Saving for a rainy day: the impact of storms on saving rates*

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Abstract

Over the period 1950 through 2019, natural disasters worldwide have caused $3.43 trillions in estimated damages, with more than 40% of the damage attributed to storms (EM-DAT). While it is important to apprehend how people recover from these disasters, little is known about the role of saving in the process of disaster recovery. This paper addresses the dynamic effect on private saving rates of storm events occurring as the sole natural disaster in a given country-year. To empirically investigate the saving behavior induced by storms, we employ an event study design using a country-year panel dataset that merged data from the Penn World Table with the EM-DAT database for 176 countries in a 69-year long period. We find a decrease in annual private saving rates by 1.85 and 2.29 percentage points four and five years following storms, respectively. Further checks reveal that storms slow down per capita labor income growth by about 3 percentage points in the first two post-storm years. For intensely damaging storms, it follows a fall in saving rates by 3.19, 4.98, and 4.59 percentage points in the third, fourth, and fifth post-storm year, respectively. The post-storm propensity to dis-save does not apply to developed countries but to developing countries, where the strike of a storm leads to a drop in saving rates by about 2 percentage points four years later. These heterogeneity-robust findings suggest the need for pro-saving policies in countries prone to intense storm damages, reconstruction jobs, insurance, credit, and investments in protections against storms, especially in developing countries.

Keywords: Disaster, Savings, Event study

JEL: D81, Q54, E21, C23

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1 Introduction

Understanding how people cope with disaster risk remains central to identifying appropriate public policies that would support preparedness for and recovery from major natural disasters around the world. Over the period 1950 through 2019, there have been 14,532 natural disasters—encompassing floods, storms, epidemics, earthquakes, droughts, landslides, extreme temperature events, wildfires, and other events—worldwide, affecting almost the entire global population, killing more than 7.53 million people, and causing more than $3.43 trillion in estimated damages, with more than 40% of the damage attributed to storms.[1] Hallegatte *et al.* (2016) find that, on average, global natural disaster-related asset losses reduce the affected country's national income by 60%. Because natural disasters destroy livelihood assets and generate income losses, they lead individuals and nations to some coping strategies which include saving, borrowing, and insuring against asset and income losses. As stated in Clarke and Dercon (2016), financial instruments are necessary to cope with disaster-induced risk, but they remain under-studied. Saving behavioral responses to risk have been addressed in the theoretical and empirical literature, while leaving a mixed answer to the question: Do natural disasters affect saving rates?

Two strands of the theoretical literature, namely the precautionary saving hypothesis and the buffer stock saving hypothesis, address the relationship between natural disasters and savings. The precautionary saving hypothesis stipulates that, when facing uncertainty about future earnings, agents reduce current consumption but increase current savings (Dreze and Modigliani, 1972; Kimball, 1990; Leland, 1968; Miller, 1976; Sandmo, 1970). Precautionary savings follow from a convex marginal utility of wealth, which is the case for agents with preferences exhibiting a non-increasing absolute risk of aversion or a constant relative risk aversion (Caballero, 1990): as the level of risk increases, agents consume less and save more in response to the risk. Following this hypothesis, it can be expected a higher share of income that is saved prior to a disaster, and probably a lower share after the disaster has passed. However, precautionary motives have been criticized by making the point that agents facing income uncertainty exhibit both prudence and impatience.

According to the buffer-stock theory of saving (Carroll, 1997; Carroll *et al.*, 1992; Deaton, 1991), if per capita income is low relatively to some target threshold, agents are prudent and

[1] These statistics are computed from the EM-DAT database (http://www.emdat.be/database). In terms of estimated damages, storms constitute the most devastating disasters. Storms account for 42% of the estimated damages, earthquakes for 24%, floods for 23%, droughts for 5%, wildfires for 3%, and the other disasters for 3%.

have incentives to save in anticipation of the next disaster; but, if per capita income is high relatively to the target threshold, agents are impatient to consume and even borrow for further consumption, which implies dis-saving until the next disaster. Hence, the buffer stock saving hypothesis lends support to the precautionary saving under low income or prudence but rejects it under buffer stock accumulation or impatience.

In addition, in the past two decades, the theoretical literature has evolved, bringing more complexity to the relationship between saving rates and natural disaster occurrence by introducing heterogeneous considerations of risk (Baiardi *et al.*, 2015, 2014, 2020; Eeckhoudt and Schlesinger, 2008; Li, 2012; Liu, 2014; Menegatti, 2015). Labor income risk has a positive effect on the saving rate, while wealth risk, asset risk, or capital income risk has an effect on the saving rate that could be negative, positive, or zero (Gunning, 2010; Vergara, 2017). Thus, the theoretical prediction for the effect of risk on the saving rate is not clear-cut. Depending on the predominant risk induced by natural disasters in a given location at a given time, the effect may be negative, or positive, or even zero.

The empirical literature also provides little evidence to clarify the link between saving rates and natural disasters (Botzen *et al.*, 2019; Kellenberg and Mobarak, 2011; Noy and duPont IV, 2018). Skidmore (2001) identifies a positive effect of (per capita and log) economic losses–as a measure of the probability of future disaster damages–on saving rates. Although this study has the merits of providing the first multi-country evidence about natural disasters and saving behavior and corroborating the precautionary savings motive, the estimation method is just a simple OLS regression of average saving rates on average economic losses and controls over the period 1965-1995 using observations for 14 OCDE countries. The study also makes no distinction between pre-disaster and post-disaster saving rates, and examines several natural disasters of geologic and climatic origin without a heterogeneity analysis. As reviewed in Botzen *et al.* (2019), most empirical studies of the impact of natural disasters on the economy lack a rigorous methodology, leading the authors to suggest the use of experimental or quasi-experimental study designs that make it possible to identify causal mechanisms.

Using a natural experiment and a difference-in-differences approach, Berlemann *et al.* (2015) explore the role of saving behavior as a causal mechanism for the impact of the European flood crisis of August 2002 on economic growth. The authors find a negative post-disaster effect on private saving levels that is significant at the extensive margin in the two to three years following the disaster. The effect on private saving rates is strong with a drop by 65 to 80 percent due to a large receipt of financial aid by disaster-affected individuals—a so-called "Samaritan's dilemma". Other quasi-experimental or experimental studies (Filipski *et al.*, 2019; Fuchs-Schündeln and Schündeln, 2005) also identify rising precautionary savings prior to a disaster or declining saving rates in the aftermath of a disaster, attributed to psychological effects (Filipski *et al.*, 2019) and changes in risk aversion (Fuchs-Schündeln and Schündeln, 2005).[2] While the above empirical literature is in accordance with the precautionary saving hypothesis for natural disasters, Luo and Kinugasa (2020) in a recent study using synthetic controls find declining post-earthquake saving rates in the short-run followed by rising saving rates leading to flat long-run post-disaster effects on saving rates. As reviewed by Lugilde *et al.* (2019), empirical tests of the precautionary saving hypothesis, at macro or micro level, lead to inconclusive results. It remains, therefore, opportune to conduct a rigorous, global study of the dynamic propensity to save relative to the occurrence of natural disaster risk.

The present empirical paper evaluates the causal effect of storm disasters on saving rates using recent advances in the difference-in-differences methodology. We employ the framework of an event study design (De Chaisemartin and d'Haultfoeuille, 2020a; Sun and Abraham, 2021) to estimate the cross-country average treatment effect of storm events on saving rates, using a country-year panel dataset that merged data from the Penn World Table with the EM-DAT database for 176 countries in a 69-year long period. The average saving rate is 35.1% in non-storm years versus 34.1% in storm years. We account for the composition of natural disasters in the country-year when storms occur and exploit variation in the timing of storms as well as in the economic damage that the storms may generate.

Storms could strike countries at different times in the panel, in a discontinuous way. About 84% of the countries get hit by storms in some year. Because storms could occur as the sole natural disaster ("single storms") or as one of multiple natural disasters ("compound storms") in a given country-year, we distinguish the dynamic effects of a single storm event from those of any storm event (single and compound storms inclusive). Across all storms, we find that saving rates drop by 1.87 percentage points in the fifth post-storm year. For single storms, we find evidence of flat saving rates instantaneously at the occurrence of the storm and for the three

[2] Substantial body of the literature, backed by structural models and evidence from experiments and surveys, shows a change in risk preferences, attitudes, and perceptions after large losses, income shocks, or natural disasters (Brown *et al.*, 2018; Cameron and Shah, 2015; Cassar *et al.*, 2017; Eckel *et al.*, 2009; Hanaoka *et al.*, 2018; Page *et al.*, 2014; Reynaud *et al.*, 2013).

following years, but a decrease in saving rates in the next two post-storm years by 1.85 and 2.29 percentage points, respectively.

We examine four main channels that may lead to these results: (1) post-storm increase in disaster-induced risk perceived by people in multiple forms, such as the relative intensity of the storms, the depreciation of assets, unemployment, and the loss of labor income per capita; (2) GDP growth which could decrease saving rates, independently of falling savings levels; (3) rising inflation which could make consumption costlier to afford, and thus, diminish savings levels; and (4) economic development institutions (insurance, credit, and policies) that could build resilience and make saving rates constant in the aftermath of storms. Our analysis points out to the relative intensity of the storms, loss of labor income per capita, and economic development institutions as drivers of the storm effects on saving rates. We find that single storms slow down per capita labor income growth by about 3 percentage points in the first two post-storm years. The decline in saving rates is observed on single storms that generate at least \$4.65 damage per capita, with a fall in saving rates by 3.19, 4.98, and 4.59 percentage points in the third, fourth, and fifth post-storm year, respectively. Moreover, post-storm saving rates are flat in developed countries (statistically insignificant decreases), while the propensity to dis-save following the strike of a single storm in developing countries is about 2 percentage points in the fourth and fifth post-storm years.

Furthermore, all placebo estimations of *ex-ante* treatment effects (De Chaisemartin and d'Haultfoeuille, 2020a) reveal saving rates are steady in pre-storm years, suggesting support to the need for measures for storm preparedness that increase saving rates or provide alternatives to the use of savings to cope with storms.

