“STORM AUTOCRACIES”:
ISLANDS AS NATURAL EXPERIMENTS[±]

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Abstract

We exploit the exogenous variation in the timing and intensity of storms in island countries to estimate the storms’ effect on the extent of democracy. Using a rich panel dataset spanning the period 1950–2020, our difference-in-differences estimations, which allow multiple treatments over time, indicate that storms trigger autocratic tendencies in island countries by reducing the Polity2 score by about four percent in the following year. These findings resonate with our simple dynamic game-theoretical model, which predicts that governments move towards autocracy by placating citizens with post-disaster assistance in response to citizens’ insurgency threat in the absence of relief, giving rise to the political regime of “storm autocracies”. Our results survive a battery of robustness analyses, randomisation tests, potential spatial biases, and other falsification and placebo checks.

Keywords: Storms, storm autocracies, natural experiments, islands, post-disaster relief, insurgency, autocracy, democracy

JEL Classification: O0, P0

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1. Introduction

Following in the footsteps of Greek philosophers Plato and Aristotle, a considerable number of scholars have attempted to understand the root causes of the variations in the quality of democratic institutions across countries.[1] Magnifying the relevance of this line of research lately is the recent appearance of authoritarian turning points around the world—e.g., the election of Trump in the United States, Putin in Russia, Duterte in the Philippines, Erdoğan in Turkey, Modi in India, and Orbán in Hungary. Similarly, many storm-prone island countries around the world—such as Haiti, Fiji and the Philippines—experience persistent autocratic regimes.

What factors help such autocratic regimes come to existence, survive, and even thrive for prolonged periods of time? One conventional argument is that these autocratic governments can mobilise the citizens' multi-peaked preferences to a single point and use this point as leverage to tilt the majority vote in their favour.[2] Once elected, these governments then repress citizens in several dimensions of the political sphere to prolong their tenure (see Berrebi and Ostwald 2011; Kim et al. 2015; and Wood and Wright 2016). Given that the rise of such nondemocratic regimes has repercussions for the well-being of billions around the world through trade wars, volatile currencies, international standoffs, refugee problems, and even famines, it is crucial to understand the dynamics behind any systematic deviations from democracy.

The primary objective of this paper is to illuminate how political agents seize a window of opportunity that is a breeding ground for autocracy in the wake of a particular type of shock, namely, major storms. As their frequency and intensity increase geometrically over time, storms constitute a major avenue of change in political conditions. We start with the observation that major natural shocks typically increase pre-existing group disparities and induce political exclusions and societal grievances via their enormous fatalities and overwhelming material destruction. In their aftermath, if governments fail to adequately compensate victims for their losses, survivors may stage protests, exhibit anti-government political behavior, and even launch massive insurgencies.[3] Thus, when a disaster with such implications occurs, the government faces a dilemma: either incur the cost of a major disaster

[1] See, most notably, Engerman and Sokoloff (1997) and Acemoglu, Johnson and Robinson (2005).
[2] See Black (1948) on the rationale of group decision-making when a decision is reached by voting or is arrived at by a group whose members are not in complete accord. Also see Arrow (1951) and Kramer (1973) on the formation of social choice under voting.
[3] Regimes typically interpret post-disaster instability and political tensions by non-government groups as possible threats, and often respond with repression (see for instance Pelling and Dill 2006).

relief effort or confront the cost of a possible insurgency, with the consequent possibility of being overthrown. In most cases, the government rightfully deems the latter cost larger, especially if the government itself is not an autocratic one, because such governments cannot effectively fight attempts to overthrow them.

Based on this intuition, our stylized dynamic game-theoretic model of citizen–government interactions in the wake of storms yields a unique equilibrium whereby the government opts to deliver to citizens adequate and effective post-disaster relief, as a result of which citizens refrain from insurgency even if the government also resorts to autocracy. Consequently, the incumbent government can tighten its grip over the polity by increasing state control in the aftermath of the shock. This means that storms end up provoking autocratic tendencies through a mutually-agreed political repression, generating a specific type of political regime for which we coin the name “storm autocracies”. [4]

Next, we provide empirical evidence of how storm shocks can result in autocratic tendencies. Our focus on a natural shock provides a plausibly exogenous source of variation for what is otherwise a highly endogenous and convoluted political equilibrium determined by historical, economic, and cultural factors. We use extensive cross-country panel data observed annually from 1950 to 2020, which exhibits significant country-by-year variation in storm presence and intensity, to estimate the reduced form relationship between storm shocks and the level of democracy in a natural-experimental setting. In so doing, we exploit an underlying geographic characteristic, namely storms in islands, as a natural experiment.[5] Our setting is intended to capture the random variation in the *timing* (i.e., the year of incidence) and *intensity* (i.e., their frequency as well as physical severity) of storm exposure in affected island countries compared with their storm-free counterparts.

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[4] A historical case that epitomizes our mechanism, in which the government placated citizens in the wake of a major storm only to establish an autocratic regime thereafter, is the case of Rafael Trujillo in the Dominican Republic. Three and a half weeks after Trujillo’s ascension to the presidency in 1930, Hurricane San Zenon hit Santo Domingo, leaving 2,000 people dead. As an immediate response to the disaster, Trujillo placed the country under martial law, which lasted until 1934. Ornes (1958, p. 62) acknowledges that “*credit must be given to Trujillo for prompt, sweeping measures intended to alleviate the plight of the inhabitants*.” Nevertheless, “*in many respects the hurricane proved a blessing for Trujillo. To meet the crisis, the National Congress passed a law suspending constitutional guarantees and investing the President with authority to take any steps, economic or otherwise, to raise funds on public credit, to distribute relief supplies and to do whatever was demanded by the circumstances*.” Consequently, “by 1934 all opposition had been silenced or driven underground, but Trujillo was not satisfied. He craved the all-out support of all Dominicans. People soon learned that they had to be vocally on "the Chief’s" side, since to be "indifferent" was as bad as to be "subversive." This end was achieved very successfully through fear, through the hope of personal advancement or through vulgar bribery. The lure of public office, after a brief visit to jail, was usually sufficient to gain converts” (Ornes 1958, p. 68; emphasis added).

[5] A similar empirical strategy was pursued by Feyrer and Sacerdote (2009), albeit in a different context.

An additional empirical advantage of storms in islands relates to the full exposure of the island constituency to the treatment. As disasters rarely affect a country's entire territorial space, it is generally challenging to determine their average treatment effect on a national outcome, such as the level of democracy.[6] Island countries help to address this pitfall, in that major storms tend to traverse a large part of their territories, especially for relatively small islands such as Haiti, Dominican Republic, and Fiji. This results in a large constituency being exposed, or 'treated', thus making such islands a viable cluster for unmasking the average treatment effect on the national outcome of interest.

We employ a generalized difference-in-differences (DID) approach with multiple treatments over time to compare the levels of democracy in island countries with and without storms. The validity of our identification strategy relies on the evidence that many island countries are victims of major storms (most notably, Dominican Republic, Fiji, Haiti, and Madagascar experience severe storms regularly), while several others are spared storms due to their geographic locations (e.g., Iceland and Singapore experienced no storms between 1950 and 2020).

We account for unobserved heterogeneity in the storm–polity nexus by relying on the fact that storms are exogenous shocks: they strike randomly in terms of both *timing* and *intensity*. Even if politicians expect storms in a given year based on a country's storm history, they cannot predict their frequency or severity in that year. The triggering factors of storms—e.g., wind patterns and the ocean surface temperature—are hardly predictable in the medium- and long-term: a storm typically originates in the ocean due to variations in its surface temperature, and its route is determined by the wind speed and direction. These factors are exogenous to both economic and political outcomes.

We achieve comparability between storm-prone and storm-free countries by accounting for an array of permanent differences across countries through controlling for country-fixed effects. We also control for year-fixed effects to help neutralize any shocks experienced by all countries. Moreover, we isolate the long-term path of political conditions (which might be driven, for example, by countries' locations or colonial history) by

[6] Between 1950 and 2009, storms claimed nearly a million lives and affected an additional 850 million people (CRED 2011), as well as causing tremendous material destruction. For example, in absolute terms, Hurricane Katrina was one of the most destructive and deadly natural catastrophes to hit the United States, incurring damage of around US$125 billion (CRED 2011). However, in relative terms, it had no significant effect on economic and political outcomes at the national level. Indeed, given the massive size of the US economy, the destruction resulting from this hurricane was comparatively negligible (see Cashell and Labonte 2005).

controlling for country-specific linear time trends. Once time-invariant country differences, year-fixed effects, and country-specific trends are all controlled for, the two country groups become reasonably comparable, at least initially, and the remaining variation in the model is likely to yield a credible average treatment effect of storms.

Our empirical analysis corroborates our theoretical prediction that storm shocks cause a deterioration in democratic conditions. First, storms in island countries reduce the Polity2 measure of democracy by 4.25 percent in the following year. In a falsification exercise, we fail to find this adverse effect in an average landlocked or coastal country. Second, we estimate that governments restrict political rights following storms, which constitutes a critical mechanism from storms to a reduced level of democracy. This finding is consistent with the endorsement that the majority of citizens give to populist-autocratic leaders around the world. It also accords with milder forms of repression, such as militarization of the disaster response, given that countries are increasingly deploying their military to extend relief and assistance, at home and abroad (Weeks 2007). We also consider coups and national elections as alternative mechanisms but rule them out. Third, we show that disaster aid is likely to play an important role in financing the repression mechanism. Noting that our benchmark results exclude very large island countries such as Australia and Indonesia, our findings are robust to the use of alternative storm measures, controlling for potential spatial biases and other economic and political control variables as well as the "adaptation" effect to storms, and satisfying the parallel trends assumption.

Our results have two major implications. First, they explain why storm-prone small island countries around the globe (e.g., Haiti, Fiji, the Philippines) remain autocratic over prolonged periods and exhibit the political regime of "storm autocracy". Our basic setup predicts that, in environmentally-challenged areas where governments need to provide relief swiftly and effectively, the storm effect can breed autocratic tendencies naturally.[7] These tendencies could increase over time, given that these polities seem to be experiencing larger numbers of catastrophic shocks due to climate change. Such developments increasingly empower "storm autocrats" because they can declare emergencies in disaster-hit zones to

[7] Our setup has some parallels with Karl Wittfogel's "Hydraulic Empires" in terms of how a society may support autocracy overall. Wittfogel (1957) argued that a centralized command was required to manage the water resources in Oriental polities where the agriculture was irrigation-driven, and that autocracy was a mutually agreed outcome between the autocrats and their subjects. This ultimately led easily to autocratic regimes. On the other hand, polities in which the agriculture was rainfall-driven saw comparatively less centralization, and hence a lower tendency toward despotism. Although the "hydraulic hypothesis" has been refuted for several regions of the world, our model embodies a specific type of political incentive after storms that leads to autocracy through society-wide agreed repression.

provide instant relief and support, only to turn into dictators later—a phenomenon of which Rafael Trujillo of the Dominican Republic is probably one of the initiators. Second, our results illuminate how politicians unify citizens into a common faculty, and how this, in turn, could pave the way for a tolerable autocratic political equilibrium. As shocks make the typical citizen feel economically and politically more vulnerable, politicians can take steps towards autocracy by 'buying off' their citizens through populist economic and political means.

