation is largely a local public good. A sea wall, drainage system, or heat action plan primarily protects the country that invests in it. A disaster therefore raises the domestic return to adaptation. Mitigation, by contrast, is a global public good. The benefits of one country's emissions reductions are shared internationally and accrue over a much longer period. A national disaster therefore does little to alter the incentives that underlie the free-riding problem in climate mitigation (Nordhaus, 2015). The empirical results are consistent with this logic. At the same time, the evidence does not support a strong claim that mitigation is unaffected by disasters. The estimated mitigation effect is positive but imprecise, the formal difference between the adaptation and mitigation responses is not statistically significant, and mitigation lawmaking exhibits a pre-trend in the event-study analysis. The appropriate interpretation is therefore that disasters clearly increase adaptation lawmaking, while no equally clear response is identified for mitigation. This distinction highlights that climate disasters appear to stimulate policy responses where benefits are immediate and nationally concentrated, but not where benefits remain diffuse and globally shared.

The result with the greatest implications for climate governance concerns national capacity. The adaptation response is concentrated in higher-capacity countries. The same pattern appears when capacity is measured by the rule of law rather than by income, suggesting that the result reflects the capacity to legislate rather than wealth alone. Capacity is not observed directly in this study. Income, governance quality, and vulnerability are treated as complementary proxies, and the interpretation rests on the consistency of their patterns. The evidence supports a capacity-based explanation. A disaster may create pressure for new legislation in many countries, but translating that pressure into enacted law requires administrative, fiscal, and technical resources that are often limited in lower-capacity settings. This interpretation is consistent with long-standing work on the state and adaptation. That work shows that the capacity to lead adaptation is itself shaped, and often constrained, by wider economic and political pressures (Eakin and Lemos, 2006). The data identify this capacity gradient but do not directly observe the mechanisms that produce it. The focusing-event and capacity-constraint explanations are therefore consistent with the evidence rather than directly demonstrated. This interpretation also aligns with the adaptation finance gap. Developing countries require substantially greater financial resources for adaptation than they currently receive (UNEP, 2024). The countries that are most exposed to climate hazards, and least able to finance adaptation, also appear to be the least able to translate disasters into protective legislation. Rather than closing this gap, disasters appear to expose it more clearly. From a governance perspective, the findings suggest that climate disasters do not automatically strengthen adaptive governance. Instead, they amplify existing differences in countries' institutional capacity to convert policy attention into enacted legislation. This has important implications for international adaptation support, because strengthening institutional capacity may be as important as financing adaptation measures themselves.

Several limitations qualify these findings. The outcome variables measure enacted laws and executive policies rather than their implementation, funding, or enforcement. Legislation is an important step toward adaptation, but it does not guarantee implementation, and the gap between enactment and

delivery may itself be greater where capacity is limited. The disaster data also have known reporting limitations, particularly in some lower-income countries and regions (Jones et al., 2022). The empirical design addresses this concern through year fixed effects, a treatment based on disaster occurrence rather than measured magnitude, and robustness checks using a reporting-independent measure of tropical-cyclone exposure. Nevertheless, residual under-reporting could remain and would, if anything, tend to understate the estimated capacity gradient. The study also identifies responses on the extensive margin and therefore cannot determine how the magnitude of disasters influences the strength of legislative responses. Finally, the results represent average effects across diverse political systems. The heterogeneity analyses demonstrate that these averages conceal substantial differences in legislative responses across countries. Future research should therefore examine whether the legislative responses identified here translate into effective implementation and improved adaptation outcomes, particularly in lower-capacity settings.

Taken together, the findings describe a specific and uneven pattern of climate governance. Climate disasters are followed by more adaptation lawmaking, but primarily where governments already possess the capacity to respond. The results therefore suggest that climate disasters create opportunities for adaptive governance, but that these opportunities are realized unevenly because they depend on institutional capacity. The broader implications of this pattern for sustainability and climate justice are discussed in the conclusion.