This paper makes three main contributions to the literature about the impact of natural disasters on saving. First, the paper provides an empirical global evidence of delayed economic effects of natural disasters in lieu of instantaneous treatment effects. The closest global studies are those of Fomby *et al.* (2013) and Cunado and Ferreira (2014) on the delayed positive effects of floods on agricultural growth (one year after the disaster event) and non-agricultural growth (two to four years after the disaster event) in developing countries. Fomby *et al.* (2013) finds a statistically insignificant effect of storms (all inclusive) on GDP growth, but a delayed positive effect of moderate storms on GDP growth by 0.09 percentage points three years after the storm, arising one year after a positive effect on agricultural growth by 0.73 percentage points in developing countries. Our analysis shows that the delayed impact of storms on saving rates is preceded by the impact of storms on the growth rate of per capita labor income—the risk of losing labor income.

Second, this paper offers an evaluation of not only the subsequent effects of natural disasters on saving behavior but also the anticipatory effects, while having a global scope across space and time, as well as accounting for compound disasters. Previous empirical evidence has focused on a single or few countries, addressed either the *ex-ante* or the *ex-post* effects, and ignored the possibly differential effects of compound disasters.

Last, the causal inference made in this paper is based on recent advances in event-study design methodologies, thus departing from previous studies in several ways. The empirical estimations do not use a binary treatment variable or a continuous treatment variable for storms. Instead, the paper uses a set of leads and lags around the treatment event with specifications robust to heterogeneous treatment effects (Borusyak *et al.*, 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). The identification strategy also adapts to the discontinuity in storm occurrence by using an appropriate estimator to treatment switching in and out (De Chaisemartin and d'Haultfoeuille, 2020a) and not relying on the estimators for staggered treatment timing.

The rest of the paper is organized as follows. Section 2 describes the data. Section 3 exposes the estimation methods. Section 4 presents the treatment effects on saving rates across all countries. Section 5 provides a discussion of the possible mechanisms behind the results, with further checks in Section 6. Section 7 concludes.

2 Data

We merge population and macroeconomic growth data from the Penn World Table (Feenstra *et al.*, 2015) with natural disaster data covering 183 countries in the world over the period 1950-2019.[3] We compute private saving rates following standard macroeconomic formulas. Due to missing values for saving rates for some countries in certain years, the panel sample is unbalanced but includes all 183 countries, each with 15 to 70 time periods. Only 55 countries have non-missing saving rates for all 70 years. We include observations with non-missing growth rate

[3] Data of PWT 9.1, as available on www.ggdc.net/pwt. EM-DAT database (http://www.emdat.be/database).

variables (for GDP, inflation, and capital stock) and exclude countries which experienced storms in 1951,[4] leading to a study sample of 9,856 country-year observations covering 176 countries over the period 1951 through 2019.

Table 1 describes the demographic and economic characteristics (Panel A) and disaster characteristics (Panel B) of the study sample. Two types of natural disasters are the most frequent in the average country: floods every four years and storms every six years. The least frequent types of natural disasters that occurred are epidemics (9.5%), earthquakes (6.2%), droughts (6.1%), etc. The average country has a population of 31 million people, a per-capita GDP of $13K growing at the rate of 2.6%, an inflation rate of 3.8%, and a saving rate of 35%. In the last thirty years, the distribution of saving rates by year shows a median saving rate around 34% and an interquartile range of 9 to 13% (Figure 1).

In the study sample, 147 countries experience storm disasters at some point in some year, versus 29 countries never experiencing them in the study sample. No country is always hit by storm disasters every year. As shown in Table A.1, relative to when storm disasters do not occur, the average saving rate across countries in times of storm occurrence is almost the same (34% versus 35%). However, Figure 2 shows a lower median and a smaller interquartile range of saving rates under storm occurrence as compared to storm non-occurrence. Using Fomby *et al.* (2013)'s definitions of intensity and severity of natural disasters, 32% of the storm disasters in the sample are "intense storms" in the sense that the adjusted number of deaths and people affected by the storm is greater than 0.01% of the population, and only 7% of the storm disasters are "severe storms" because the same adjusted number is greater than 1% of the population (Table A.1). Storms usually occur in storm-prone countries. Computing the probability of being hit by a storm disaster as the ratio of the number of years the country has been hit by a storm to the number of years it is present in the full sample, Table A.1 shows the probability of being hit by a storm disaster is greater where storms occur (33%) than where they do not (11%).

Another feature of the data relates to compound natural disasters. Out of 147 countries hit by storm disasters in some year, 118 countries experience other disaster types in combination with storms in the same year. Table A.1 shows in years when storms occur in the countries, there is a variety of disasters occurring in the same years: floods (45%), epidemics (13%), earthquakes

[4] Four countries (Japan, Mexico, Philippines, and Taiwan), despite not treated with storms every year, are always-treated countries in the staggered sense because the time since the year of first storm in the study sample will always be non-negative. We would not have good counterfactuals for the treated observations in such countries.

(11%), droughts (10%), and other disasters (23%). Because disaster occurrence and saving rates are observed on an annual basis, the presence of other disasters when we are only interested in the effect of storms makes treatment and control groups less pure than desired. To account for this problem, we assess two treatments: (1) any storm (all inclusive), and (2) single storm. The effects of the second treatment are estimated after removing the potential confounding effects of compound storms from the effects of the first treatment.

Table 1: Summary statistics of demographic, economic, and disaster characteristics

	Obs	Min	Mean	SD	Max
Panel A: Demographic and economic characteristics					
Population (in millions)	9856	0.00443	30.52	118.7	1433.8
Expenditure-side real GDP at current PPPs (in billions 2011 USD)	9856	0.0225	289.5	1227.4	20791.4
Annual growth rate of expenditure-side real GDP (percent)	9856	-81.14	4.468	8.805	153.2
Per-capita expenditure-side real GDP at current PPPs (2011 USD p.c.)	9856	242.2	13309.7	19639.8	300354.2
Annual growth rate of per-capita expenditure-side real GDP (percent)	9856	-80.85	2.607	8.549	143.5
Annual price inflation rate (percent)	9856	-76.11	3.808	14.06	355.7
Average depreciation rate of the capital stock	9856	0.0125	0.0422	0.0128	0.1000
Employment rate (percent)	9067	4.323	38.47	9.033	76.34
Annual growth rate of per-capita real labor income (percent)	7124	-80.85	2.529	8.429	103.9
Real private savings at current PPPs (in billions 2011 USD)	9856	0.0103	103.9	470.4	10415.7
Private savings rate	9856	0.00376	0.349	0.119	0.996
Panel B: Disaster characteristics					
Flood occurred (1=yes, 0=no)	9856	0	0.249	0.432	1
Storm occurred (1=yes, 0=no)	9856	0	0.151	0.358	1
Epidemic occurred (1=yes, 0=no)	9856	0	0.0947	0.293	1
Earthquake occurred (1=yes, 0=no)	9856	0	0.0623	0.242	1
Drought occurred (1=yes, 0=no)	9856	0	0.0610	0.239	1
Other disaster type occurred (1=yes, 0=no)	9856	0	0.121	0.326	1

Notes: This table describes the study panel sample composed of 176 countries, each with 14 to 69 annual records. Private saving rate is the ratio of real private savings to expenditure-side real GDP. Five countries have GDP growth rates lower than -50%: Georgia (1992), Iraq (1991), Lebanon (1976), Rwanda (1994), and Venezuela (2015, 2016, and 2017). Three countries have GDP growth rates higher than 100%: Equatorial Guinea (1997), Kuwait (1974), and Liberia (1997). Inflation rate is the growth rate of price level of household consumption, with price level for USA in 2017=1. There are fifteen countries with inflation rates lower than -50%: Angola (1996 and 1998), Argentina (2002), Bulgaria (1991), Brunei Darussalam (1981), Dominican Republic (1985), Iran (1985), Iraq (1993), Lebanon (1984), Mozambique (1987), Mauritania (1963), Malawi (1994), Seychelles (1995), Suriname (1989), Syrian Arab Republic (1985), and Zimbabwe (2006). There are eleven countries with inflation rates higher than 100%: Angola (1992, 1997, and 1999), Brunei Darussalam (1974 and 1980), Djibouti (1994), Equatorial Guinea (2002), Lebanon (1988), Mauritania (1961), Saudi Arabia (1974), Seychelles (1994), United Arab Emirates (1974), Venezuela (2015, 2016, and 2017), and Zimbabwe (2004, and 2005). Per capita labor income is the product of per capita GDP and the share of labor compensation in GDP. GDP growth, per-capita labor income growth, and inflation rates are not computed for the initial year of each country. Disaster types other than those five listed in the table include extreme temperature, landslide, wildfire, volcanic activity, mass movement (dry), insect infestation, etc.