Our study relates to several different strands of the literature. We contribute to a growing body of literature examining the effects of exogenous events (e.g., rainfall shocks, climate change, drought, and natural disasters) on political conditions. For example, Cavallo et al. (2013) indicate that natural disasters followed by radical political revolutions (e.g., the Islamic Iranian Revolution in 1979 and the Sandinista Nicaraguan Revolution in 1979) have a negative effect on output in both the short and the long run. Leigh (2009) and Wolfers (2006) suggest that incumbent politicians may be rewarded or punished for economic developments that are clearly outside their sphere of influence. In a similar vein, Achen and Bartels (2017) and Healy and Malhotra (2009) find that leaders are punished for droughts, floods, and even shark attacks. While remaining in this strand overall, we depart from it by highlighting the quality of political institutions in the wake of storms.

We also contribute to the broader literature on the influence of natural hazards-driven disasters on the political space. Notable examples include Albala-Bertrand (1993), Davis and Seitz (1982), Anbarci, Escaleras and Register (2005), and Kahn (2005). This paper is also closely related to the literature on the government's responsiveness to citizens in the aftermath of disasters (see Sobel and Leeson 2006). Most notably, Cole et al. (2012) find that voters punish the incumbent party for weather events beyond its control, but fewer voters punish the ruling party when the government responds vigorously to the crisis. Our paper fits this vein of the literature by studying the interactions between citizens and government in the wake of storms.

Our study also contributes to the rich body of literature on government repression dynamics under the threat of citizen insurgencies (see Berrebi and Klor 2006; Berrebi and Ostwald 2011; Kim et al. 2015; and Wood and Wright 2016). Several studies on Sri Lanka show that terrorism escalated significantly in the years following the 2004 Indian Ocean tsunami (Beardsley and McQuinn 2009; and Le Billon and Waizenegger 2007). Wood and Wright (2016) argue that the incumbent government escalates repression in the wake of disasters because the combination of increased grievances and declining state control they produce creates a window of opportunity for dissident mobilization and challenges to state authority. We add to this line of research by examining the potentially dangerous and delicate interaction between autocracy and insurgency following storms. Specifically, we argue that the combination of the citizenry's weakened urge to stage insurgency and the government's repression underpinned by relief assistance in the wake of storms may provide an incentive to deviate from democracy. This argument is also consistent with 'disaster militarism', which has become increasingly common in the public space after disasters, given the extensive use of the military's policing, fire-fighting, army-engineering, nursing, and emergency-handling skills in the post-disaster relief process.[8]

This paper is also related to the rich and broad literature on transitions away from democracy. A wide range of views exist regarding the increased risk of transitions from democracy to authoritarianism owing to turmoil, crises and economic downturns, which reduce the opportunity costs of coups and revolutions (see, among others, Acemoglu and Robinson 2001; Svolik 2008; Teorell 2010). Our contribution to this literature is to highlight an environment in which autocratic perpetuity arises. In our setting, natural disasters may undermine democracy persistently because they can lead to the congregation of disgruntled masses and revolutionary threats,[9] force large numbers of displaced people to gather in shelters and refugee camps, and facilitate mass movements. As a result, repression can arise in response to overt dissent through prohibitions on assemblies, curfews and monitoring (and even the detention of dissidents, see Ritter and Conrad 2016; and Sullivan 2015, 2016). It is also not difficult to see that authoritarian tendencies can arise if regimes use preemptive repression to manage anticipated dissent.[10]

A burgeoning body of literature also exists which focuses on the economic impacts of hurricanes, tornadoes, and typhoons. Notably, Deryugina et al. (2018) use administrative tax return data to demonstrate that Hurricane Katrina had only a small, transitory impact on the employment and incomes of its victims (see also Vigdor 2008; and Deryugina 2017). Belasen

[8] Examples of military support in the aftermath of disasters include that of the Bangladesh military after the 1991 cyclone, the Central American militaries after Hurricane Mitch in 1998, the US military after Hurricane Katrina in 2005, the UK military following the floods in Britain in 2007, and the Chinese military in the aftermath of the earthquake in Sichuan province in 2008 (https://odihpn.org).

[9] See for instance Preston and Dillon (2005) for a description of how the aftermath of an earthquake in Mexico City in 1985 led to the formation of a protest movement, and Bommer (1985) for the way in which earthquakes and floods promoted protests in Nicaragua, contributing to the downfall of President Somoza.

[10] Our paper also adds to the literature on the scale effect of constituency on democracy (Alesina, Spolaore and Wacziarg 2000; Anckar 2004; Dahl and Tufte 1973; Srebrnik 2004; Strobl 2012; Tiebout 1956; and Wittman 2000).

and Polachek (2009) investigate the labour market impact of hurricanes in Florida, and Ouattara and Strobl (2014) examine the local migration effects of hurricane strikes in U.S. coastal counties. Elliot, Strobl and Sun (2015) study the local economic impact of typhoons in China. We contribute to this line of research by focusing on the effects of storms on democratic conditions.

Finally, this paper is related to the literature on resource windfalls and their effects on political regimes. For example, Caselli and Tesei (2016) find that windfalls such as oil shocks have ignorable effects in democracies (Norway) or entrenched autocracies (Saudi Arabia), but swing the political equilibrium in more unstable autocracies (Nigeria, Venezuela). In Sub-Saharan Africa, Brueckner and Ciccone (2011) find that democracy scores improve and the probability of democratic transitions increases following adverse income shocks induced by negative rainfall spells, which is consistent with Acemoglu and Robinson's (2001) theory of political transitions. Our contribution to this literature is the finding that the government must provide disaster relief (which can be at least partly funded by foreign aid) to avoid insurgencies, and the relief can be disbursed in a more orderly fashion in autocratic regimes. That is, in our setup, not only is disaster relief a cheaper alternative than costly suppression, it is likely to be more orderly under more autocratic regimes, which are also better prepared against insurgencies. We highlight the interaction between disaster relief, its orderly distribution, and the regime's preparedness against insurgencies in the rise of autocratic regimes following resource windfalls.

Taken together, this paper makes two main contributions to the literature. First, we shed theoretical and empirical light on how a political equilibrium emerges in the form of "storm autocracies", whereby the government becomes more autocratic in return for providing the citizenry with relief assistance following a natural shock, and the citizens accept it. Second, empirically, we exploit storm shocks in island countries. We measure storms in terms of their binary presence each year, frequency per annum, and physical severity via a composite storm index that accounts for different physical severity measures of the storms.

The remainder of this article is organized as follows. Section 2 describes the data sources. Section 3 explains our identification strategy. Section 4 outlines our estimation method and model specification. Section 5 presents the benchmark results, potential mechanism, and robustness checks. Section 6 concludes.

2. Hypothesis, Data, and Measurement

2.1. Hypothesis

What are the possible dynamics between political agents and citizens in the wake of a storm? The core of the answer lies in the government's preferences, conditional on citizens' reactions following a storm event. Appendix A1 in Supplementary Online Appendix presents a simple game that is played between a government and its citizens after a storm, in two stages. Diagram A1 shows the game tree that describes their strategic interactions. In a nutshell, after observing the storm's damage, a government takes the initiative and decides simultaneously whether to provide post-disaster relief to citizens, and whether to be autocratic or democratic in providing relief and in countering any insurgency, looting, or chaos by citizens. Based on the government's initial – possibly pre-emptive – move, citizens may or may not choose to show their discontent with the post-disaster environment in the form of an insurgency, which may be a more viable possibility if the government provides no relief. Of course, any insurgency, chaos or looting will be costly for any type of government to neutralize, especially in the dire conditions of the post-disaster environment. However, it is reasonable to assume that a democratic government will find it costlier to neutralize than an autocratic one, since the latter's emphasis on and preparation of the military will be higher than that of a democratic government (as elaborated in the Appendix).

The game is a two-stage relief allocation game between the government and its citizens in the aftermath of storms. The solution to such a dynamic game is obtained through 'backward induction', whereby the analysis starts with the citizens' decision at stage two regarding insurgency. Then, once everyone has calculated that decision, the government can determine its joint optimal decision on relief provision and the type of regime. The game provides a unique equilibrium whereby citizens do not resort to insurgency and the government chooses to be autocratic and to provide post-disaster relief, pre-empting citizens' insurgency threat. That is, the government "buys" the right to be autocratic by incentivising citizens via post-disaster relief assistance in the face of an insurgency threat (which would be very credible in the absence of relief). Carrying this theoretical result to our empirical framework, we first test the following hypothesis:

Hypothesis Storm shocks provoke the government's nondemocratic tendencies in island countries along with the provision of post-disaster relief, independent of any other channel, owing to the threat of citizen insurgencies.

There might be alternative perspectives to our payoff-maximization-based hypothesis in relation to the dynamics between political agents and citizens in the aftermath of storms. For example, how could we separate our argument from one in which democracy is considered a piece of infrastructure like any other, but one that is slower to rebuild? There are several similarities between infrastructure and democracy, but there are also some significant differences. Any infrastructure requires time, effort, and resources to rebuild, and so does democracy. Further, both require maintenance for quality assurance, as they tend to deteriorate over time. Thus, one can liken democracy to infrastructure, in that both are naturally vulnerable to storms. However, a major feature of infrastructure is that it can be improved via foreign resources, whereas re-establishing democratic conditions via foreign intervention may not be received well by the citizens at large. Rebuilding democracy is primarily a domestic affair and requires the right power balance among diverse social groups, the elite, and politicians. In our setting, we take the perspective that storms distort the power balance in favour of the *incumbent* authorities, given that they are the drivers of the post-storm relief and recovery interventions. The resulting stable period, which is endorsed by the populace and focuses on effective post-disaster relief, is likely to enable the incumbent to consolidate their power through nondemocratic means.

Another aspect of our model is that it mostly captures scenarios with an irregular storm prevalence. If storms happen very regularly and cause moderate damage, they can be associated with well-calculated advanced preparedness. However, multiple storms one after another, especially with excessive damage, would make such advanced preparedness very difficult. Our model mostly focuses on the latter irregular storm prevalence case. Empirically, this matches our model aptly because our estimates capture storm effects beyond the linear time trends.

2.2. Data on storms

We use storm data from two alternative sources: Emergency Disasters Database (EM-DAT) and International Best Track Archive for Climate Stewardship (IBTrACS). EM-DAT reports data on severe storms[11] during the period 1950–2020 if a storm event meets *any* of its four severity criteria: (i) 10 or more people are reported as killed; (ii) 100 people are reported as affected; (iii) there is a call for international assistance; and (iv) a state of emergency is declared. These criteria ensure that the data set includes most major storms (Ramcharan

[11] Following EM-DAT, we define storms as tropical and extra-tropical cyclones, typhoons, hurricanes, and tornados.

2007). We use EM-DAT to generate a naive binary treatment variable that takes a value of 1 if a storm hits a country at least once in a given year, and 0 otherwise. Appendix A2 shows 195 countries, of which 52 are storm-free, while the rest faced storms at least once during the period 1950–2020) Also, we identify years with multiple storm exposures by constructing a frequency measure that is the total number of storm events that occurred in a year. Figure 1 shows the total number of storms that occurred annually in each island country.

However, the EM-DAT dataset has been criticized in the literature for a few reasons. First, it is argued that it is more likely to include disasters from richer countries, as it is based on the use of insurance claims (Felbermayr and Gröschl 2014). Second, the EM-DAT dataset is likely to identify mostly severe storms (see Kahn 2005; Toya and Skidmore 2007; and Felbermayr and Gröschl 2014). Third, it is argued that there are measurement errors due to the underreporting of lower-intensity events prior to 1990 (Ramcharan 2007). Fourth, population-based measures of storm intensities are likely to be a function of economic development, which may be confounded with the quality of political institutions (see Kahn 2005; Toya and Skidmore 2007; and Felbermayr and Gröschl 2014).