8. Conclusion and policy implications

Climate disasters reshape national climate lawmaking, but in a narrow and uneven way. Across 199 countries over nearly three decades, a climate disaster is followed by a substantial increase in adaptation lawmaking over the following three years, while mitigation lawmaking shows no clear response. Governments respond to climate shocks by adopting laws that address realized harm, whereas lawmaking aimed at reducing emissions does not increase in the same way. This pattern is consistent with the economics of the two policy types. Adaptation primarily benefits the country that undertakes it, whereas mitigation generates a global public benefit that provides limited incentives for any one country to expand its efforts following its own disaster (Nordhaus, 2015). It is also consistent with evidence that political incentives favor visible responses to realized harm over less visible investments in prevention (Healy and Malhotra, 2009). The findings therefore show that climate disasters primarily reshape domestic adaptive governance rather than the broader trajectory of national climate policy.

The more important finding concerns who responds. The increase in adaptation lawmaking is concentrated in higher-capacity countries, particularly those with higher incomes and stronger governance. Disasters reveal the need for adaptation everywhere, but they lead to new adaptation legislation mainly where the administrative, institutional, and fiscal capacity to legislate already exists (Smit and Wandel, 2006; Adger, 2006). Viewed alongside the adaptation finance gap, this represents a governance challenge with an important justice dimension (UNEP, 2024). The communities and

countries most exposed to climate hazards are typically least responsible for the emissions that drive them and least able to finance adaptation (Barrett, 2013). This disparity places distributive justice at the center of how disasters translate into protective law. The countries that are most exposed to climate hazards, and least able to finance adaptation, also appear to be the least able to translate disasters into protective legislation. The results therefore suggest that unequal institutional capacity is itself an important driver of unequal adaptive governance.

The policy implications follow directly from these findings. First, because the legislative response is concentrated in higher-capacity countries, strengthening administrative and fiscal capacity in lower-income countries is likely to be more effective than raising awareness alone. Building the institutions needed to design, finance, and implement adaptation policies would increase the ability of vulnerable countries to convert climate disasters into protective legislation. Second, because the response is associated with the occurrence of disasters rather than their measured magnitude, support mechanisms need not wait for catastrophic losses before action is taken. Timely assistance following climate shocks may therefore be more effective than assistance triggered only after exceptionally large disasters. Third, because the legislative response is strongest during the first few years after a disaster, adaptation finance and implementation plans are likely to be most effective when they are established in advance and can be activated quickly while the policy window remains open. Together, these findings suggest that strengthening institutional capacity should be viewed as a core component of climate adaptation policy rather than as a separate development objective. These implications should be interpreted within the limits of the empirical design. The analysis concerns enacted laws rather than their implementation and identifies within-country changes in lawmaking rather than the effects of specific policy interventions.

The findings should also be interpreted in light of several limitations. The study measures laws and executive policies on the books rather than their implementation, funding, or enforcement. Disaster reporting remains imperfect despite the robustness checks undertaken here, particularly in lower-income countries. Finally, although the identification strategy supports a causal interpretation of the adaptation results, the evidence remains observational rather than experimental. Within these limits, the findings point to a clear conclusion. Climate disasters are followed by more adaptation lawmaking, but primarily in countries that already possess the capacity to respond. Strengthening that capacity is therefore likely to be as important as responding to the disasters themselves. More broadly, the study shows that climate disasters do not automatically generate more adaptive governance. Their legislative consequences depend fundamentally on the institutional capacity of states to translate moments of crisis into durable policy change.

Declarations

Data availability

All data used in this study are derived from publicly available sources, each cited in the reference list. The replication package will be made available on request.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author(s) used ChatGPT to correct and improve the language, readability, and cohesiveness of the text. After using this tool, the author(s) reviewed and edited the content as needed and take(s) full responsibility for the content of the published article.