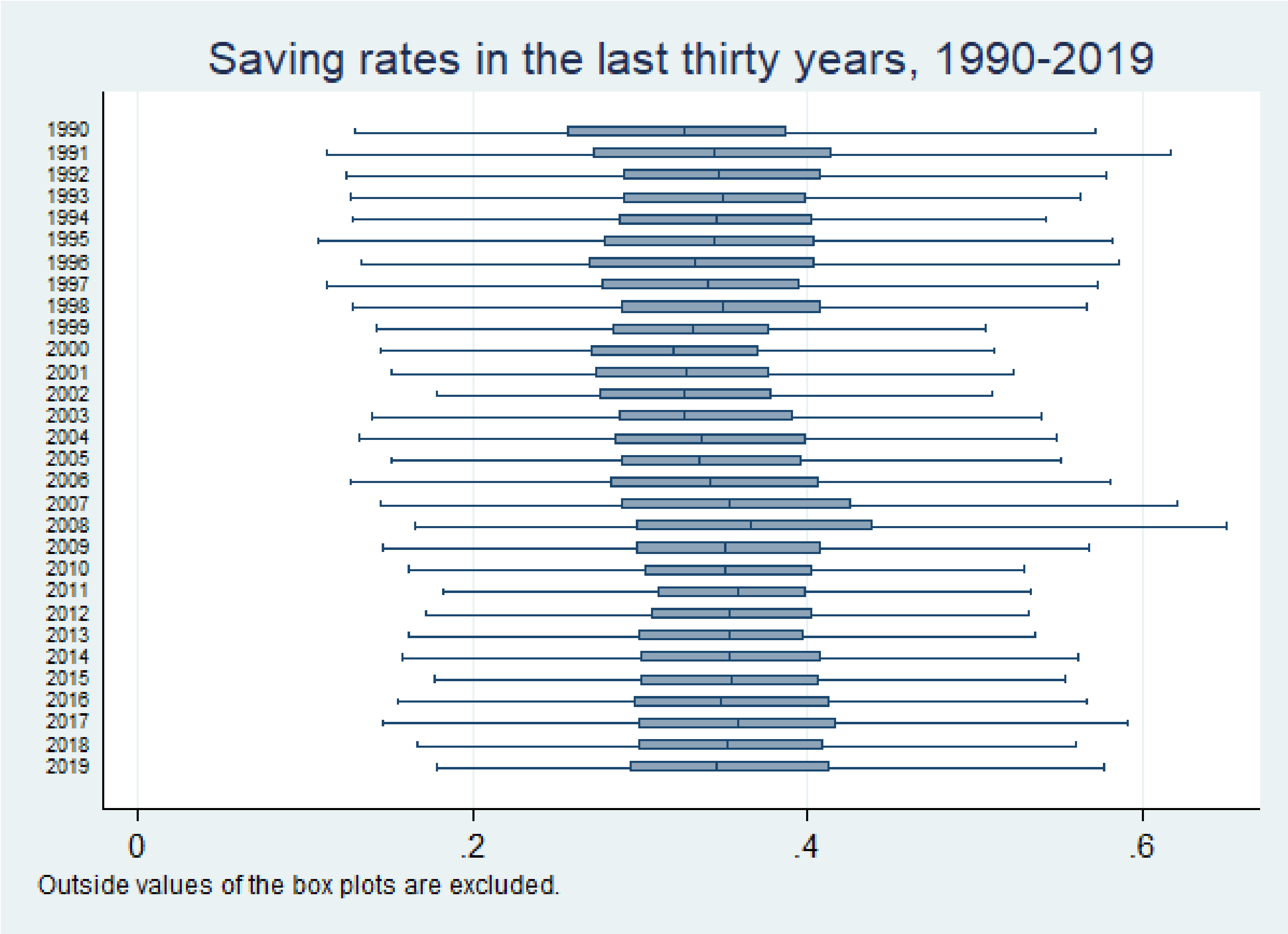


Figure 1: Saving rates in the last thirty years (1990-2019)

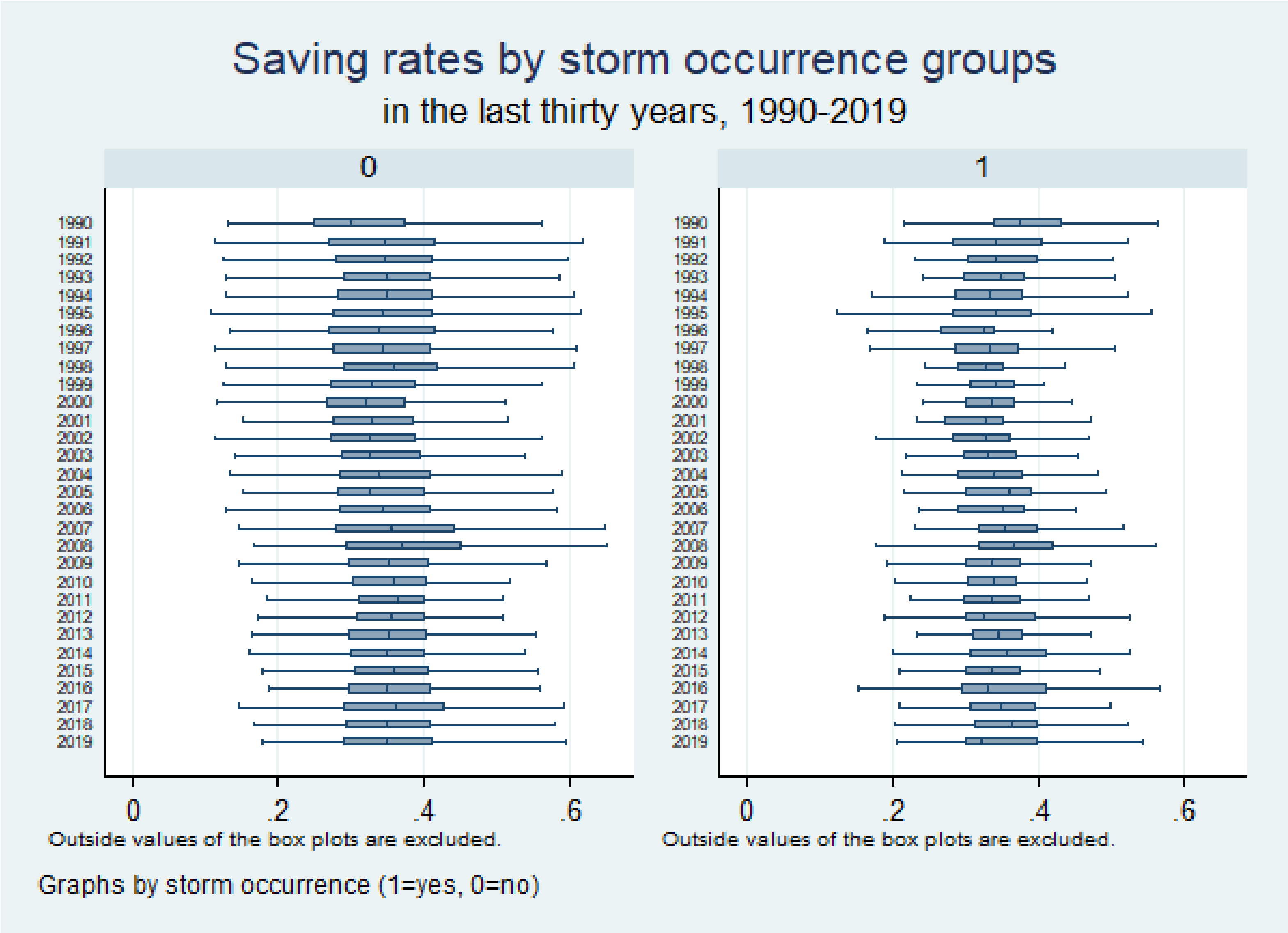


Figure 2: Saving rates in the last thirty years (1990-2019), by storm occurrence groups

3 Empirical strategy

To empirically evaluate the causal effect of storms on saving rates, we use a methodological approach that relies on the event study design (Borusyak *et al.*, 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). The goal of this research design is to compare saving rates within countries before and after the occurrence of the natural disaster of interest, while allowing for countries to get hit by the natural disaster at varying and multiple times. With the use of the time of disaster occurrence as a binary treatment indicator, a before-after difference could be interpreted as causal if there are no systematic changes within countries over time except for the natural disaster. But the generalized event study specification uses pre-treatment and post-treatment indicators (relative-time indicators).

Consider a panel of N countries observed for T years. The savings rate outcome $O_{i,t}$ is observed for each $i \in \{1, ..., N\}$ and $t \in \{1, ..., T\}$. The indicator variable for being treated–hit by the natural disaster event of interest–is denoted $D_{i,t} \in \{0, 1\}$. Let denote E_i the time when country i initially becomes treated and $K_{i,t} = t - E_i$ the number of years relative to E_i. Within the event study design framework, a dynamic two-way fixed effects (TWFE) regression to estimate the dynamic treatment effect of floods and storms on saving rates is specified as follows:

$$O_{i,t} = \alpha_i + \beta_t + \sum_{k=-H}^{-2} \delta_k \mathbf{1}\left[K_{i,t} = k\right] + \sum_{k=0}^{L} \gamma_k \mathbf{1}\left[K_{i,t} = k\right] + \varepsilon_{i,t}, \qquad H, L \leq T. \tag{1}$$

The specification in Eq.1 illustrates a regression of country-year saving rates $O_{i,t}$ using country fixed effects α_i and year fixed effects β_t. The coefficients δ_k corresponding to the time periods leading up to the treatment ("leads" or "pre-trends") indicate the incidence on saving rates k years before the disaster occurs and the coefficients γ_k corresponding to the time periods following the treatment ("lags") indicate the incidence on saving rates k years after the disaster occurs. The coefficient γ_0 indicates the instantaneous effect of disaster occurrence on saving rates in the year that the country is hit relative to the year before the treatment. All coefficients are relative to the baseline reference "lead1" (δ_{-1}), which is omitted in Eq.1.

In the study sample, the longest leading time is $H = 61$ years, and the longest lagged time is $L = 68$ years. Longer horizons are meaningless to interpret because on average storms occur every six years (Table 1). Hence, we estimate Eq.2 which is Eq.1 using the time window $[-5, 5]$

and binning leads and lags at the new endpoints H and L.[5]

$$O_{i,t} = \alpha_i + \beta_t + \delta_{-H}\mathbf{1}\left[K_{i,t} \leq -H\right] + \sum_{k=-H+1}^{-2} \delta_k \mathbf{1}\left[K_{i,t} = k\right] + \sum_{k=0}^{L-1} \gamma_k \mathbf{1}\left[K_{i,t} = k\right] + \gamma_L \mathbf{1}\left[K_{i,t} \geq L\right] + \varepsilon_{i,t}. \tag{2}$$

Under standard assumptions of parallel trends,[6] no anticipatory effects,[7] and homogeneity of treatment effects,[8] TWFE estimates are unbiased. However, this is rare in practice. There could be heterogeneous treatment effects across countries, in time since treatment, or both across countries and in time since treatment (Roth *et al.*, 2022). This is likely to be the case because countries experience neither the same intensity or severity of single disasters across years nor the same composition of compound disasters across years. If homogeneity of the treatment effects is violated, the coefficients δ_k are not valid, and so is the parallel trends test of their joint insignificance (Sun and Abraham, 2021).