We address these criticisms by constructing a physical storm intensity measure from the IBTrACS database. The IBTrACS data (version 4), maintained by the National Climatic Data Center of the National Oceanic and Atmospheric Administration (NOAA), is unequivocally the most complete global collection of storms. It collates data from the World Meteorological Organization (WMO), regional specialized meteorological centres, and other government and non-government organizations around the world.[12] The IBTrACS provides data on the total number of storm events, along with their wind speeds (in knots), wind directions and barometric pressures, at granular nodal points on the earth's surface at 6-hourly intervals. However, the dataset provides neither the names of affected countries nor the length of each storm's track, so we undertake extensive geographic information system (GIS) work to identify each storm track and the storm-affected countries (see Map 1). In particular, we match the point data of a storm event with the associated vector data of wind direction for a nodal point to trace the path of a given storm. This enables us to determine the inland trajectory of each storm track, which is crucial in our setting for checking the extent to which a storm affected a country's territory. We determine the severity of different storms by supplementing the identified storm tracks with nodal data on wind speeds and barometric

[12] The data are obtained from a variety of sources, such as reconnaissance aircraft, ships, and satellites (Chu et al. 2002; Yang 2008; Felbermayr and and Gröschl 2014).

pressures along the storm's path. Finally, in the spirit of Felbermayr and Gröschl (2014), we aggregate the total storm track length on land (distance travelled over land), storm frequency, wind speed, and wind pressure deviation from the average atmospheric pressure, and construct an annual storm intensity index for each country between 1950 and 2020.[13] This index can be considered as a "damage function" that could potentially indicate the severity of storm damage across countries and over time. That is, holding the level of income constant, for example, stronger winds or a longer storm track on land should result in greater damage.

It should be clarified that EM-DAT and IBTrACS draw data from the same storm events on the Earth's surface, though the former picks up only more major ones that pass a certain threshold of severity. That is, EM-DAT, in principle, captures the subset of storms that are most devastating, while the IBTrACS enables us to capture storms of any size/severity. From the perspective of our conceptual mechanism, this difference merits some discussion. First, we aim to capture storms that instigate a relief and recovery process, because our aim is to identify the consequences of this process for political development. Thus, while one may be concerned in general that EM-DAT may suffer from under-reporting, for our purposes, one could likewise be plausibly worried that the IBTrACS may suffer from 'over-reporting', as it includes storms of *any* size. That is, small storms are unlikely to trigger a relief and recovery mechanism, whereas severe storms do. Second, while EM-DAT may suffer from selectivity because disaster declaration is not a random process in a country, any composite index has its own measurement error problems which can potentially cause attenuation bias.[14] That is, even if the IBTrACS measure can serve as a damage function, it nevertheless provides an estimate of the damages only by proxy. Thus, from a measurement perspective, the IBTrACS dataset is unlikely to be a panacea for the shortcomings of EM-DAT and probably has its own pitfalls. Third, it is argued that EM-DAT has a reverse causality problem when the focus is on income and growth. However, reverse causality seems likely to be weaker when the dependent variable is *Polity2* because even *most* autocrats are open about their storm experience, to attract international aid. Nevertheless, we

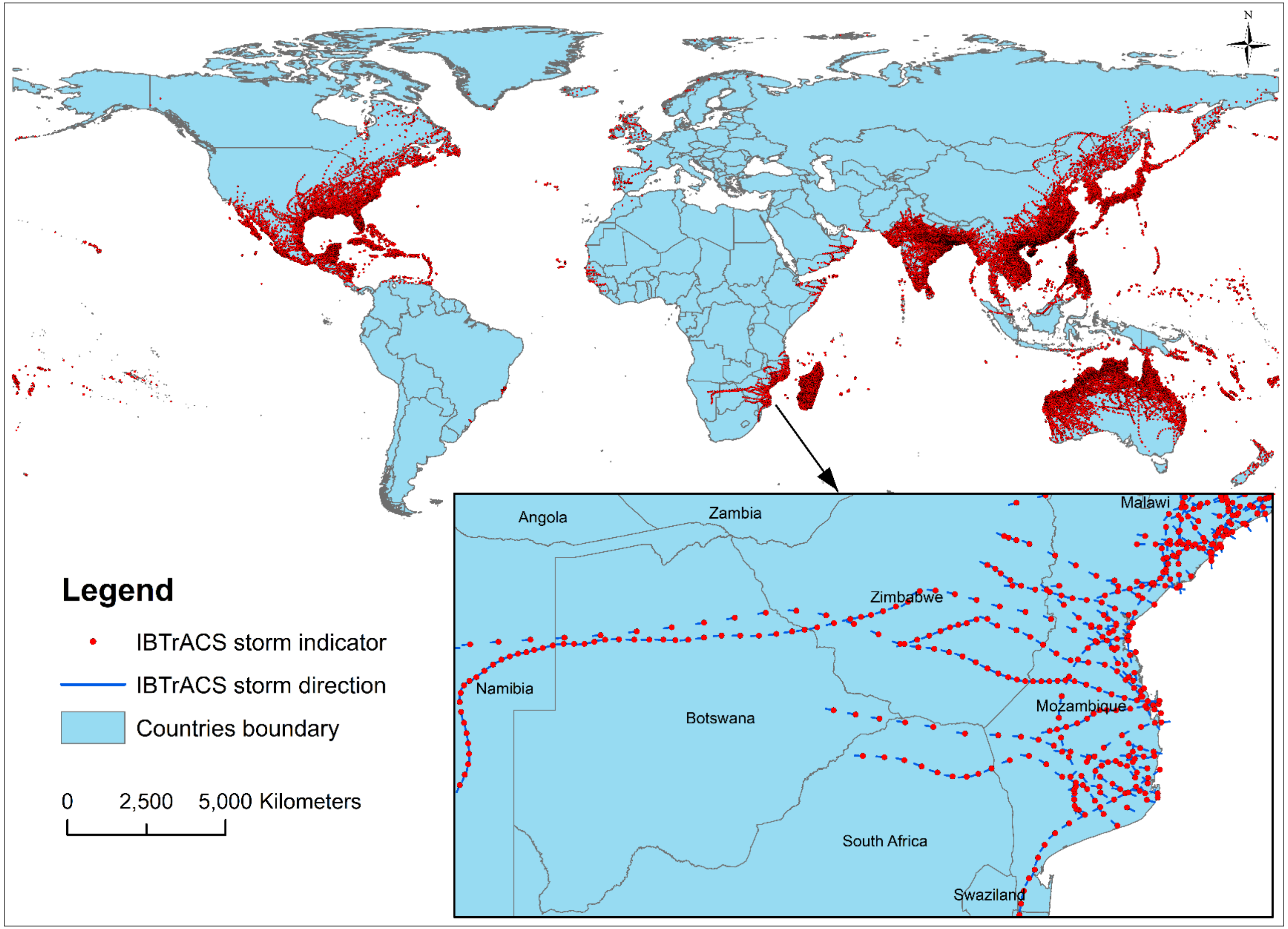
[13] As an example of construction, consider the two storm tracks in the north of South Africa shown in the inset box in Map 1. Our first task is to trace out each of these storm tracks based on the point data on the storm events and the associated vector data on wind directions. Once the tracks are identified, we incorporate the wind speed and barometric pressure data, to determine the severity of each storm. Finally, we construct the storm index per year at the country level by aggregating different storm events using four (exogenous) correlation-matrix-based variables: storm frequency, wind speed, deviation from atmospheric pressure, and total length of the storm track. We use an average sea-level atmospheric pressure of 1013.25 millibars.

[14] Indeed, we observe that the IBTrACS dataset misses some storms that are reported in the EM-DAT database, especially those for smaller countries where, presumably, the data holders were unable to communicate with the local meteorological organisations to confirm the event.

cannot remove the possibility of reverse causality completely, in that the quality of political institutions may improve the effectiveness of disaster management institutions to save lives and limit human casualties (see Strömberg, 2007). Hence, our benchmark analysis avoids using EM-DAT's population-based measures in order to bypass several endogeneity issues. Overall, our analysis takes a balanced stance on both the EM-DAT and IBTrACS measures *ex ante*.

Figure 1: Frequency and Severity of Storms in Island Countries

Notes: Each of the 282 dots represents a year with one or more storm events; the size of each dot is proportional to the number of storms. We exclude from our regression sample island countries with surface areas below 500 km^2 (i.e., American Samoa, Antigua and Barbuda, Aruba, Barbados, Grenada, Malta, Nauru, Palau, Seychelles, St Vincent and Grenadines, and Tuvalu) or above 1 million km^2 (i.e., Australia and Indonesia).



Map 1: International Best Track Archive for Climate Stewardship (IBTrACS) Data on Storm Locations, 1950-2020

2.3. Data on political institutions and other variables

Our benchmark measure of political institutions is the well-known, revised combined Polity score (i.e., Polity2) of the Polity IV database (Marshall and Jaggers 2010), measured in the interval range [–10, 10], from absolute non-democracy to mature democracy. Arguably the best polity measure of democracy widely used in the literature, Polity2 measures the level of democracy based on three concept variables: the competitiveness of political participation, the openness and competitiveness of executive recruitment, and constraints on the executives. We also use Freedom House's Political Rights and Civil Liberties index as a (reverse) measure of the repression mechanism.[15]

Table 1 presents the means and standard deviations of the variables used in this paper. Appendix A3 shows 195 countries, of which 42 are landlocked, 47 are islands and the rest have both coastal belts and land borders. Appendix A4 provides detailed sources and definitions.

3. Identification Strategy: Exogeneity Assumptions and Controls

We divide all countries around the world into three groups: island, landlocked, and coastal. The countries within each group differ in many ways; however, island countries are relatively more homogeneous than the other two in terms of their spatio-economic characteristics.[16] First, island countries have no land borders with neighbours. Second, most islands have small land areas and populations. Thus, from an economic perspective, they are likely to present similar market characteristics.[17] Third, because they are surrounded by oceans, modes of international human movement are limited. These idiosyncratic characteristics lead their political agents to behave in similar fashions (see Anckar 2004; Srebrnik 2004; Srinivasan 1986).

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[15] This mechanism acknowledges that political rights and civil liberties are an intermediate step on the path to high-quality democratic institutions. In other words, political rights and civil liberties refer to the democratic process and the procedural aspects of democracy in terms of the electoral process, political pluralism and participation, while the *Polity2* measure of democracy measures the quality of more advanced stages (i.e., constraints imposed on the executive and checks and balances established in the country).

[16] Even landlocked countries can face storms that traverse through their coastal belt neighbors. Our sample includes 27 storm-prone and 15 storm-free landlocked countries.

[17] To illustrate this, Alesina and Spolaore (2003) listed five disadvantages of countries with small population sizes: (a) higher per capita costs of public goods and comparatively inefficient tax systems; (b) expensive per capita military costs; (c) lower productivity due to the lack of specialization, although access to international markets may attenuate this effect; (d) an inability to share local risks at the national level, as local shocks often turn into national shocks due to small size; and (e) less ability to redistribute income.

3.1. Islands as special case for storms

Island countries are clearly more exposed to storm shocks than landlocked and coastal countries. As island borders are open to the ocean, storms may hit island countries from any direction. In addition, given that most island countries are small, storms are more likely to traverse the whole territory. This results in a large constituency being 'treated', making them a viable cluster for analysing a national outcome of interest. Conversely, most non-island countries—landlocked and coastal nations—rarely or never feel the effects of storms at the national level, due to both the lower probability of facing storms and the countries' sheer size (see Strobl 2011). This argument fits well in the literature on the political economy of the mass media (see, for example, Eisensee and Strömberg, 2007), in that storms may lead most of the affected citizens of island countries to proactively follow up the government's post-disaster emergency responses and recovery activities and, if necessary, hold them responsible.