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Figures

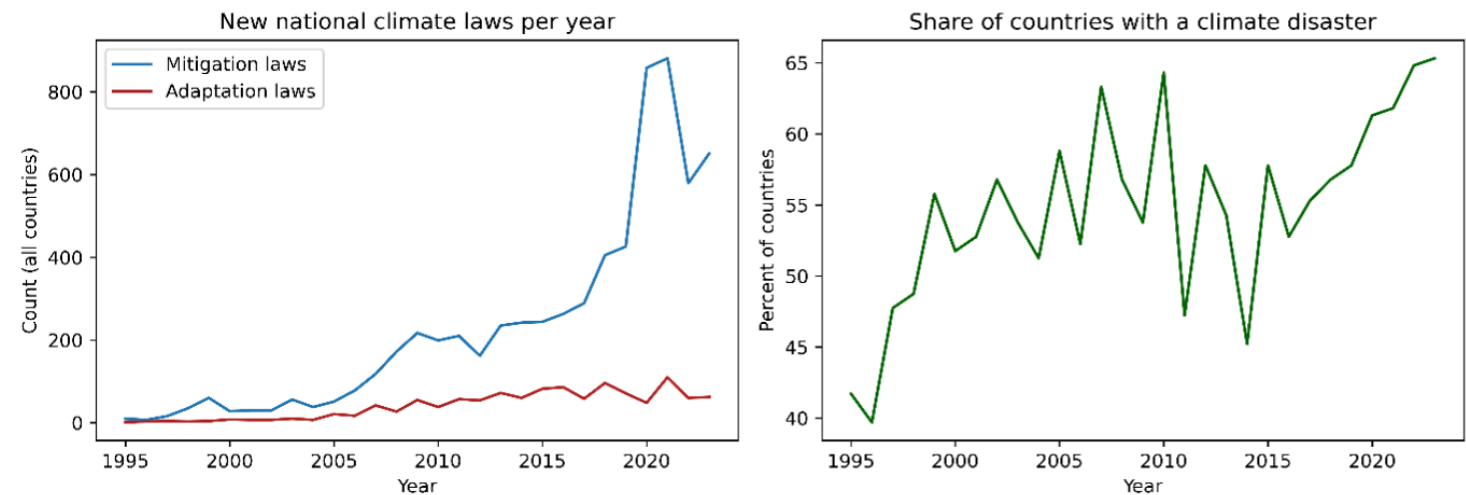


Figure 1

New national climate laws per year (left) and the share of countries recording a climate disaster (right), analytic sample, 1995 to 2023.

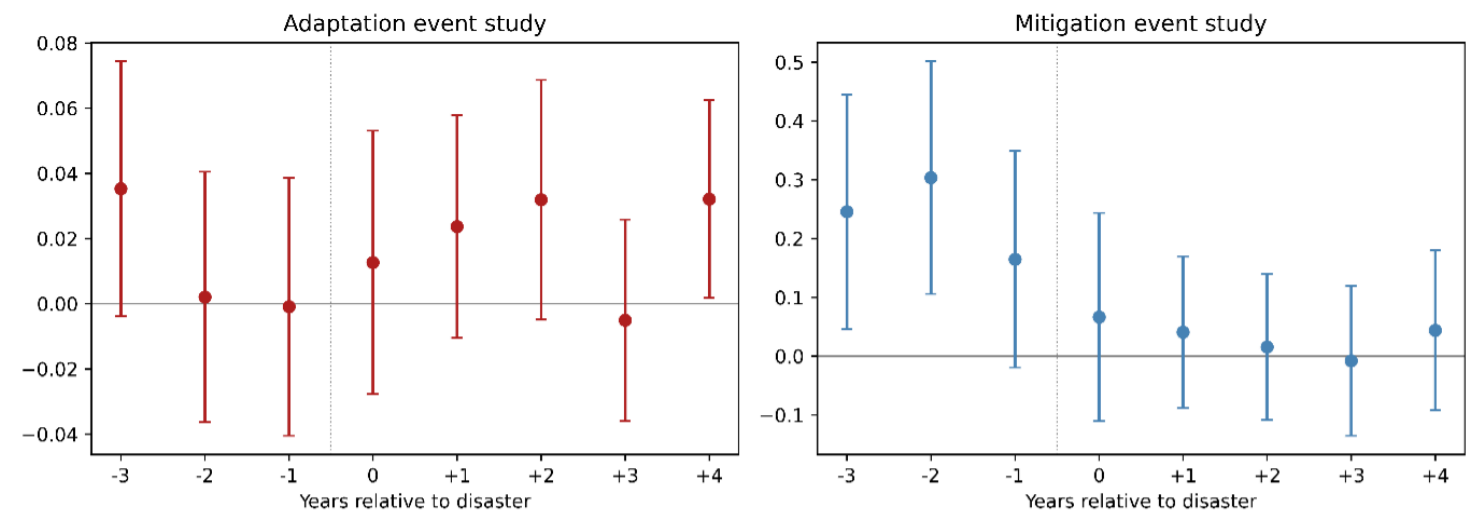


Figure 2

Event-study estimates for adaptation (left panel) and mitigation (right panel). Event time zero denotes the disaster year. Negative event times represent leads, and positive event times represent lags. Vertical bars show 95 percent confidence intervals based on country-clustered standard errors.

Supplementary Files

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