Given that TWFE is generally biased under heterogeneous treatment effects, multiple heterogeneity-robust difference-in-differences (DiD) estimators suggested in the literature allow for a proper weighting of the treatment effects by choosing appropriately the counterfactuals to avoid forbidden comparisons in the computation of the treatment effects (Baker *et al.*, 2022; de Chaisemartin and D'Haultfoeuille, 2022; Roth *et al.*, 2022). Due to subtle differences among those estimators,

[5] As discussed in Schmidheiny and Siegloch (2019), binning the treatment indicators at the endpoints is equivalent to assuming the treatment effects stay constant outside the time window of interest; it is a practical "necessity" that reduces the number of parameters to be estimated and enlarges the control group to identify the dynamic treatment effects even when the event study design has no never-treated units.

[6] Following Autor (2003), Rambachan and Roth (2019) and Roth (2020), parallel trends assumption would be satisfied under the non-rejection of the hypothesis that the lead coefficients δ_k in the time window $[-H, -2]$ are jointly insignificant in Eq.2. However, this standard test of event study pre-trends is a subject of debate among difference-in-differences and event study design scholars. For some scholars, obtaining insignificant pre-trends after estimating a linear model does not lend validity to the design because non-linear violations of parallel trends are possible. There is a multiplicity of parallel trends, some of which not requiring pre-trends to be zero (Marcus and Sant'Anna, 2021). For other scholars, parallel pre-trends should be imposed prior to estimating the causal effects rather than tested after estimating the causal effects. Gardner (2021) suggests the use of a two-stage approach to the problem: first obtain the estimates of unit and time fixed effects on outcomes using the untreated sample, then regress the demeaned outcomes on the lags and leads for a valid identification strategy. Borusyak *et al.* (2021)'s imputation estimator employs the same approach as the one in Gardner (2021).

[7] Following Abbring (2003), Abbring and Van den Berg (2003), Heckman and Navarro (2007), Abbring and Heckman (2007), and Abbring and Heckman (2008), the no-anticipation assumption would require that countries' current saving rates depend only on past disaster occurrences and saving rates, but not on the countries' forward-looking behavior toward disasters and savings. We could argue that it holds in this study. Although certain countries may be prone to certain disasters, form priors on the odds of being hit in the near future, and issue timely watches and warnings, countries cannot perfectly foresee when they would be hit by natural disasters and how they would adjust saving rates in the future.

[8] Homogeneity of treatment effects requires that all countries have the same treatment effect at every relative-time k, among other requirements. As shown by an endless list of authors (Borusyak *et al.*, 2021; De Chaisemartin and d'Haultfoeuille, 2020a; Goodman-Bacon, 2021; Sun and Abraham, 2021), the coefficients δ_k and γ_k in Eq.2 are weighted averages across countries and periods of $\delta_{k,i,t}$ and $\gamma_{k,i,t}$, respectively, requiring non-negative weights summing up to one; thus, homogeneity systematically fails if weights are negative. The "underweighting" arises because selection into treatment occurs at different timings, and comparing "later" treated to "earlier" treated countries ("forbidden comparisons") creates contamination in the treatment effects (Goodman-Bacon, 2021).

it is important to make clear how the variation in the timing of the treatment assignment justifies the choice of the estimator, and be transparent about which countries are being compared for identification.

Most heterogeneity-robust estimators (Borusyak *et al.*, 2021; Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020b; Gardner, 2021; Sun and Abraham, 2021) apply to the general case of staggered treatment timing when units (countries) switch in treatment at different periods (years) but stay treated afterwards; that is, once the treatment turns on, it is forever on. In this sense, the coefficient γ_k captures the average effect of having started to experience the natural disaster of interest k years ago; in other words, it is the cumulative effect of experiencing the natural disaster of interest for k years. Callaway and Sant'Anna (2021)'s and De Chaisemartin and d'Haultfoeuille (2020b)'s DiD estimators use the not-yet-treated or the never-treated units as the controls. Sun and Abraham (2021)'s DiD estimator uses the units treated last or the never-treated units as the controls. Several other heterogeneity-robust DiD estimators for staggered treatment timing include Borusyak *et al.* (2021), Caetano *et al.* (2022), Gardner (2021), Wooldridge (2021), etc., with some proposing to use strictly exogenous, time-varying covariates in the regressions to satisfy parallel trends conditional on these covariates.

Another estimator De Chaisemartin and d'Haultfoeuille (2020a) applies to more complicated cases of treatment timing when units may switch in and out of the treatment at any particular period; that is, the treatment is allowed to turn on and off. In this sense, the coefficient γ_k captures the average effect of having switched to experience the natural disaster of interest for the first time k years ago. With this estimator, each γ_k results from the canonical 2x2 DiD comparing first-time switchers k years ago to not-yet switchers. Moreover, De Chaisemartin and d'Haultfoeuille (2020a)'s estimator computes δ_k as the placebo estimate of the difference in outcome evolution between the same two groups $k + 1$ years ago (before countries switching treatment do so). While the joint insignificance of the placebo estimates δ_k represents a robust test of the parallel trends assumption, these placebo estimates provide an evaluation *ex-ante* of future treatment (De Chaisemartin and d'Haultfoeuille, 2020a).

Our preferred estimates rely on De Chaisemartin and d'Haultfoeuille (2020a)'s estimator because the occurrence of disasters turns on and off in the countries over the years in the sample. Making first no difference between single and compound storms, we estimate De Chaisemartin and d'Haultfoeuille (2020a)'s robust dynamic treatment effects of storms on saving rates.

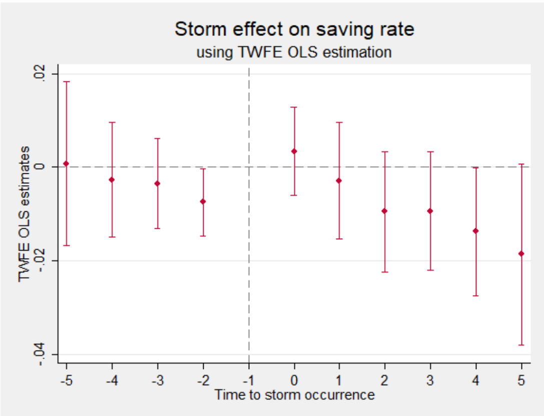
Further, we estimate robust dynamic effects of a refined treatment variable that captures the occurrence of storms as the sole natural disaster in the year. To apprehend the mechanisms driving the results, we conduct estimations for additional outcomes: total damage per capita, ratio of disaster damage to GDP, average rate of depreciation of capital stock, employment rate, per capita labor income growth, GDP growth, and inflation rates.

4 Results

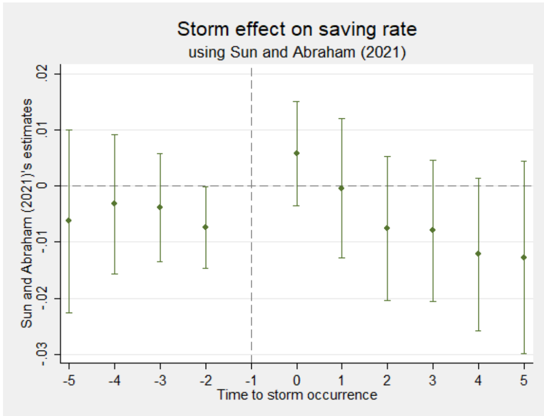
4.1 TWFE on saving rates and heterogeneity-robust effects for staggered treatment timing

Figure 3a presents the storm (all inclusive) event study plot for TWFE OLS, with its counterpart plot correcting for heterogeneity in treatment effects using Sun and Abraham (2021)'s estimator in Figure 3b. The estimated coefficients are shown in columns 1-2 of Table 2. There are two salient patterns in these results. First, treatment effects (lag coefficients) are insignificant up to the third post-storm year, and the decreasing effects by 1.2 to 1.9 percentage points in later post-storm years are only significant at the 10% level. Second, although a significant difference in saving rates between storm and non-storm years by 0.7 percentage points is observed one year prior to the baseline (lead2 coefficient), all lead coefficients are jointly insignificant. However, this non-violation of the parallel pre-trends assumption does not lend validity to the results. Even if one used other staggered adoption DiD estimators, such as Borusyak *et al.* (2021); Callaway and Sant'Anna (2021); De Chaisemartin and d'Haultfoeuille (2020b); Wooldridge (2021), the results would remain invalid for this study because the treatment timing is not staggered.

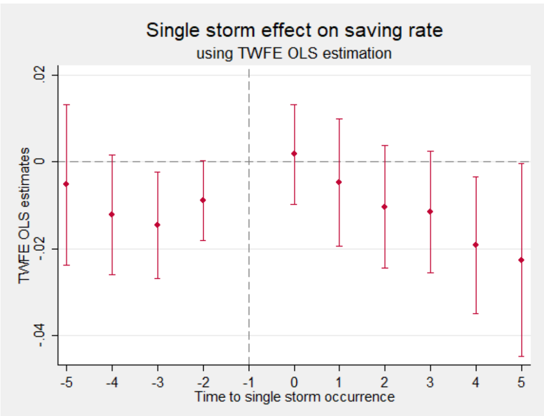
The country-year comparisons of single storms to non-storms using the naive TWFE OLS estimator (column 3 of Table 2 and Figure 3c) and Sun and Abraham (2021)'s estimator (column 4 of Table 2 and Figure 3d) indicate a significant reduction in saving rates by 1.7 to 1.9 percentage points in the fourth post-storm year at the 5% level. However, the parallel pre-trends assumption fails using the heterogeneity-robust estimation, as shown by the rejection of the joint test of significance of the lead coefficients at the 5% level (column 4 of Table 2).