To demonstrate further how islands form a special case for storms, Table 1 provides the sample means of some key storm variables by year for island, landlocked and coastal countries. The unconditional relationships indicate that island countries do indeed differ significantly from coastal and landlocked countries in some storm-related aspects. For example, EM-DAT shows that island countries are exposed to more storms than other countries (i.e., 2.582, 1.160 and 1.672 storm events/year for island, landlocked and coastal countries, respectively), with the differences being statistically significant. While acknowledging the limitations of its population-based measures, EM-DAT further shows that the proportion of storm-affected people in island countries is conceivably higher than in landlocked or coastal countries. Moreover, the IBTrACS measure of the frequency of storms, of any size, is higher in island countries (18.749 storm events/year) than in either landlocked (8.571 storm events/year) or coastal (9.930 storm events/year) countries, on average, and these differences are statistically significant. [18]

[18] There are at least two other reasons why islands are more appropriate for our analysis than landlocked and coastal countries. First, the borders of most island countries are drawn by nature only, and thus are fully exogenous, whereas the borders of landlocked and coastal-belt countries may be endogenous to politico-economic forces (Alesina 2003) that may have ongoing effects on democratic conditions, such as conflict with neighbors and guerilla wars. Second, island countries are likely to be homogeneous in ethnic diversity, national identity, and religion (see Royle 2001).

Table 1: Descriptive Statistics: Means and Standard Deviations

Variable	Island Countries		Landlocked Countries		Coastal Countries	
	Year of Storm=1	*Year of Storm=0*	*Year of Storm=1*	*Year of Storm=0*	*Year of Storm=1*	*Year of Storm=0*
Polity2 measure of democracy	4.723 (6.308)	4.683 (6.649)	3.613 (6.413)	-0.342*** (7.406)	3.610** (6.483)	1.324*** (7.296)
Coup (Powell-Thyne measure)	0.042 (0.248)	0.037 (0.249)	0 (0)	0.054 (0.306)	0.034 (0.228)	0.068*** (0.332)
Observations	*285*	*840*	*75*	*1271*	*415*	*3753*
Measure of Political Rights, *t*	3.162 (1.890)	2.839 (1.920)	4.013*** (2.165)	4.520*** (2.075)	3.458* (2.079)	3.866*** (2.189)
Observations	*259*	*641*	*75*	*1099*	*402*	*2933*
Measure of Civil Liberty, *t*	3.270 (1.637)	2.981 (1.684)	3.827*** (1.913)	4.308*** (1.791)	3.530* (1.743)	3.825*** (1.874)
Observations	*259*	*641*	*75*	*1100*	*402*	*2933*
EM-DAT: Binary storms, *t* – 1	1 (0)		1 (0)		1 (0)	
EM-DAT: Frequency of storms, *t* – 1	2.582 (2.430)	-	1.160*** (0.369)	-	1.672*** (1.122)	-
EM-DAT: Total storm-affected people (per 100k population), *t* – 1	3473.2 (8857.7)	-	2672.8 (12634.3)	-	2068.1* (11928.5)	-
Observations	*285*	*840*	*75*	*1271*	*415*	*3753*
IBTrACS: Frequency of storms, *t* – 1	18.749 (21.178)	-	9.930*** (7.423)	-	8.571*** (9.526)	-
IBTrACS: Total length of storm track (km), *t* – 1	648.443 (748.804)	-	390.971*** (358.854)	-	305.852*** (379.687)	-
IBTrACS: Storm Index, *t* – 1	1.360 (1.048)	-	0.457 (0.409)	-	0.517 (0.572)	-
Observations	*411*	*716*	*57*	*1290*	*424*	*3750*
IBTrACS: Average maximum wind speed (knots), *t* – 1	46.551 (20.726)	-	24.31*** (10.279)	-	33.937*** (18.232)	-
Observations	*405*	*716*	*53*	*1290*	*397*	*3750*
IBTrACS: Average maximum wind pressure (mb), *t* – 1	906.305 (201.728)	-	937.48 (165.269)	-	907.086 (227.996)	-
Observations	332	*716*	48	*1290*	309	*3750*
Disaster Aid (in US$ 1million)	0.876 (6.607)	0.056 (0.607)	0 (0)	0.015** (0.331)	0.690*** (10.564)	0.021 (0.721)
Official Development Assistance (in US$ 1million)	13.888 (53.117)	3.339 (49.641)	22.597 (46.665)	6.858** (26.858)	21.607 (115.216)	11.554*** (86.738)
Observations	*285*	*840*	*75*	*1271*	*415*	*3753*
Real GDP Per Capita (US$), *t* – 1	8049.938 (9701.078)	9892.368 (9259.327)	6449.896 (10667.49)	5096.834*** (8260.917)	7680.898 (9140.566)	8124.588*** (10821.6)
Openness (%), *t* – 1	64.488 (37.831)	90.058 (74.366)	88.923*** (47.164)	64.723*** (41.671)	68.475 (41.092)	64.501 (41.275)
Observations	*229*	*737*	*38*	*1014*	*281*	*3152*

Notes: Standard deviations are given in parentheses. The statistical significance of the group mean differences of years of storm presence (absence) of landlocked and coastal countries is with respect to years of storm presence (absence) of island countries. For example, Year of Storm=0 in landlocked and coastal countries is compared with Year of Storm =0 in island countries. Significant at the: * 10% level; ** 5% level; *** 1% level.

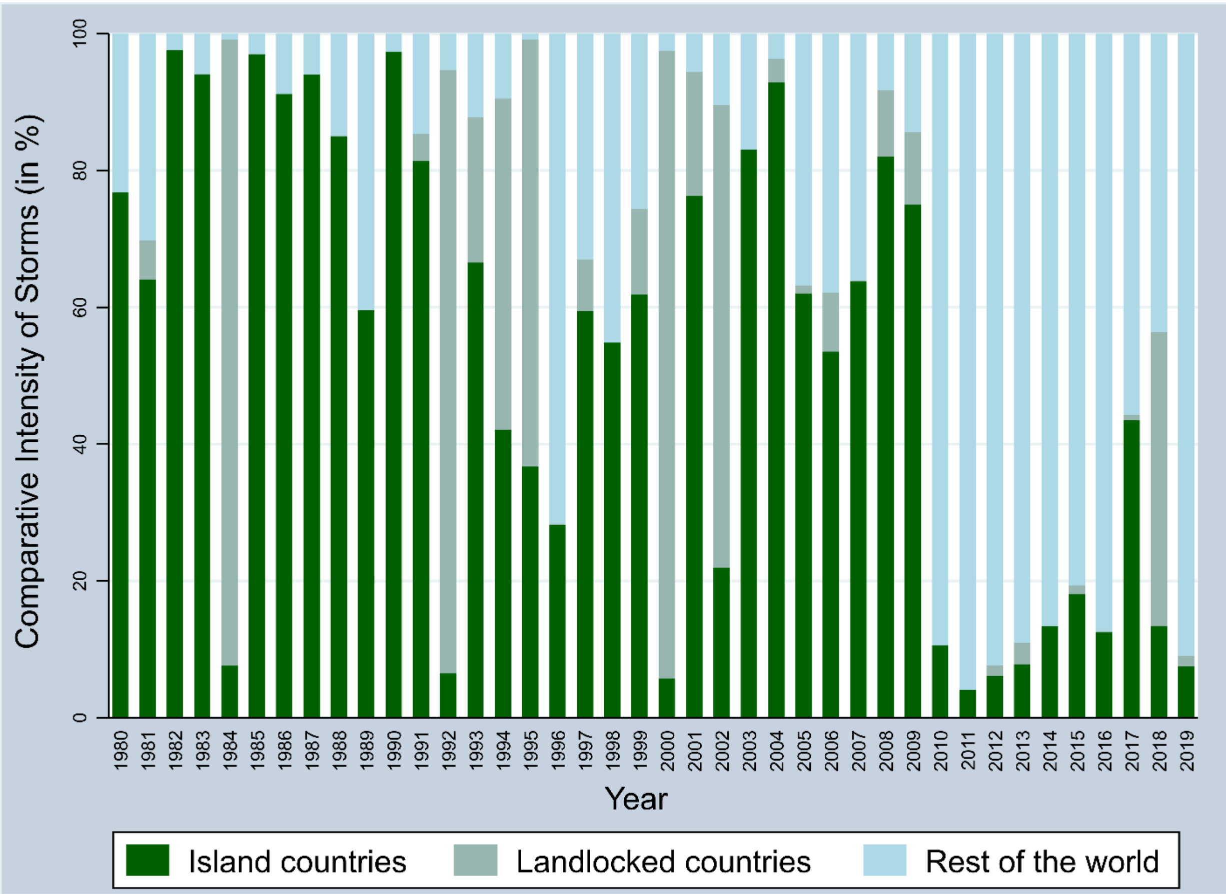
3.2. Storms as exogenous shocks

We argue that, conditional on several observable and unobservable factors, storms in island countries are random in terms of their *timing* (i.e., the year of incidence) and *intensity* (i.e., frequency in a given year, and physical severity). Consequently, we assume that the random assignment of storms' *timing* and *intensity* across countries yields exogenous variation in government decisions regarding the level of democracy and palliative efforts in time and intensity.

We identify at least three factors which suggest that storms are exogenous. First, storms generally originate in the ocean, and their routes depend on the wind *direction* and *speed* (see Craghan 2003). As the atmospheric attributes of winds are regarded as exogenous, the resulting storms due to wind variability are also exogenous. Second, current meteorological techniques provide accurate early warning systems that can predict the destinations of storms shortly after they appear. However, such prediction is not possible until the source of the storm in the ocean has been identified. The *source* and *timing* of storms cannot be predicted before their generation: storms may form at any time in any place on earth (see Figure 2). Third, a storm's direction cannot be controlled or changed, even once its source in the ocean is identified. The storm is likely to strike a land surface at some point; the only questions are when and where. Taken together, these points indicate that storms are likely to be exogenous in terms of the timing of their occurrence, their frequency in a given year, and the physical severity of a given strike.

There is potentially valid skepticism about the selectivity associated with storms, as they do strike tropical island countries more frequently than those in the rest of the world. For example, it could be that island countries in the Atlantic Ocean face more storms than those in the Pacific Ocean. Note that we capture the differential level of the time-invariant component of the storm risk across countries by controlling for country fixed effects. Nonetheless, we also check our benchmark estimates by imposing additional control of ocean-fixed effects (that is, we group all countries that are surrounded by the same ocean, assuming that countries in the same ocean a share similar levels of storm risk in terms of frequency), and our findings remain qualitatively the same (see Appendix A5).

Figure 2: Storms: Islands, Landlocked Countries, and Coastal Countries



Notes: This figure depicts the comparative intensity of storms with a human cost, which is calculated as the percentage share of the total affected people in storms in a given year, across the samples of island, landlocked and coastal countries. These percentile shares are weighted by the number of countries in each group.

3.3. Treatment and comparison groups

Given the exogeneity of storm shocks, we now discuss the treatment and comparison groups of island countries. Although all islands are fully exposed to oceans, not all island countries are hit by storms. Our dataset includes 47 islands, of which 35 have experienced at least one storm since 1950; the remaining 12 countries have not experienced a storm since 1950.[19] Thus, we divide island countries into two groups: countries treated with storms (i.e., the treatment group) and those with no storms (i.e., the comparison group) during our sample period.