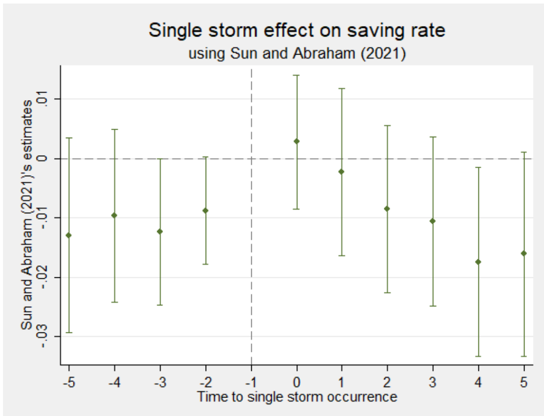
(a) Two-way fixed effects OLS estimator, all storms



(b) Sun and Abraham (2021)'s estimator, all storms



(c) Two-way fixed effects OLS estimator, single storms



(d) Sun and Abraham (2021)'s estimator, single storms

Figure 3: Staggered treatment estimators for the average causal effect of storms on saving rates

Table 2: Average causal effect of storms on saving rates (estimates for staggered treatment timing)

VARIABLES	(1) TWFE	(2) S-A	(3) TWFE	(4) S-A
storm_lead5	0.001	-0.006	-0.005	-0.013
	(0.009)	(0.008)	(0.009)	(0.008)
storm_lead4	-0.003	-0.003	-0.012*	-0.010
	(0.006)	(0.006)	(0.007)	(0.007)
storm_lead3	-0.003	-0.004	-0.015**	-0.012**
	(0.005)	(0.005)	(0.006)	(0.006)
storm_lead2	-0.007**	-0.007**	-0.009*	-0.009*
	(0.004)	(0.004)	(0.005)	(0.005)
storm_lag0	0.003	0.006	0.002	0.003
	(0.005)	(0.005)	(0.006)	(0.006)
storm_lag1	-0.003	-0.000	-0.005	-0.002
	(0.006)	(0.006)	(0.007)	(0.007)
storm_lag2	-0.010	-0.008	-0.010	-0.009
	(0.007)	(0.007)	(0.007)	(0.007)
storm_lag3	-0.009	-0.008	-0.012	-0.011
	(0.006)	(0.006)	(0.007)	(0.007)
storm_lag4	-0.014*	-0.012*	-0.019**	-0.017**
	(0.007)	(0.007)	(0.008)	(0.008)
storm_lag5	-0.019*	-0.013	-0.023**	-0.016*
	(0.010)	(0.009)	(0.011)	(0.009)
Observations	9,856	9,856	8,920	8,920
R-squared	0.566	0.645	0.577	0.642
Baseline	lead1	lead1	lead1	lead1
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Treatment	Any storm	Any storm	Single storm	Single storm
Joint test of lead2-lead5: p-value	0.262	0.240	0.197	0.0234

Notes: This table reports the estimation results on saving rates from Eq.2 using Two-Way Fixed Effects (TWFE in odd-numbered columns) and heterogeneity-robust (Sun and Abraham (2021) specifications denoted S-A in even-numbered columns) for storms. The sample in columns 1-2 is the full sample. The sample in columns 3-4 excludes observations for compound storms. The joint test of significance of all lead coefficients prior to the baseline is the test of parallel pre-trends. Standard errors, in parentheses below the coefficients, are clustered at the country level. Significance levels: *** 1%, ** 5%, * 10%.

4.2 Heterogeneity-robust effects on saving rates for switching in and out of the treatment

Using the appropriate DiD estimator for the variation in timing of disaster occurrence (De Chaisemartin and d'Haultfoeuille, 2020a), we find that saving rates are affected by storms (all inclusive)—not instantaneously, but after a four-year delay (Figure 4). In the fifth post-storm year, the saving rate decreases by 1.87 percentage points (Figure 5, Panel A). Parallel pre-trends is satisfied, with placebo estimates of pre-trends jointly insignificant (Figure 4).

After accounting only for storms that occur as the sole natural disaster in the year, we find constant saving rates in the first three years following these single storms (statistically insignificant decreases), and falling saving rates by 1.85 and 2.29 percentage points in the fourth and fifth post-storm year, respectively (Figure 5, Panel B). Parallel pre-trends for single storms versus non-storms is at the limit of rejection due to positive point-estimates for the placebo effects in the first two years prior to the baseline, preceded by negative point-estimates (Figure 4). Notwithstanding, parallel pre-trends is not rejected when looking at multiple factors that may explain the observed saving behavior in Section 5.

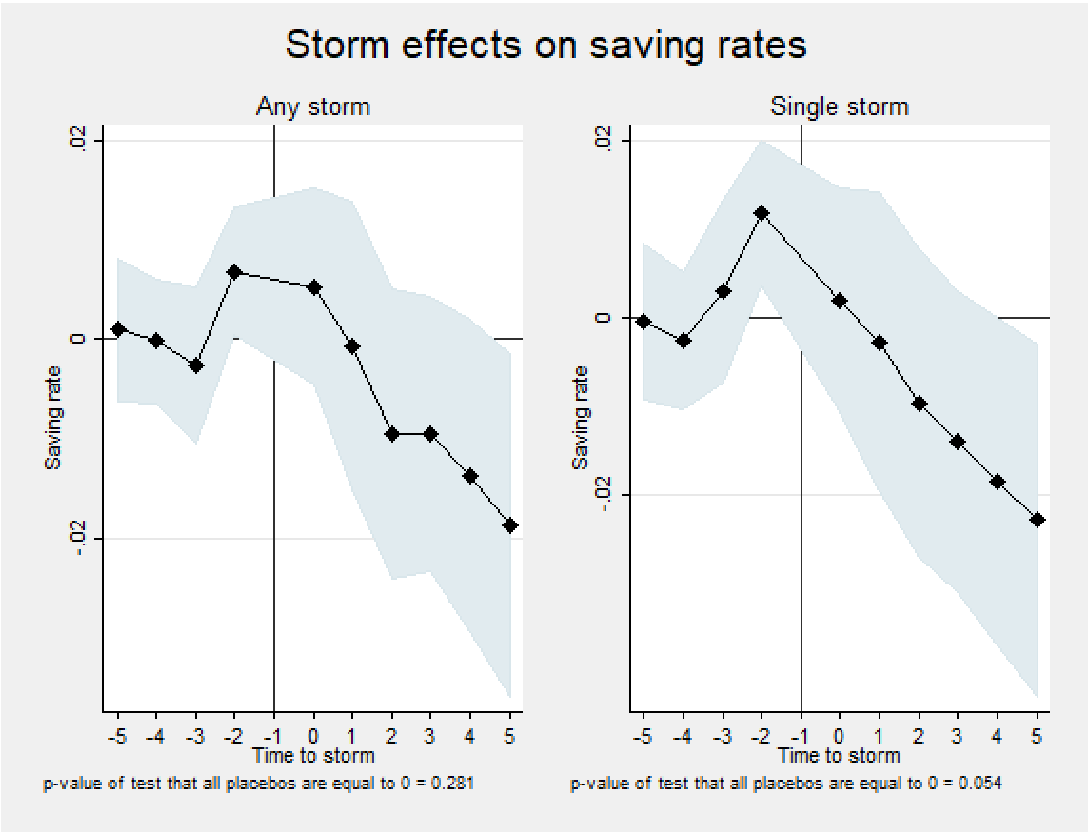


Figure 4: De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on saving rates

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0053188	.0050537	-.0045865	.0152241	3953	143
Effect_1	-.0006613	.0073556	-.0150782	.0137556	3829	143
Effect_2	-.0094058	.0073859	-.0238822	.0050706	3701	143
Effect_3	-.0094488	.0070492	-.0232652	.0043677	3549	141
Effect_4	-.0136397	.0079622	-.0292456	.0019663	3430	141
Effect_5	-.018652	.0087703	-.0358418	-.0014622	3312	141
Placebo_1	.0068501	.0032287	.0005218	.0131783	3787	139
Placebo_2	-.0025295	.0039911	-.0103522	.0052931	3673	137
Placebo_3	-.0001792	.0031658	-.0063842	.0060258	3398	131
Placebo_4	.0010127	.0036559	-.006153	.0081783	3291	129

p-value of test that all placebos are equal to 0 = 0.28120184

Panel A. Treatment: Any storm. Outcome: Saving rate.

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0019851	.0064554	-.0106675	.0146377	4013	113
Effect_1	-.0027954	.0085777	-.0196076	.0140169	3712	109
Effect_2	-.0097712	.0088563	-.0271296	.0075871	3451	104
Effect_3	-.0139997	.0086871	-.0310264	.0030269	3262	99
Effect_4	-.0185328	.0094049	-.0369663	-.0000992	3089	95
Effect_5	-.0228874	.0101656	-.0428119	-.0029628	2935	88
Placebo_1	.01173	.0041636	.0035694	.0198907	3724	104
Placebo_2	.0029418	.0052703	-.007388	.0132717	3558	100
Placebo_3	-.0026194	.0039429	-.0103476	.0051087	3311	97
Placebo_4	-.0005057	.0044738	-.0092743	.0082629	3018	91

p-value of test that all placebos are equal to 0 = 0.05047294

Panel B. Treatment: Single storm. Outcome: Saving rate.

Figure 5: De Chaisemartin and d'Haultfoeuille (2020a)'s robust dynamic treatment effects of storms on saving rates

Notes: This is the output of the Stata command *did_multiplegt* for De Chaisemartin and d'Haultfoeuille (2020a). Each row Effect_# is the result of the heterogeneity-robust DID estimating the effect of having switched in treatment for the first time # years ago. Estimate column = estimated effect of the treatment at the time period when first-time switchers switch. SE column = Standard Error computed using 100 bootstrap replications. LB CI and UB CI columns = Lower Bound and Upper Bound of the 95% confidence interval. N column = total number of observations (first-time and not-yet switchers) used in the estimation of the treatment effect. Switchers column = number of first-time switchers the estimate applies to.

5 Impact mechanisms

In the wake of natural disasters, saving behavior may reflect tangible, direct outputs of the disasters that people perceive, such as the physical destruction of private assets, unemployment, loss of income, increases in prices, and fatalities. Most economic studies of natural disasters have examined the effects of disaster damage per capita (Kellenberg and Mobarak, 2008; Schumacher and Strobl, 2011; Skidmore, 2001), ratio of disaster damage to GDP (Toya and Skidmore, 2007), and intensity measures of disasters based on the death toll (Cunado and Ferreira, 2014; Fomby *et al.*, 2013). In the same vein, we have investigated as a direct impact mechanism the possibly decreasing effect of the intensity of storms on saving rates (see part a of Subsection 5.1 and Subsection 6.1). We use two measures of the relative intensity of storms (total damage per capita and ratio of disaster damage to GDP).[9] In addition, we explore the possibility of several other direct impact mechanisms: (i) decreasing effect of storms on the average rate of depreciation of capital stock—risk of asset loss leading to a fall in saving rates (see Subsection 5.1, part b); (ii) decreasing effect of storms on employment rate—risk of unemployment leading to the decline in saving rates (see Subsection 5.1, part b); (iii) decreasing effect of storms on per capita labor income growth—risk of income loss channel to declining saving rates (see Subsection 5.1, part b); and (iv) increasing effect of storms on inflation rates—rising consumer prices as a channel to the reduction in saving rates (see Subsection 5.2).

Beside disaster-induced risk channels and the inflation rate channel, changes in post-storm saving rates may be the result of three indirect factors. First, disaster aid—also known as "Samaritan's dilemma" or "charity hazard"—constitutes a channel through which post-storm saving rates may fall with no changes in savings levels. At the household level, remittances and transfers in times of disaster, by smoothing consumption, may raise the denominator of private saving rates and, thus, lower private saving rates without even affecting private savings levels. Examples of post-decreasing saving rates due to charity or relief transfers to affected households are numerous in the literature (Andor *et al.*, 2020; Berlemann *et al.*, 2015).[10] At the nation level, it is also possible that countries use debt to finance their disaster preparedness and recovery plans, or that developing countries receive aid from developed countries, which may distort how

[9] We do not rely on the death toll based measures of the intensity of storms because by these measures, 93% of the storms are moderate and only 7% are severe (Table A.1).

[10] This negative effect of the inflow of post-disaster transfers affects not only saving rates but also insurance (Andor *et al.*, 2020; Cai, 2016; Deryugina, 2017; Kousky *et al.*, 2018; Raschky *et al.*, 2013; Raschky and Weck-Hannemann, 2007).

saving rates adjust to natural disasters. Remittances including official development assistance lessen the negative macroeconomic impacts of natural disasters (Hochrainer, 2009; Raschky and Schwindt, 2016; Yang, 2008). We investigate this channel by estimating the treatment effects on per capita GDP growth (see Subsection 5.3), which might follow the same trends as those on GDP growth (see Subsection 6.2).

Second, post-disaster private saving rates could be flat in the short term through three ways: (i) extensive access to insurance against natural disasters; (ii) use of credit to self-finance post-disaster consumption; or (iii) government investments in disaster mitigation and adaptation policies that may delay the asset destruction process and reduce the consequences of natural disasters. Regarding insurance (i), Von Peter *et al.* (2012) points out that uninsured losses rather than insured losses matter the most in building the resilience against natural disaster. However, a recent global study finds that access to private insurance builds resilience against natural disasters (Breckner *et al.*, 2016), which suggests that insuring against income losses could downplay the response to disasters through adjustments in saving behavior and explain the finding about flat post-storm savings rates. Concerning credit (ii), the argument may apply mostly for developing countries, as several studies (De Mel *et al.*, 2012; Deaton, 1992; Fafchamps and Pender, 1997) highlight the role of savings and access to credit as substitutes in consumption smoothing in developing countries. With regard to government interventions (iii), good institutions in general may make it possible for people to recover from disasters in the short-term without the reliance on their personal savings. Recognizing data limitations to properly scrutinize this second indirect impact mechanism, we opt to explore it by estimating the treatment effects on saving rates in developing versus developed countries (see Subsection 5.4). Given that developed countries offer a more enabling environment for access to insurance and credit (Kunreuther, 1996; Toya and Skidmore, 2007) and protections (Hallegatte, 2013; Noy, 2009; Raschky, 2008) than developing countries do, we expect a wider extent of flat post-storm saving rates or a lesser extent of declining post-storm saving rates in developed countries.

Last, natural disasters can change individual time preferences in disaster-stricken locations, which in turn alter consumption and saving patterns. However, this is more likely to be the case of earthquakes (Filipski *et al.*, 2019) and extreme disasters (Slemrod, 1990) than that of storms. Accordingly, we do not explore this channel.

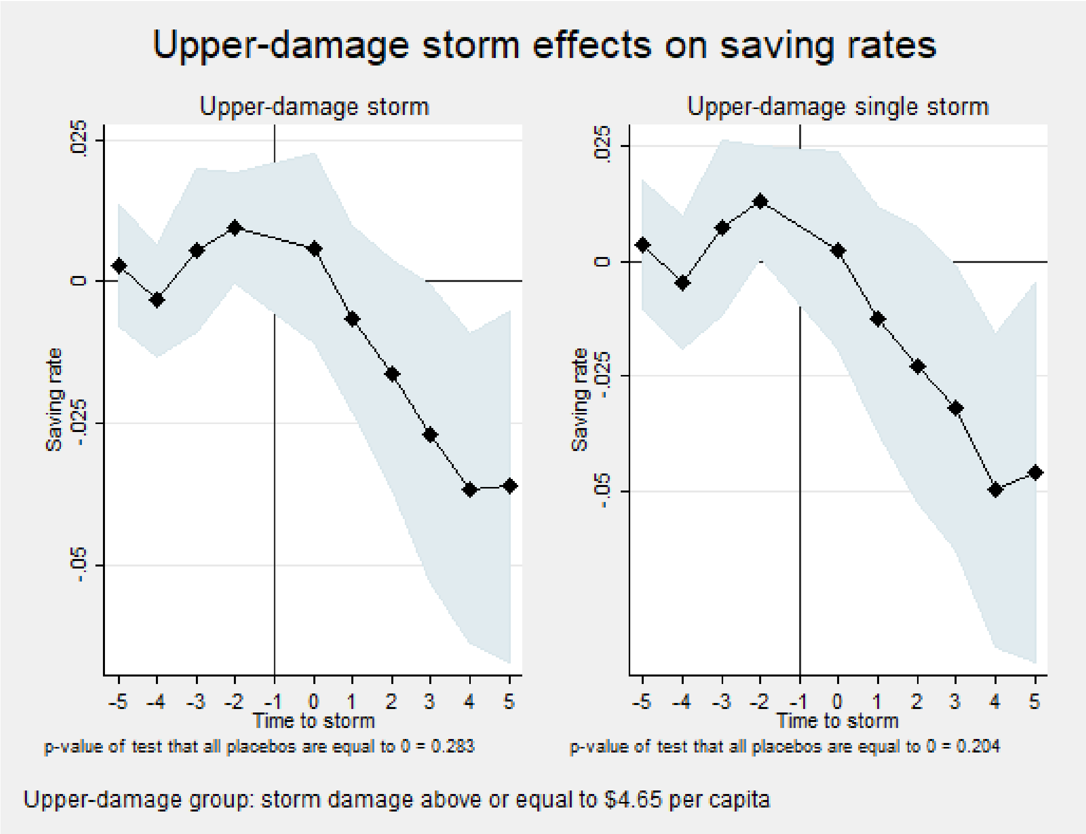
5.1 Perceived disaster-induced risk

a. Total damage per capita and ratio of damage to GDP

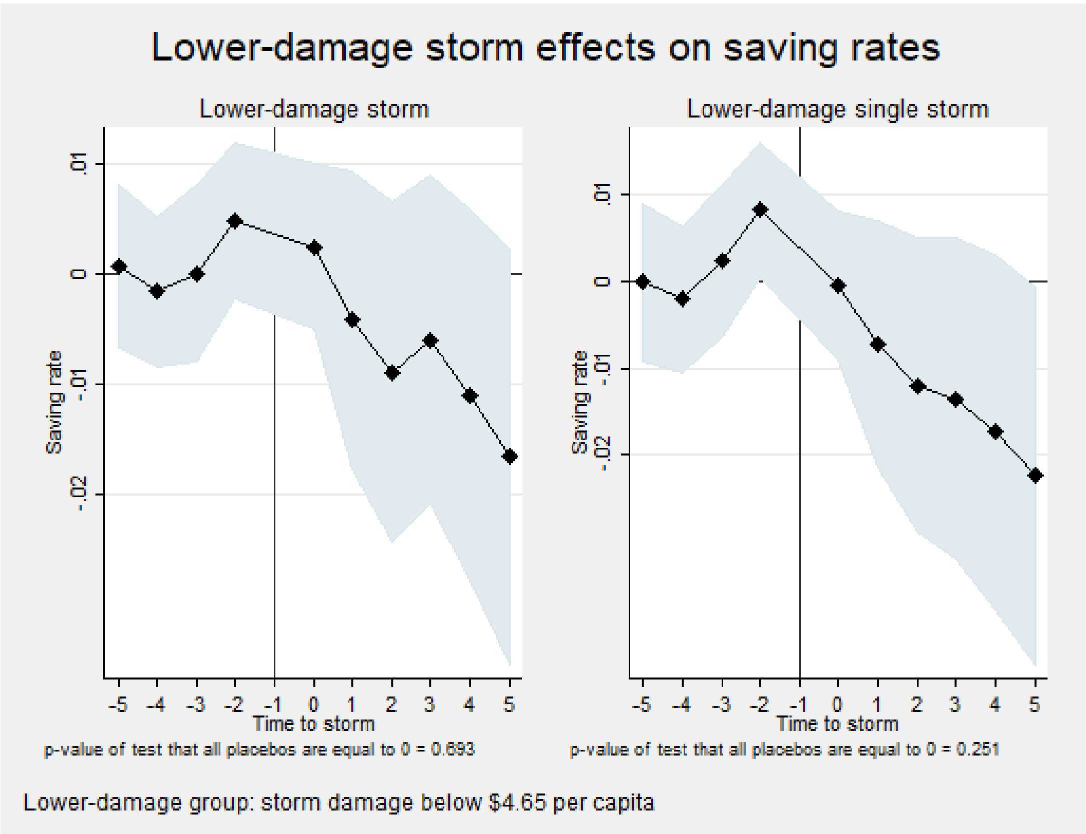
To further check whether storm effects follow the magnitude of total damage per capita, we create two groups of storms using the third quartile of total damage per capita for storms as cutoff[11], which is $4.65 per capita. Average damage is of 42 USD cents per capita in the lower-damage group versus $934 per capita in the upper-damage group. Figure 6a indicates that upper-damage storms (all inclusive) and single storms reduce saving rates after a two-year delay and for three years. The treatment effects for upper-damage storms (all inclusive) are -2.69, -3.65, and -3.60 percentage points in the third, fourth, and fifth post-storm year, respectively (Figure A.1, Panel A). Among upper-damage storms, a stronger effect on saving rates is observed for single storms: a decrease by 3.19, 4.98, and 4.59 percentage points in the third, fourth, and fifth post-storm year, respectively (Figure A.1, Panel B). In contrast, Figure 6b shows that saving rates are not affected by lower-damage storms (all inclusive), and the treatment effect for lower-damage single storms of -2.24 percentage points is observed only in the fifth post-storm year (Figure A.1, Panel D). Hence, the higher the damage per capita, the higher level of risk induced by the storm, and the higher use of the share of income that is saved on coping with the disaster.