Two sources of comparability problems can exist between island countries *with* and *without* storms. First, some storm-prone countries may have developed specific institutions to

[19] Of the 42 landlocked countries, 27 had faced storms since 1950 and the remainder had not. Similarly, of the 106 countries with coastal belts, 80 had been hit by storms since 1950, while the rest had not.

cope with storms. If this institutional variation across storm-prone countries is systematically related to the quality of democracy, then our estimation is unlikely to capture the unbiased effect of storm shocks. This concern is alleviated by the fact that we exploit the random variation in storms' *timing* and *intensity*. An additional mitigating factor is that even if politicians expect one or more storms in a given year based on the country's storm history, they cannot predict the number of storms or their severity. However, these arguments are likely to rule out endogeneity only within the treatment group. To address selection based on the storm history (or lack thereof) of storm-free islands, our estimations control for an array of time-invariant differences, year fixed effects, and country-specific time trends. In an additional exercise, we exclude all storm-free island countries from our sample. It turns out that exploiting only the time variation within storm-prone island countries is sufficient to obtain our key results; see Section 5.6.

Second, islands may not be comparable in terms of their land size. For example, Australia is much larger than Jamaica. Thus, we exclude from our benchmark sample all island countries with a land area greater than 1,000,000 km^2. We also drop island countries smaller than 500km^2.[20]

Turning to the differences between island, landlocked and coastal countries, we revisit Table 1. While we remain cautious about drawing any conclusions based on these descriptive statistics, we observe statistically significantly different patterns in landlocked and coastal countries. Island countries seem to be more repressive than landlocked and coastal countries. They also receive more disaster aid in years with storms than their counterparts. In terms of time-varying characteristics, the real per capita GDPs of island countries are not statistically different from those of landlocked and coastal countries in years with storms. In addition, the openness levels of island countries are not statistically different from those of coastal countries. Empirically, these patterns across country groups are not entirely surprising, because there are permanent differences between island and non-island countries. Our country fixed effects, common-time effects, and country-specific time trends are expected to eliminate an array of such endogenous country characteristics related to countries' political trajectories and economic conditions.

[20] Another type of selection bias may arise if the declining income levels in developed countries around the globe lead to a decline in the number of tourists in island countries. In this case, all island countries could face differential but substantive decreases in the value of their tourism sectors. Controlling for year fixed effects and log real GDP per capita takes care of this possible selection.

To check whether the endogenous variation in country characteristics is indeed eliminated after accounting for all country fixed effects, year fixed effects, and country-specific time trends, Table 2 tests if the log real GDP per capita and openness to trade, which may affect political outcomes differentially across treatment and comparison groups, are likely to constitute confounding covariates in the relationship between storms and democracy. Column (1) shows a significant relationship between storms, measured using four different EM-DAT- and IBTrACS-based indicators, and log real GDP per capita. However, as is evident from columns (2) and (3), the effect of storms on the log real per capita GDP mostly disappears once we control for permanent country characteristics, year fixed effects and country-specific time trends. The only storm indicator that has a significant link with log GDP per capita is the EM-DAT-based frequency indicator, with a negative sign that is significant at the 10% level, see column (3). This result is not surprising because, as mentioned, EM-DAT picks up mostly major storms, which suggests an adverse effect on the level of income, thus pointing to the need to control for this covariate in storm regressions. Meanwhile, columns (4), (5) and (6) indicate no discernible relationship between storms and openness levels. We interpret these findings as evidence, at least initially, that island countries in storm-prone and storm-free zones become reasonably comparable once the geographic and temporal fixed factors and country-specific trends are completely isolated.

Note that our identification strategy does not require the levels of our polity measure between island countries *with* and *without* storms to be equal in the absence of such shocks. Rather, it assumes that the changes in the level of the polity measure between storm-prone and storm-free island countries could have trended similarly in the absence of storms. We cannot check the validity of this assumption directly under our randomly-assigned multiple treatment setting. Thus, we undertake an 'event study analysis' to trace out the trend in the polity measure of democracy year by year for the periods leading up to and preceding years with storms. Figure 3 presents a schematic of our event study design.

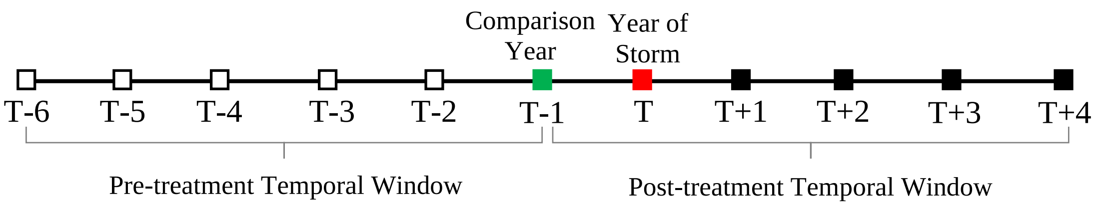
Table 2: Time-Varying Characteristics of Island Countries and Storm History

	Log real GDP per capita	**Log real GDP per capita**	**Log real GDP per capita**	**Openness**	**Openness**	**Openness**
	(1)	(2)	(3)	(4)	(5)	(6)
EM-DAT: Storm indicator (1 = Yes), *t*	0.226***	-0.010	-0.016	2.279	2.782*	1.812
	(0.046)	(0.024)	(0.025)	(1.974)	(1.427)	(1.207)

EM-DAT: Frequency of storms, *t*	0.078*** (0.017)	-0.034* (0.020)	-0.036* (0.020)	0.773 (0.712)	0.783 (0.782)	0.129 (0.638)
IBTrACS: Frequency of storms, *t*	-0.003 (0.002)	-0.002 (0.002)	-0.003 (0.002)	0.116 (0.100)	0.061 (0.073)	-0.020 (0.075)
IBTrACS: Storm Index, *t*	-0.072** (0.035)	-0.035 (0.034)	-0.048 (0.035)	2.279 (1.784)	0.721 (1.139)	-0.517 (1.208)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country time trends	No	Yes	Yes	No	Yes	Yes
Common time effects	No	No	Yes	No	No	Yes
Observations	1772	1772	1772	1772	1772	1772

Notes: Each cell corresponds to coefficients from a separate regression model. Exogenous storm indicators (i.e., binary presence in a year, frequency in a given year, and storm index in a given year) are used. The storm indicator takes a value of 1 if a storm strikes in year *t*, and 0 otherwise. The frequency of storms refers to the total number of storms that occurred in year *t*. The storm index is constructed using the average total wind speed, the deviation of wind pressure from the average atmospheric pressure, and the total storm track length. The estimation method is least squares; heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are given in parentheses (see Bertrand, Duflo and Mullainathan 2004). Significant at the: * 10% level; ** 5% level; *** 1% level.

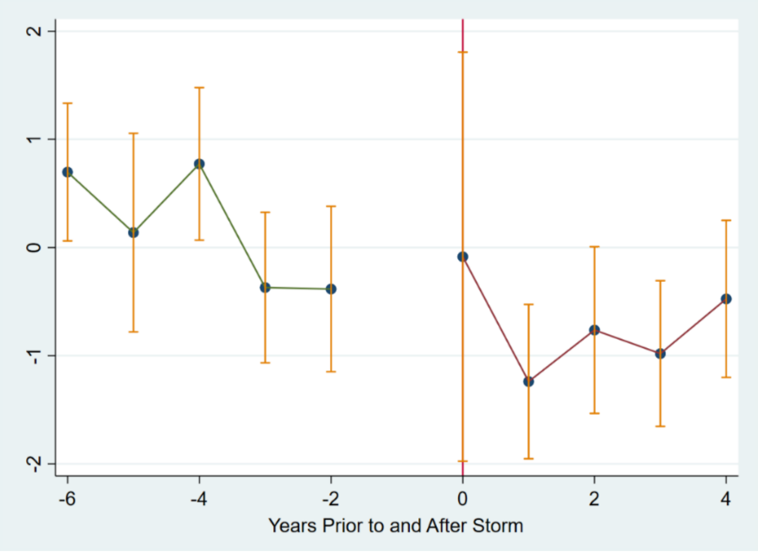
Figure 3: Schematic of Event Study



Notes: The figure shows the schematic of our event study design. T stands for ‘year’. The reference year is (T – 1), which is ‘one year’ prior to storms, which occurred at time T. That is, our pre-treatment period is from (T – 6) to (T – 1), and the post-treatment period from (T – 1) to (T + 4).

Estimating a regression of the *Polity2* score on a set of indicators for the years prior to and following a year of storm(s), along with country fixed effects and country time trends, we observe hardly any pre-existing trend in the *Polity2* score prior to years with storms. This finding is presented in Figure 4, which plots the beta coefficients over time, allowing an inspection of pre- and post-storm changes in the level of democracy. The estimates from this exposition suggest almost no evidence of systematic changes in the patterns of the *Polity2* score in the years prior to storm shocks. However, Figure 4 shows that democracy worsens visibly for a few years following the storm surge. Taken together, conditional on country fixed effects and country-specific time trends, the barely significant pre-storm trends and the presence of significant post-storm effects on democracy confirm that our empirical setting is unlikely to violate the parallel trends assumption.

Figure 4: Event Study Estimates of the Effect of Storms on the Polity Measure of Democracy



Notes: The figure displays the coefficients and 95% confidence intervals. The reference category is 'one year' prior to storms. The coefficients are net of country fixed effects, common time effects, and country-specific time trends.

Taken together, Figures 3 and 4 support the comparability of our treatment and comparison groups, which forms the platform for our empirical analysis.[21]

4. Estimation Method

We identify the effect of storms on the polity measure of democracy in island countries with storms compared to storm-free island countries by performing a generalized DID estimation. Given our panel data setting, the generalized DID model can be specified as follows:

$$(1) \quad Polity2_{i,t} = \alpha_i + \rho_i t + \emptyset_t + \delta_1 Storm_{i,t-1} + \delta_2 X_{i,t-1} + \varepsilon_{i,t},$$

where $Polity2_{i,t}$ is the polity measure of democracy, and $Storm_{i,t-1}$ represents the storm shock (i.e., indicators from both EM-DAT and IBTrACS) for country *i* and year *t*. α_i stands for country-specific fixed effects, $\rho_i t$ stands for country-specific time trends, $\emptyset_t$ is year fixed effects, $X_{i,t-1}$ are time-varying country covariates (i.e., log GDP per capita and trade openness), and ε is the disturbance term.

[21] In Appendix A6, we plot our democracy measure against the measure of the frequency of storms net of country-fixed effects and country-specific time trends. In particular, as predicted by our game theoretic model, the plot reveals the expected negative relationship between the democracy measure and the frequency of storms.

We include α_i to capture any permanent differences across countries. If storm events are *systematic* rather than *random* that are correlated with time-invariant country characteristics (e.g., specific geographic locations), then the country fixed effects will capture the full gamut of permanent factors that may affect both political conditions and storm events. We also control for country-specific time trends. Due to their open exposure to oceans, most island countries were colonised. Several island countries experienced tutelary relationships with European countries, culminating in European-type electoral politics and semi-autonomous governance prior to independence. This implies that the long-run trajectory of political conditions, as driven by colonial history, may follow a country-specific trend. Year fixed effects account for any unobserved factors that are common to all countries (e.g., the global financial crisis).

Note that if our time-variant storm measure is exogenous and properly captures the dynamic nature of the treatment, then the estimate of λ_1 should not be sensitive to controlling for country-specific time trends, as there will be little variation left for these trends to capture, and any remaining variation will be uncorrelated with our measure of storms. Put another way, the inclusion of these variables means that any changes in the measure of political conditions in the treatment and comparison groups would have been analogous in the absence of storm shocks. This assumption is key to our identification strategy.