Similarly to the storm damage per capita, we explore the ratio of storm damage to GDP as another proxy for storm intensity. Using the third quartile of the ratio of storm damage to GDP as the cutoff, we classify storms into relatively severe (total damage equal or above 0.04% of GDP) or relatively moderate (total damage less than 0.04% of GDP) storms. Mean proportion of damage to GDP is 4% for relatively severe storms versus 0.004% for relatively moderate storms. Figure 7a shows decreasing saving rates due to relatively severe storms (all inclusive) and single storms. Relatively severe storms lead to a fall in saving rates by 2.65, 3.34, and 2.97 percentage points in the third, fourth, and fifth post-storm year, respectively, (Figure A.2, Panel A). The effects of relatively severe single storms on saving rates are much stronger, with a decline by 2.87 and 4.15 percentage points in the third and fourth post-storm year, respectively, (Figure A.2, Panel B), as opposed to those of relatively moderate single storms by -2.34 percentage points in the fifth post-storm year (Figure A.2, Panel D). Refining storm intensity groups by using other cutoffs as a robustness check in Subsection 6.1 maintains the finding of a negative effect of severe storms —in terms of damage— on saving rates.

[11] Median storm damage per capita is very low (1.62 USD cents per capita), hence not used as the cutoff.

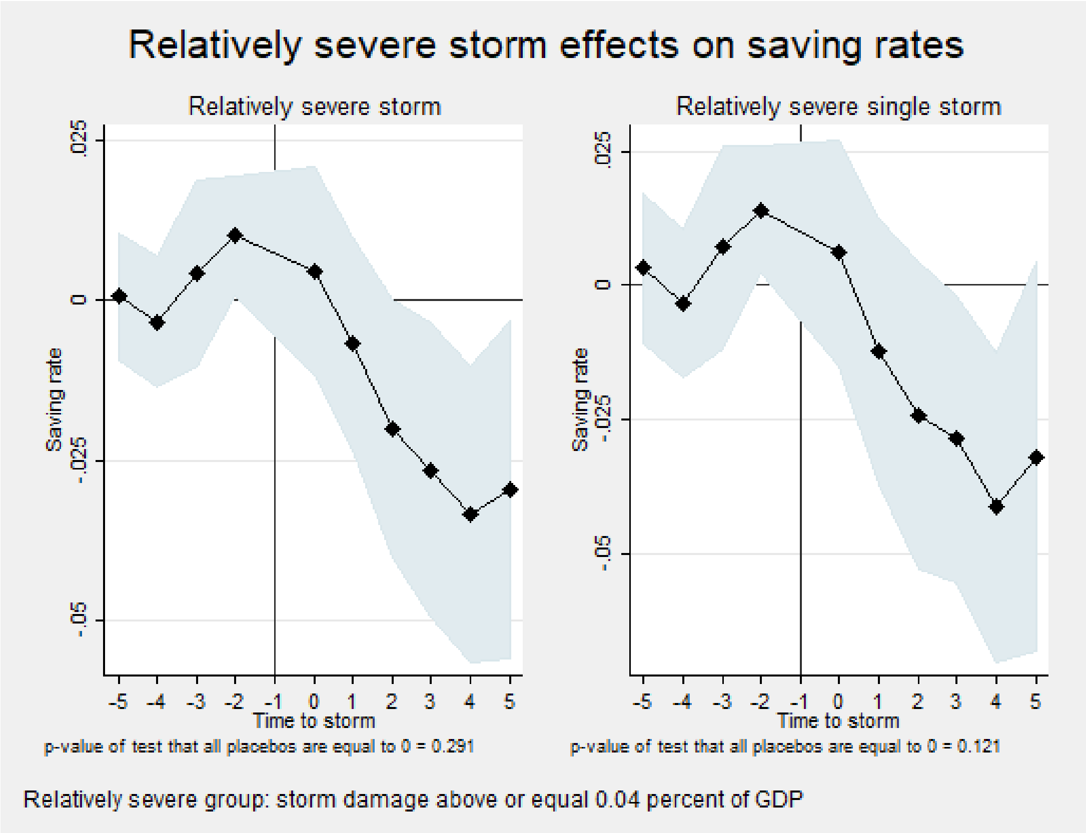


(a) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of upper-damage storms on saving rates

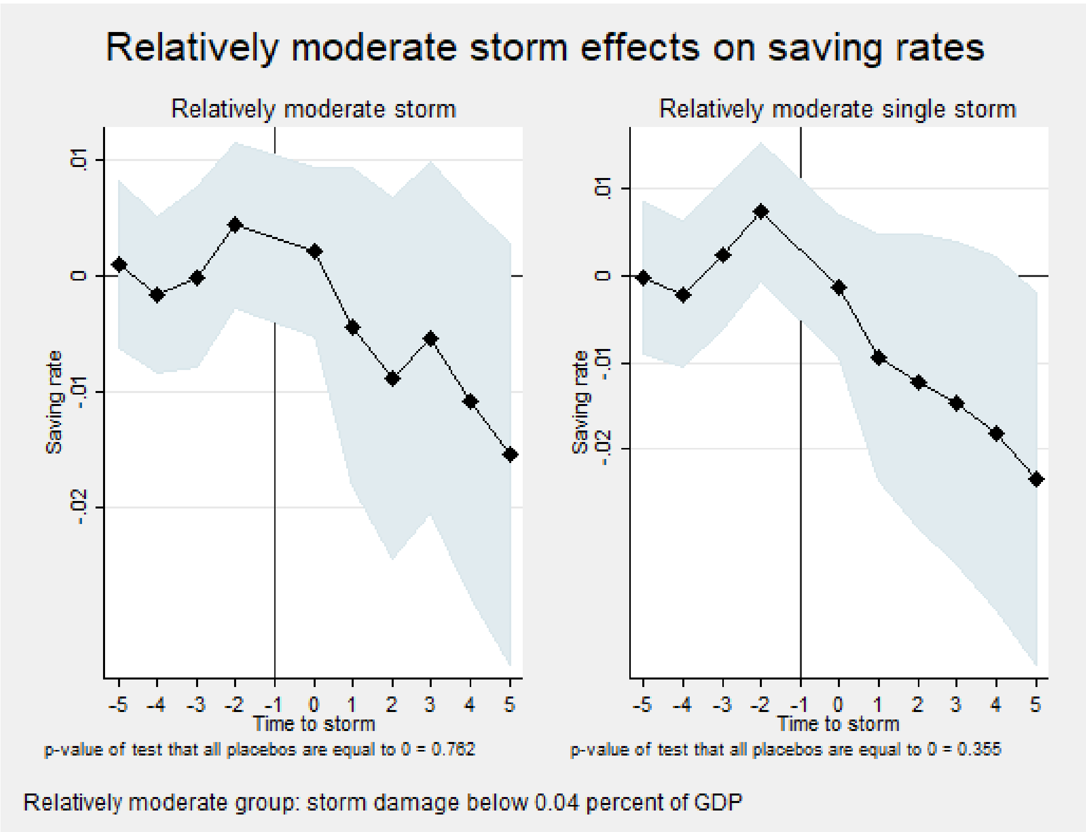


(b) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of lower-damage storms on saving rates

Figure 6: Average causal effect of storms on saving rates by per capita damage groups (third quartile as cutoff)



(a) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of relatively severe storms on saving rates



(b) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of relatively moderate storms on saving rates

Figure 7: Average causal effect of storms on saving rates by groups of ratio of damage to GDP (third quartile as cutoff)

b. Other disaster-induced risk channels

Asset loss

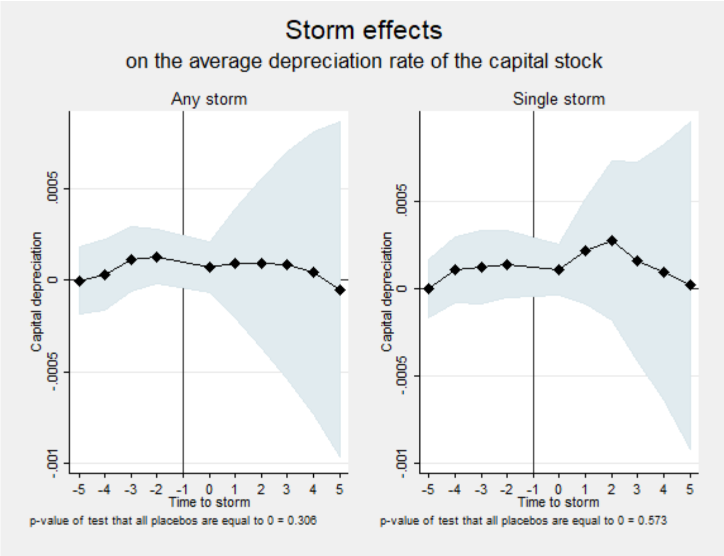
People may spend their savings on newer assets and asset improvements if they perceive a loss in the value of their assets due to the occurrence of storms. Notwithstanding, we find a negative effect of single storms on the average depreciation rate of the capital stock at levels of significance higher than 10% (Figure 8a). Thus, we rule out the risk of asset loss as a channel leading to a fall in saving rates.