In Equation (1), the errors ε might be correlated across time and countries, meaning that the persistence in storm events could induce a time series correlation at the country level. Serial correlation could also appear in the cross-sectional dimension, since a particular major storm event may strike a series of countries simultaneously (e.g., Hurricane Sandy in 2012, Hurricane Georges in 1998 and Hurricane Inez in 1966). We avoid such potential biases in standard errors by using heteroskedasticity and autocorrelation consistent (HAC) standard errors. For the identification assumption of our fixed effects model, we assume the condition of contemporaneous exogeneity, $Cov(StormIndex_{i,t-1}, \varepsilon_{i,t}) = 0$. We do not expect this condition to be violated, given that storms are random events in terms of their timing and intensities and cannot be influenced by past political conditions.

The storm indicator in Equation (1) helps us estimate the average effect of all storm events on democracy. However, important heterogeneities may emerge due to different storm frequencies, with a country with only one storm event in a given year being likely to be affected differently from a country with more storm events. We address this issue by also using the storm frequency for $Storm_{i,t-1}$ in country i and year t.

As was discussed above, the way in which storms are measured may introduce certain biases in the estimation. First, as EM-DAT reports more data from richer countries and the richer countries are less likely to be nondemocratic (see Lipset 1959), our regressions may produce downward-biased storm estimates. Noting also the statistically significant, albeit only at the 10% level, link between the EM-DAT-based storm frequency and log GDP per capita found in Table 2, it is crucial to control for the log GDP per capita in the regressions. Second, the non-random disaster declaration process may cause an upward bias in storm effects if governments declare a state of emergency following storm events that coincide with critical junctures in their political regimes. Third, the argued underreporting of events prior to 1990 in EM-DAT may exclude low-intensity storms, which could lead to an upward bias in storm estimates if more severe storms do indeed lead to nondemocratic outcomes. Fourth, the storm index constructed based on meteorological properties may be associated with an attenuation bias due to measurement error. However, the inclusion of several types of fixed effects and time-varying covariates in our specifications should alleviate these concerns.

In a series of robustness checks of our specification, we control for potential spatial biases and other economic and political variables (i.e., population density, government expenditure, gross capital formation, historical legacies, and adaptation effects), different lags of storms and different trend specifications, and also address omitted variables. These checks are reported in our Supplementary Online Appendix.

5. Estimation Results

5.1. Benchmark results

Column (1) in Table 3 shows that, controlling jointly for time-invariant factors, common time effects and country-specific time trends, a storm presence in the past year (as measured by EM-DAT) reduces the polity score of democracy by 0.70 points in the current year; this estimate is significant at the 1% level. Column (2) also controls for log GDP per capita and trade openness. This exercise leaves the coefficient of storm virtually unaffected, with the estimate standing at –0.689, significant at the 1% level. This result confirms that the estimated democracy effect of major storms is reasonably independent of the log GDP per capita and openness to trade in our specification. These results confirm our hypothesis derived from the theoretical model in Appendix A1, that severe storm shocks provoke nondemocratic tendencies in island countries. The richer specification in column (2) shows that major storms reduce the polity measure of democracy by 3.45 percent on average [(−0.689/20) × 100 × 1].

Columns (3) and (4) of Table 3 provide the results when the storm shock is measured by the EM-DAT-based storm frequency within a given year. The regression results with this measure are largely analogous to those with our initial binary storm presence indicator. In particular, controlling for the full array of country characteristics and covariates, column (4) in Table 3 shows that an additional storm that occurred in the past year reduces the polity score of democracy by 0.254 points in a given year. This estimate suggests a total reduction of *Polity2* by 0.655 for the mean number of storms (0.254 × 2.582), which is comparable to the 0.689 found in column (2).

The estimates in columns (5) to (8) using the IBTrACS data confirm our main finding that storm shocks provoke nondemocratic tendencies in island countries. Columns (5) and (6) show that an additional storm having occurred in the last year reduces the polity score of democracy by 0.033 points. With the average number of storms, of any size, in our dataset being 18.749, a year with storms is therefore followed by a reduced *Polity2* score of 0.618 (0.033 × 18.749) in the following year. This magnitude is comparable to the 0.689 that is obtained in column (2) using the EM-DAT data. Using the storm index in the richest specification, column (8) shows that storms reduce the polity measure of democracy by 4.25 percent on average [(–0.625/20) × 100 × 1.36], which is statistically significant at the 1% level.

Table 3: The Effect of Storms on the Polity Measure of Democracy: Island Countries

	Dependent variable: Polity2, *t*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EM-DAT: Storm indicator (1 = Yes), *t* – 1	-0.700*** (0.229)	-0.689*** (0.229)						
EM-DAT: Frequency of storms, *t* – 1			-0.237* (0.130)	-0.254** (0.128)				
IBTrACS: Frequency of storms, *t* – 1					-0.033** (0.016)	-0.033** (0.016)		

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IBTrACS: Storm Index, $t-1$							-0.617***	-0.625***
							(0.290)	(0.289)
Log of real GDP per capita, $t-1$		-0.442**		-0.526**		-0.476**		-0.473**
		(0.209)		(0.214)		(0.203)		(0.204)
Openness, t-1		-0.001		-0.001		-0.001		-0.001
		(0.003)		(0.003)		(0.003)		(0.003)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Common Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1122	1122	1122	1122	1122	1122	1122	1122

Notes: Exogenous storm indicators (i.e., binary presence in a year, frequency in a given year, and storm index in a given year) are used. The binary storm indicator takes a value of 1 if a storm strikes in year *t*, and 0 otherwise. The storm frequency refers to the total number of storms that occurred in year *t*. The storm index is constructed using the average total wind speed, the deviation of wind pressure from the average atmospheric pressure, the storm frequency, and the total length of storm tracks on land. The dependent variable is the Polity2 score, ranging from −10 to 10 (i.e., autocracy to democracy). The estimation method is least squares; the heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are given in parentheses. Significant at the * 10% level; ** 5% level; *** 1% level.

Overall, considering all of the fixed factors and time-varying covariates being controlled in the regression, the EM-DAT and the IBTrACS datasets deliver relatively similar outcomes, which strongly supports the predictions outlined in our hypothesis. This finding also suggests that our rich specification is likely to avoid the pitfalls associated with storm measures and that the different storm measures tend to capture the distribution of storms similarly, at least in the benchmark model, where we consider the average effect of storms on democracy.

5.2. Falsification test: Coastal and landlocked countries

As was mentioned in Section 3, the proportion of people affected by storms is likely to be higher in island countries than in landlocked and coastal countries, because the adjacent countries may act as geographic guards for non-island countries. If our intuition is correct, we should observe no significant (or at least a weaker) storm–polity nexus in either landlocked or coastal countries. We check these arguments in Table 4. Our estimates using both the EM-DAT and IBTrACS datasets collectively indicate that storms do not have any reasonable effect on democracy in an *average* landlocked and coastal country. This result supports the notion that island countries are the best candidates for analysing the effects of storms because they yield the full-treatment condition in a spatial sense.

Table 4: The Effects of Storms on the Polity Measure of Democracy: Coastal and Landlocked Countries

	Dependent variable: Polity2, *t*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Coastal Countries				Landlocked Countries			
EM-DAT: Storm indicator (1 = Yes), *t* – 1	-0.002				0.396			
	(0.205)				(0.389)			
EM-DAT: Frequency of storms, *t* – 1		0.175				0.302		
		(0.116)				(0.324)		
IBTrACS: Frequency of storms, *t* – 1			-0.019				0.014	
			(0.018)				(0.032)	
IBTrACS: Storm index, *t* – 1				-0.281				0.486
				(0.443)				(0.649)
Log of real GDP per capita, *t* – 1	0.700***	0.706***	0.699***	0.699***	0.195	0.197	0.195	0.195
	(0.147)	(0.147)	(0.147)	(0.146)	(0.213)	(0.213)	(0.211)	(0.211)
Openness, *t* – 1	-0.011***	-0.011***	-0.011***	-0.011***	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Common Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4137	4137	4137	4137	1331	1331	1331	1331

Notes: The dependent variable is the Polity2 score, ranging from −10 to 10 (i.e., autocracy to democracy). Least squares estimation; heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are given in parentheses. Significant at the: * 10% level; ** 5% level; *** 1% level.

5.3. The mechanism: Restricted political rights

We now tackle the potential mechanisms in the estimated reduced-form polity effects of storms shown in Table 3. Our hypothesis points to the 'political repression' mechanism as a potential candidate: storms provoke nondemocratic tendencies in island countries through restrictions on political rights and civil liberties induced by citizens' threat of insurgency. As our theory in Appendix A1 suggests, citizens tend to revolt if there is no (sufficient) post-disaster relief provision. Thus, the government's best option is to provide relief and enforce autocracy by restricting political rights and civil liberties as an equilibrium outcome.[22]

[22] An example of extreme political repression in the wake of a storm comes from the Trujillo period in the Dominican Republic. Trujillo used Cyclone San Zeno in 1930 as an excuse to enact policies that violated human rights, to order the torture and death of political prisoners, and to force poor people to relocate far outside the city after the hurricane destroyed their shelter. Trujillo burned thousands of bodies of alleged disaster victims in the middle of the city "to prevent a public health problem" (Hicks 1946, pp. 43-45).

Table 5 formally checks if restricted political rights are an intermediate outcome for the reduced *Polity2* score. Columns (1) to (4) use *political rights* as the dependent variable. Our estimates in columns (1) and (2) using the EM-DAT-based binary storm indicator and the frequency storm dummy, respectively, show that more severe storms are significantly associated with more restricted political rights in island countries. Columns (3) and (4), using the IBTrACS measure, do not pick up a significant effect on political rights, possibly because this measure includes smaller storms. Columns (5) to (8) consider political rights as an additional covariate in Equation (1). As anticipated, the coefficients of the EM-DAT storm measures lose significance, their significance shifting to the political rights measure (columns (5) and (6)). *Political rights* is estimated with the anticipated negative sign, suggesting that more restricted political rights are followed by weaker constraints on the executive and weaker checks and balances, which the *Polity2* measure captures. These results indicate that major storms affect the *Polity2* measure through the political rights channel, and controlling for such a channel makes the coefficients of storms insignificant. The IBTrACS-based storm measures are estimated to be insignificant in columns (7) and (8), indicating that *political rights* is still significant in explaining *Polity2*. One explanation of this finding could be that once major storms hit, their severity does not matter for political democracy. Appendix A7 carries out the same analysis using the civil liberties measure; while there is evidence of a mechanism for this channel using the EM-DAT measure, the effect is statistically stronger for the political rights channel in Table 5.

We now check if alternative mediators may also explain our reduced-form relationship. One could argue that our storm–polity nexus may work through triggering a *coup*. As island countries are protected to some extent by oceans, the military could be relatively free of external threats compared to land-bordered countries. This may open a window of opportunity for the military to gain people's support, tempting it to overthrow the government. However, columns (1)–(6) of Appendix A8 show that storms are unlikely to trigger a coup. Another potential mediator for explaining the storm–polity nexus is *national elections*. Although national elections generally take place at regular intervals, storms could trigger a critical juncture in the political sphere that may result in the fast-tracking of national elections. Accordingly, we empirically check whether storms affect the occurrence of national elections. Our estimates in columns (7)–(12) of Appendix A8 fail to support such a claim, meaning that storms do not tend to change the calendar of national elections. Furthermore, we check this mechanism with an alternative specification where we augment our benchmark model with an interaction term between our measure of storms and an election dummy (see Appendix A9). We find no evidence supporting the conjunction of elections and storms in affecting the *Polity2* measure.

Table 5: Potential Mechanisms: Political Rights in Island Countries

	(1) FH Political Rights, *t*	(2) FH Political Rights, *t*	(3) FH Political Rights, *t*	(4) FH Political Rights, *t*	(5) Polity2, *t*	(6) Polity2, *t*	(7) Polity2, *t*	(8) Polity2, *t*
EM-DAT: Storm indicator (1 = Yes) *t* – 1	0.197***				0.091			
	(0.058)				(0.165)			
EM-DAT: Frequency of storms, *t* – 1		0.046*				-0.036		
		(0.028)				(0.050)		
IBTrACS: Frequency of storms, *t* – 1			-0.004				0.004	
			(0.003)				(0.009)	
IBTrACS: Storm index, *t* – 1				-0.029				0.086
				(0.054)				(0.177)
FH Political Rights, *t*					-1.800***	-1.794***	-1.795***	-1.796***
					(0.125)	(0.126)	(0.125)	(0.126)
Log of real GDP per capita, *t* – 1	0.103	0.115*	0.108*	0.107*	-0.836***	-0.841***	-0.837***	-0.837***
	(0.066)	(0.067)	(0.062)	(0.062)	(0.182)	(0.179)	(0.183)	(0.183)
Openness, *t* – 1	0.001***	0.001***	0.001***	0.001***	0.003	0.003	0.003	0.003
	(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.002)	(0.002)	(0.002)	(0.002)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Common Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	904	904	904	904	903	903	903	903

Notes: The Freedom House political rights index ranges from 1 to 7 (i.e., most free to least free). The estimation method is least squares; heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are given in parentheses. Significant at the: * 10% level; ** 5% level; *** 1% level.

5.4. Disaster Aid Effects

Foreign aid might play a prominent role in our conceptual mechanism, in that it could, at least partly, finance the government's post-disaster relief, leading to the citizens' tolerance of government's repressive arrangements. This could encourage the incumbent government to tap into post-disaster foreign aid from other countries and international allies. We formally put this argument to the empirical test by regressing disaster-related foreign aid on our storm measures. Since we are interested in the more immediate relationship between storms and foreign aid, we investigate the contemporaneous relationship between the two. As Table 6 shows,[23] our estimates using the EM-DAT measures of storms in columns (1) and (2) and the IBTrACS measures in columns (3) and (4) uniformly indicate that island countries receive

[23] Note that governments' post-storm relief funding may not depend entirely on the receipt of foreign aid; instead, they may reallocate domestic resources to provide citizens with timely post-storm relief assistance.

more disaster aid in the year of storms. All of these estimates are statistically significant at the 1%–10% levels.

Table 6: The Effect of Storms on Disaster-Related Foreign Aid in Island Countries

Dependent Variables:	Disaster Aid/ GDP per capita, *t*				Official Development Assistance/ GDP per capita, *t*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EM-DAT: Storm indicator (1 = Yes), *t*	0.272*				-0.210			
	(0.152)				(0.641)			
EM-DAT: Frequency of storms, *t*		0.397*				0.327		
		(0.218)				(0.261)		
IBTrACS: Frequency of storms, *t*			0.0258**				0.021	
			(0.011)				(0.015)	
IBTrACS: Storm index, *t*				0.519***				0.176
				(0.221)				(0.160)
Log of real GDP per capita, *t*	-0.234	-0.147	-0.228	-0.248	0.833	0.908	0.841	0.831
	(0.274)	(0.267)	(0.250)	(0.242)	(0.970)	(0.991)	(0.957)	(0.960)
Openness, *t*	0.009	0.010*	0.009	0.009	0.038***	0.039***	0.038***	0.039***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.012)	(0.013)	(0.012)	(0.012)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Common Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	982	982	982	982	982	982	982	982

Notes: The dependent variables are the total disaster aid (in thousand USD) from the EM-DAT and the total official development assistance (in thousand USD) from the ODA databases, both scaled with real GDP per capita (USD). The estimation method is least squares; heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are given in parentheses. Significant at the: * 10% level; ** 5% level; *** 1% level.

To better understand the disaster-related foreign aid dynamics in island countries, we test if other types of assistance change after storms. To do so, we regress official development assistance on storms in columns (5) to (8) in Table 6. Our findings show that official development assistance does not change after storm shocks, confirming that only disaster aid increases in the year of storms.

In general, altruistic motives drive foreign countries to extend humanitarian assistance to storm-ravaged countries. We check whether this motive depends on the democratic status of the recipient country by regressing disaster aid on storms after controlling for *Polity2*. We find that storms attract post-disaster aid even after controlling for the political regime of the recipient countries (see Appendix A10). That is, storm-affected countries are likely to receive disaster-related foreign aid, and this is not conditional on their *Polity2* scores.[24]

To complete the loop regarding the effects of storms on democracy via restricted political rights and increased repression, and the funding of this mechanism (at least partially) by post-disaster aid, we next investigate the effects of storms on political rights and other repression measures. Table 7 shows that disaster-related foreign aid reduces political rights significantly at the 1% level (column (1)). While its effect on civil liberties is insignificant, it has a statistically significant effect on the average of political rights and civil liberties at the 1% level (columns (2) and (3)). Column (4) uses a different measure of political repression from Wood and Gibney (2010), and confirms the finding of increased political repression in the year of disaster aid. In stark contrast, columns (5) to (8) indicate that official development assistance has the opposite effect: development assistance to island countries improves political rights and civil liberties and reduces political repression, which may be a condition of such assistance. Overall, these results show that post-disaster aid is likely to perpetuate a specific political repression mechanism that ultimately leads to a deterioration in democracy in island countries.[25]

Table 7: The Effect of Disaster Aid on Political Rights and Civil Liberties in Island Countries

	(1) FH Political Rights, *t*	(2) FH Civil Liberties, *t*	(3) Average of FH PR and CL, *t*	(4) Political Repression	(5) FH Political Rights, *t*	(6) FH Civil Liberties, *t*	(7) Average of FH PR and CL, *t*	(8) Political Repression
Disaster Aid/PCY, *t*	0.038***	0.006	0.022***	0.030*				
	(0.010)	(0.007)	(0.007)	(0.018)				
ODA/PCY, *t*					-0.007**	-0.006***	-0.007***	-0.009**
					(0.003)	(0.002)	(0.002)	(0.004)
Log Real GDP Per Capita, *t*	-0.465**	-0.636***	-0.551***	0.204	-0.471**	-0.642***	-0.556***	0.196
	(0.233)	(0.137)	(0.167)	(0.260)	(0.234)	(0.135)	(0.167)	(0.266)
Openness, *t*	-0.003	-0.005***	-0.004***	0.003	-0.003	-0.005***	-0.004**	0.003
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

[24] Our analysis thus far has used disaster aid and official development assistance scaled by real GDP per capita as the dependent variable. Appendix A11 uses log disaster aid (i.e., without any scaling) and log disaster aid per person in the population, and provides regressions with and without *Polity2* as a control. Our finding of increased post-disaster aid after storms is sustained even after these robustness checks.

[25] In a related study, Nunn and Qian (2014) find that an increase in US food aid increases the incidence and duration of civil conflicts in recipient countries. They refer to an extensive body of literature suggesting that humanitarian aid promotes conflict. Our finding indicates that disaster aid represents a further dimension in the political struggle in recipient countries in the wake of the transfer by reducing political rights and increasing political repression of the polity.

Common Time Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	748	748	748	509	748	748	748	509

The FH Political Rights and Civil Liberties measures range from 1 to 7 (i.e., from the highest to lowest degrees of freedom) The Political Repression measure (Wood and Gibney 2010) ranges from 1 to 5 (i.e., from the lowest to highest degrees of repression). Total disaster aid (in thousand USD) from the EM-DAT and total official development assistance (in thousand USD) from ODA databases are both scaled with real GDP per capita (USD). The estimation method is least squares; heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are given in parentheses. *Significant at the: 1% level; **5% level; ***1% level.

5.5. Scale effects

The effect of storms on nondemocratic conditions may be more pronounced in countries with smaller land areas. If this argument is plausible, our benchmark estimates in Table 3 should be more pronounced—in terms of coefficient sizes and possibly the level of significance—for the sample of small island countries than for their larger counterparts. While it is a challenge to define a small island, we use a threshold of 20,000 km^2, which is roughly the 30^{th} percentile of land areas in our sample. Re-estimating our benchmark specifications—reported in columns (1) and (4) of Table 3—with island countries split into two groups based on a land mass below or above 20,000 km^2, our estimates in columns (1) and (5) of Appendix A12 indicate that the effect of storms occurring in the previous year is to decrease the *Polity2* score by 0.798 points in small islands and by 0.596 points in larger island countries. We obtain similar findings when we replace the binary storm indicator with the storm frequency, as shown in columns (2) and (6). Specifically, the effect of storms on small island countries is slightly larger, but this difference does not seem to be significant in statistical terms. The IBTrACS frequency and storm index data do not display any difference in the sample split above (columns (3), (4), (7) and (8)).

5.6. Fraction of the population affected

Our conceptual framework suggests that the fraction of the population that is affected by storms is important for democratic outcomes. We check if the proportion of the population affected due to storms influences democratic deterioration by pooling all countries (islands, landlocked and coastal) and running a regression of *Polity2* and the proportion of the population affected. Keeping in mind the endogeneity concerns regarding the EM-DAT's population-based measures of storms, Table 8 shows that the share of people rendered homeless by storms is negatively related to the *Polity2* score, an effect that is significant at the 1% level. The shares of killed and otherwise affected people are insignificant, suggesting that a loss of residence or shelter matters most in terms of an effect on political democracy.

Appendix A13 shows that the effect related to population made homeless in storms is mainly driven by small island countries whose land mass is below 20,000 km^2.

Table 8: Population Measure of Storm Severity and Polity Measure of Democracy

	Dependent Variable: Polity2, *t*					
	(1)	(2)	(3)	(4)	(5)	(6)
Percentage share of population killed in storms, *t* – 1	3.074	4.345				
	(4.656)	(4.491)				
Percentage share of population made homeless by storms, *t* – 1			-0.061***	-0.050***		
			(0.019)	(0.018)		
Percentage share of population affected by storms, *t* – 1					-0.006	-0.001
					(0.010)	(0.009)
Log of real GDP per capita, *t* – 1		0.549***		0.545***		0.548***
		(0.142)		(0.142)		(0.142)
Openness, *t* – 1		-0.007***		-0.007***		-0.007***
		(0.002)		(0.002)		(0.008)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Country Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
Common Time Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6594	6594	6594	6594	6594	6594

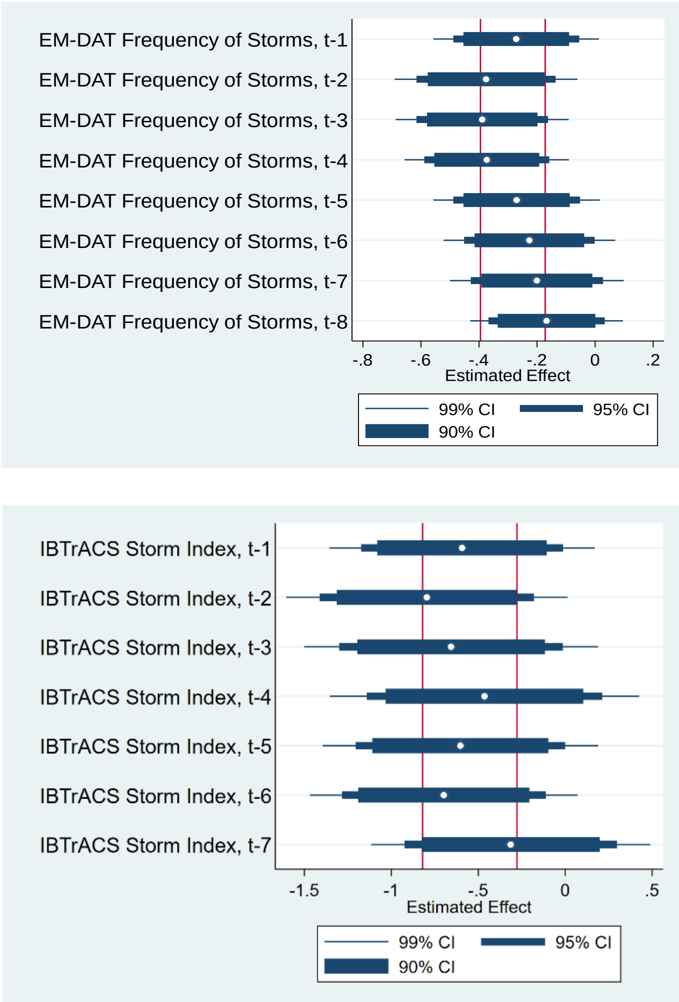
Notes: Least squares estimation; heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are given in parentheses. Significant at the: * 10% level; ** 5% level; *** 1% level.

5.7. Lagged effects

We now investigate whether democracy "recovers" between storms. We do so by empirically testing whether a continued storm effect exists for several years before eventually dying out. In particular, we run our benchmark specification by including up to seven or eight (year) lags of our EM-DAT storm frequency and the IBTrACS storm index measure. As Figure 6 shows, the storms' effect on democracy persists over a six- to seven-year period and the lag effects are generally significant at the 5% level. We find that each additional storm reduces the *Polity2* score by 2.108 points using the EM-DAT data over the next seven years (Appendix A14), and a 1-unit increase in the storm index reduces the *Polity2* score by 3.811 points over the subsequent six years (Appendix A15). In our sample of island countries, the average EM-DAT storm frequency and our constructed storm index are 2.583 and 1.360, respectively, implying that storms reduced the *Polity2* score by 5.445 and 5.183 points (i.e., 24–26%) cumulatively over the subsequent six to seven years over the sample period 1950-

2020. This result sheds light on why island countries that face regular storms tend to remain relatively authoritarian in the longer term (e.g., Haiti, Fiji, and the Philippines).

Figure 6: Lagged Effects of Storms on Democracy



Notes: These figures depict the coefficients associated with the regression model reported in columns (7) and (8) of Appendices A14 and A15, respectively.

5.8. Placebo effects

We run several placebo tests to check whether our estimated polity effect of storms is a mere artefact of statistical correlations. First, we artificially move the binary storm indicator forward by 10 years and construct a placebo storm dummy. We choose the 10-year mark because, as shown above, a storm's effect may last for six to seven years. We then run our benchmark specifications again, replacing the true storm indicator with this dummy. Our estimates, shown in column (1) of Table 9, are statistically insignificant. Similar findings are found using the frequency of storms (see columns (2) and (3)).

Second, we reshuffle the true storm indicator randomly to construct a hypothetical measure of storms and re-estimate our benchmark specifications. The artificially-constructed storm indices do not produce statistically significant results (columns (4)–(6)). In addition, we adopt the Monte Carlo simulation approach and report in square brackets the associated ρ-values drawn from Fisher's two-sided randomization inference test statistic that the placebo coefficients are larger than the actual. In particular, we draw 1,000 subsamples and estimate the regression models to compute ρ-values, which turn out to be statistically insignificant. These estimates indicate that our coefficients are likely to represent the causal relationship between storms and democracy.

Table 9: Placebo Effects

	Dependent variable: Polity2, *t*					
	Forwarding storm timing by 10 years			Reshuffling storm timing randomly		
	(1)	(2)	(3)	(4)	(5)	(6)
EM-DAT: Placebo storm indicator (1 = Yes), *t* + 10	0.067 (0.216)					
EM-DAT: Placebo frequency of storms, *t* + 10		-0.026 (0.133)				
IBTrACS: Placebo frequency of storms, *t* + 10			-0.021 (0.016)			
EM-DAT: Artificially-assigned storm indicator				-0.154 (0.182) *[0.552]*		
EM-DAT: Artificially-assigned frequency of storms					-0.028 (0.166) *[0.856]*	
IBTrACS: Artificially-assigned frequency of storms						-0.015 (0.025) *[0.546]*
Log of real GDP per capita, *t* – 1	-0.669 (0.410)	-0.655 (0.407)	-0.653 (0.416)	-0.456** (0.205)	-0.459** (0.204)	-0.452** (0.205)
Openness, *t* – 1	0.009 (0.006)	0.009 (0.006)	0.008 (0.006)	-0.0009 (0.003)	-0.0009 (0.003)	-0.0009 (0.003)
Observations	1003	1003	1003	1122	1122	1122

Notes: Least squares estimation; heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are in parentheses. All models control for country fixed effects, country time trends and common time effects. Columns (1)–(3) artificially forward each occurrence of storm event by a ten-year period and construct a placebo storm indicator. Columns (4)–(6) reshuffle the true storm indicator randomly to construct a hypothetical measure of storms, i.e., placebo storm measures. We report the ρ-values based on Fisher's two-sided randomization inference test statistic that the placebo coefficients are larger than the actual. That is, we draw 1,000 subsamples using the Monte Carlo permutation approach and estimate the regression models to compute the ρ-values that turn out to be statistically insignificant. Significant at the: * 10% level; ** 5% level; *** 1% level.

5.9. Randomization tests

Our experimental design is based on the effects of storms across island countries. However, island countries are a rather small subset of all countries, which may threaten the credibility of the randomisation due to the small sample or a clustered treatment (i.e., storms always affect a group of countries simultaneously). We adopt a Monte Carlo simulation method similar to those of Fujiiwara and Wantchekon (2013) and Heß (2017) to assess whether our treatment is naturally random. In Table 10, columns (6) and (7) report the ρ-values of the benchmark estimates (given in columns (1)–(4) of Table 3), based on Fisher's two-sided randomisation inference test statistic, that the placebo coefficients are larger than the actual. Specifically, we draw 1,000 subsamples using the Monte Carlo permutation approach and estimate the regression models to compute the ρ-values that turn out to be statistically significant. These randomisation inference tests confirm that our estimates are unlikely to be contaminated due to either a small sample bias or a clustered treatment.

Table 10: Randomisation Inference: The Polity Effect of Storms in Island Countries

	Dependent variable: Polity2,*t*						
					Causal inference ρ-value	Randomisation inference ρ-value: Monte Carlo permutation	Randomisation inference ρ-value: Heß (2017) resampling
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
EM-DAT: Storm indicator (1 = Yes), *t* – 1	-0.689*** (0.229)				0.003	0.002	0.004
EM-DAT: Frequency of storms, *t* – 1		-0.254** (0.128)			0.047	0.025	0.024
IBTrACS: Frequency of storms, *t* – 1			-0.033** (0.016)		0.043	0.058	0.065
IBTrACS: Storm index, *t* – 1				-0.625*** (0.289)	0.030	0.034	0.037
Observations	1122	1122	1122	1122	1122	1000	1000

Notes: The dependent variable is the Polity2 score, ranging from −10 to 10 (i.e., autocracy to democracy). Columns (1)–(4) present the least squares estimation results; heteroskedasticity and autocorrelation consistent (HAC) clustered robust standard errors are given in parentheses. All models control for country fixed effects, country time trends and common time effects. We account for a one-year lag of log real GDP per capita and openness at 2005 constant prices in columns (2) and (4). Column (5) shows the relevant ρ-values of the regression models given in columns (1)–(4). Columns (6) and (7) report ρ-values based on a two-sided randomization inference test statistic that the placebo coefficients are larger than the actual. The ρ-values are computed based on 1,000 random draws using the Monte Carlo permutation approach in column (6) and the Heß (2017) resampling strategy in column (7). Significant at the: * 10% level; **5% level; ***1% level.

5.10. Additional robustness checks

We carry out several other robustness checks of our benchmark results, reported in Appendices A16 to A25. These include testing whether our results are sensitive to controlling for potential spatial biases and other economic and political control variables (Appendices A16–A18), different trend specifications (A19), the reliability of the EM-DAT storm data (A20), exploiting only the time variation in the data (A21), addressing omitted variables (A22), controlling for historical legacies (A23) and "adaptation" effects (A24), and using different sub-components of polity (A25). These checks can be found in the Supplementary Online Appendix attached to this paper.

6. Concluding Remarks

Natural disasters are humankind's greatest killers after wars and disease. We take storms in islands as natural experiments and exploit the random variation in their *timing* and *intensity* to examine whether they provoke nondemocratic conditions in island countries. Focusing on islands enables us to trace the effect of storms on a macro-level outcome. Natural disasters are generally characterized as local events, and their effects are rarely traceable to national outcomes. We overcome this problem by considering island countries: since storms tend to traverse their entire territory, the country ends up fully 'treated'. This makes them a viable cluster to unmask the causal effect of storms on a national outcome of interest, here the polity measure of democracy.

The most significant finding in our analysis is a robust, negative, and stable relationship between storm events and the level of democracy in island countries. We find that the reduction in the *Polity2* score induced by storms in the following year is about four percent. This key finding is supported by our dynamic game-theoretic model, where, in a mutually-agreed political outcome, "storm autocracies" treat the storm shock as an opportunity to gain support from vulnerable citizens by granting them generous disaster relief assistance while simultaneously enforcing a more authoritarian regime. The finding is also supported by the broader literature in political science and economics, where autocratic forms of government are perceived as more efficient at speeding up the decision-making process during emergencies.

Our novel approach to studying the causal link between storms and islands' political institutions is likely to enhance the understanding of the dynamics of deviations from democracy around the world. Our setting suggests that the constituencies of island countries, which are vulnerable to disasters, may be unified in allowing for autocracy in the wake of a storm shock in order to obtain an efficient allocation of relief and recovery assistance and to ensure that the government minimizes plunder, hardship and chaos. The novelty of our approach is that the variations in the timing and intensity of storms in island countries are likely to offer causal evidence for what would otherwise be a highly endogenous political equilibrium due to historical, economic, and cultural factors.

An interesting point which may support our formal theoretical and empirical findings is that the military are often deployed in post-disaster situations to accelerate the emergency response and recovery activities. "Storm autocrats" seem to elevate this "disaster militarism", an increasingly notable component of the public sphere following natural shocks, to the level of political repression. The finding also explains how a ruler exercises greater oppressive authority when citizens turn to the government for relief and assistance in times of hardship. In the face of traditional family relationships breaking down and communities turning to the government for help as a coping mechanism for disasters, "storm autocrats" exploit citizens' vulnerability by employing a stronger nondemocratic orientation.

Our results shed light on why many island countries around the globe remain autocratic over time and address the question of whether a new type of social contract is emerging on the international scene. This aspect of our analysis is important because extreme weather events are increasingly creating new challenges for both states and citizens in the evolution of social contracts. Many island countries and small developing countries do not possess the evolved social contracts against wars, diseases, and potentially disastrous class inequalities, which advanced countries started to develop during the Age of Enlightenment. Instead, island countries' social contracts, if any, have been initiated only in the last few decades. Given that climate change has ushered in a new era of frequent and severe storms around the world, meaning that extreme weather events and the consequent disasters have become the new normal, our paper suggests that the reciprocal rights, obligations, and responsibilities of states and citizens in island countries need to be entrenched in constitutions more strongly than ever.

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