Unemployment

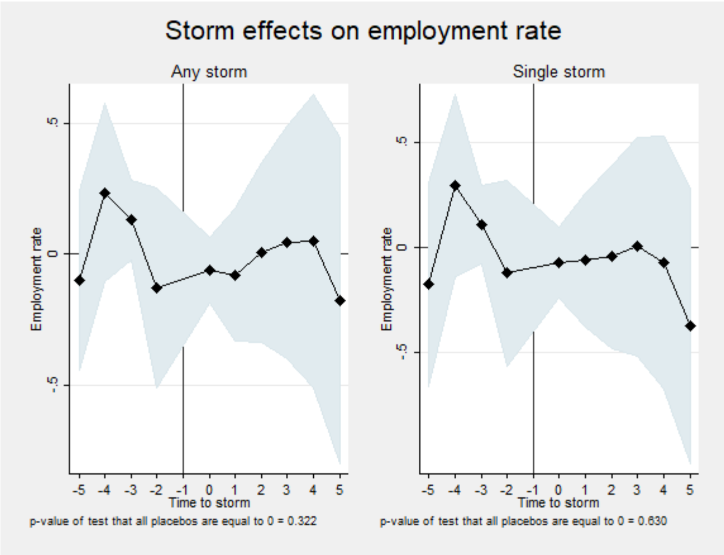
Another channel we examine is the possibility that the proportion of people employed in the country drops following the disaster, which signals the risk of losing jobs and may depress a higher share of income that is saved. Thus, a positive effect of storms on the employment rate places no or little threat for a fall in saving rates. As shown in the trends in Figure 8b, employment rates are not worsened by storms, a finding also reported in several impact studies of natural disasters (Cavallo and Noy, 2009; Noy, 2009). That the occurrence of natural disasters may even create more jobs is possible if the disaster damages request high investments in reconstruction, but this varies with the sector of employment and has further consequences on the productivity of firms (Kameda *et al.*, 2021). The lack of evidence on employment rate suggests to rule out an unemployment channel driving the observed reduction in post-storm saving rates.

Per capita labor income loss

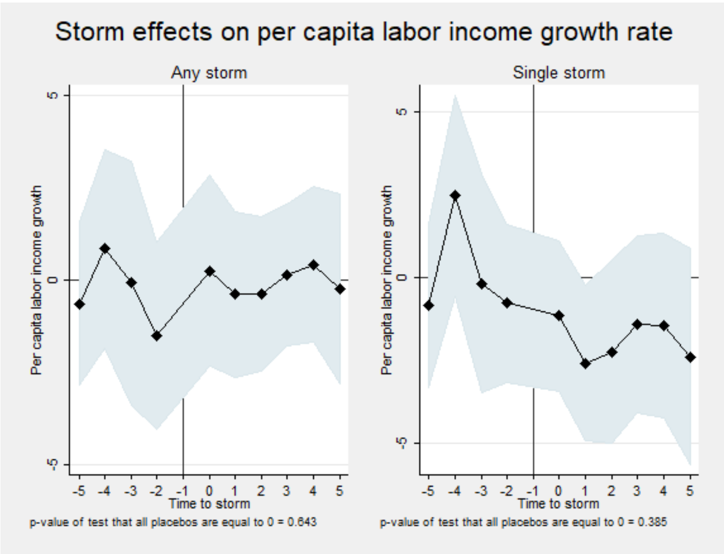
Similarly to employment rate, we expect the growth in per capita labor income—computed as the product of per capita GDP and the share of labor compensation in GDP—to be associated with the growth in saving rates. Figure 8c does not seem to indicate a statistically significant effect of storms (all inclusive) on per capita labor income growth rates; however, there is a drop in per capita labor income growth by 2.58 percentage points in the first year following single storms. This evidence suggests a higher level of risk of losing labor income in the aftermath of single storms, which could explain the later significant changes in saving rates after single storms (Figure 4). Hence, we fail to reject the income loss channel to declining post-storm saving rates.



(a) Average depreciation rate of the capital stock



(b) Employment rate



(c) Per capita labor income growth rate

Figure 8: De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on other disaster-induced risk outcomes

5.2 Inflation rate

We find no evidence of rising inflation rates instantaneously or in the years following storms (all inclusive) and single storms (Figure 9). We could have argued that the rise in inflation rates would increase the value of consumption, and thus, diminish savings levels. If, in addition, there is no GDP growth in the year of rising inflation rates, then diminishing saving rates will follow. However, we invalidate this inflation-led channel due to the missing evidence. Previous studies also find that inflation rates are not affected by natural disasters (Albala-Bertrand, 1993; Noy, 2009; Ramcharan, 2007).

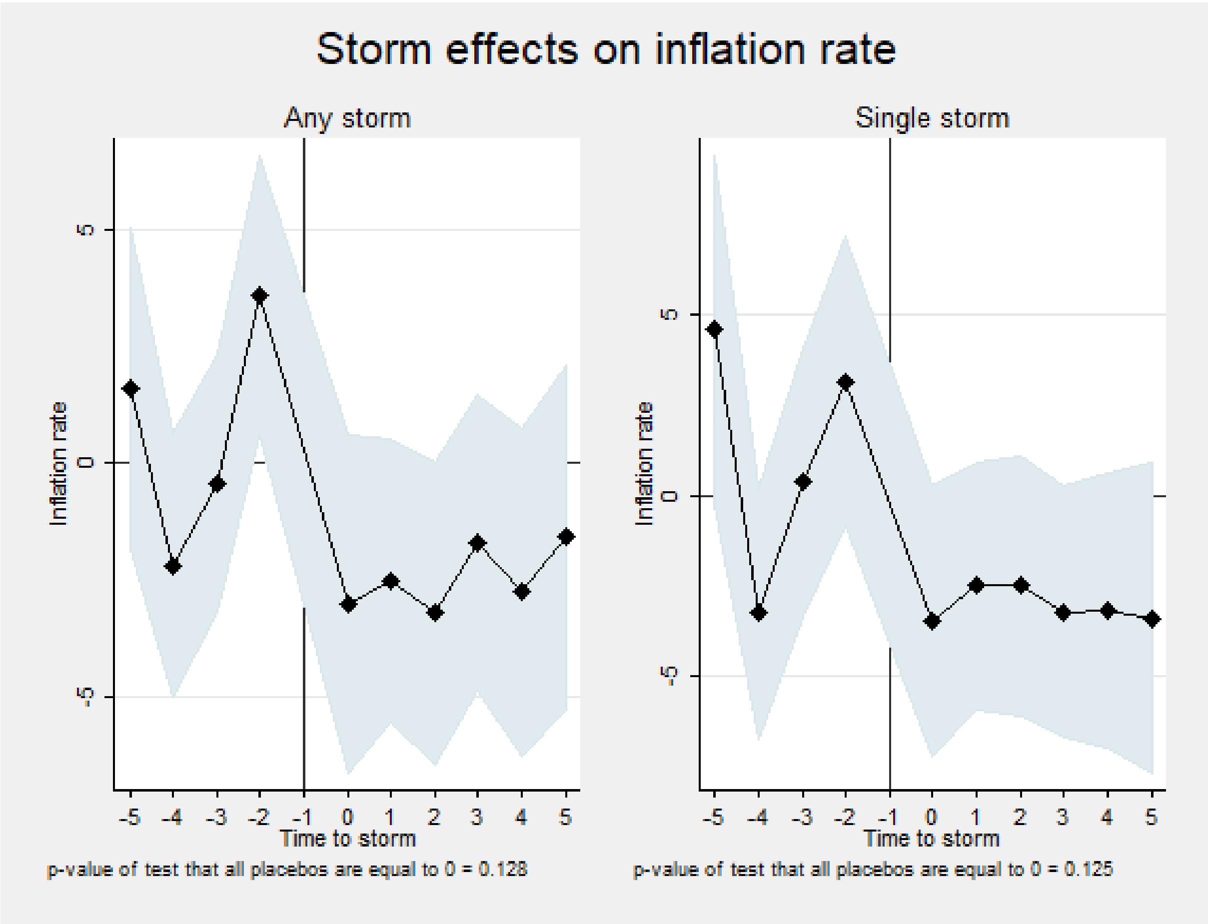


Figure 9: De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on inflation rates

5.3 Per capita GDP growth

The insignificant effects of single storms on per capita GDP growth rates (Figure 10) suggest the invalidation of the channel positing decreasing saving rates led by a rising per capita GDP growth. The economic growth effects of natural disasters have been extensively reviewed in the literature (Cavallo and Noy, 2009; Kellenberg and Mobarak, 2011; Noy and duPont IV, 2018). Skidmore and Toya (2002) indicate a positive effect of the number of disaster events on per-capita GDP growth, led by capital stock accumulation, human capital accumulation. Nevertheless, much of the evidence about the effects of specific natural disasters on economic growth have been about floods. Floods are reported to have a positive impact on GDP growth two years after the strike, transmitted via a positive impact on land productivity in the agricultural production cycle that follows the strike (Cunado and Ferreira, 2014; Fomby *et al.*, 2013). We examine further the treatment effects of storms on GDP growth rates in Subsection 6.2 before drawing a conclusion on the disaster aid channel.

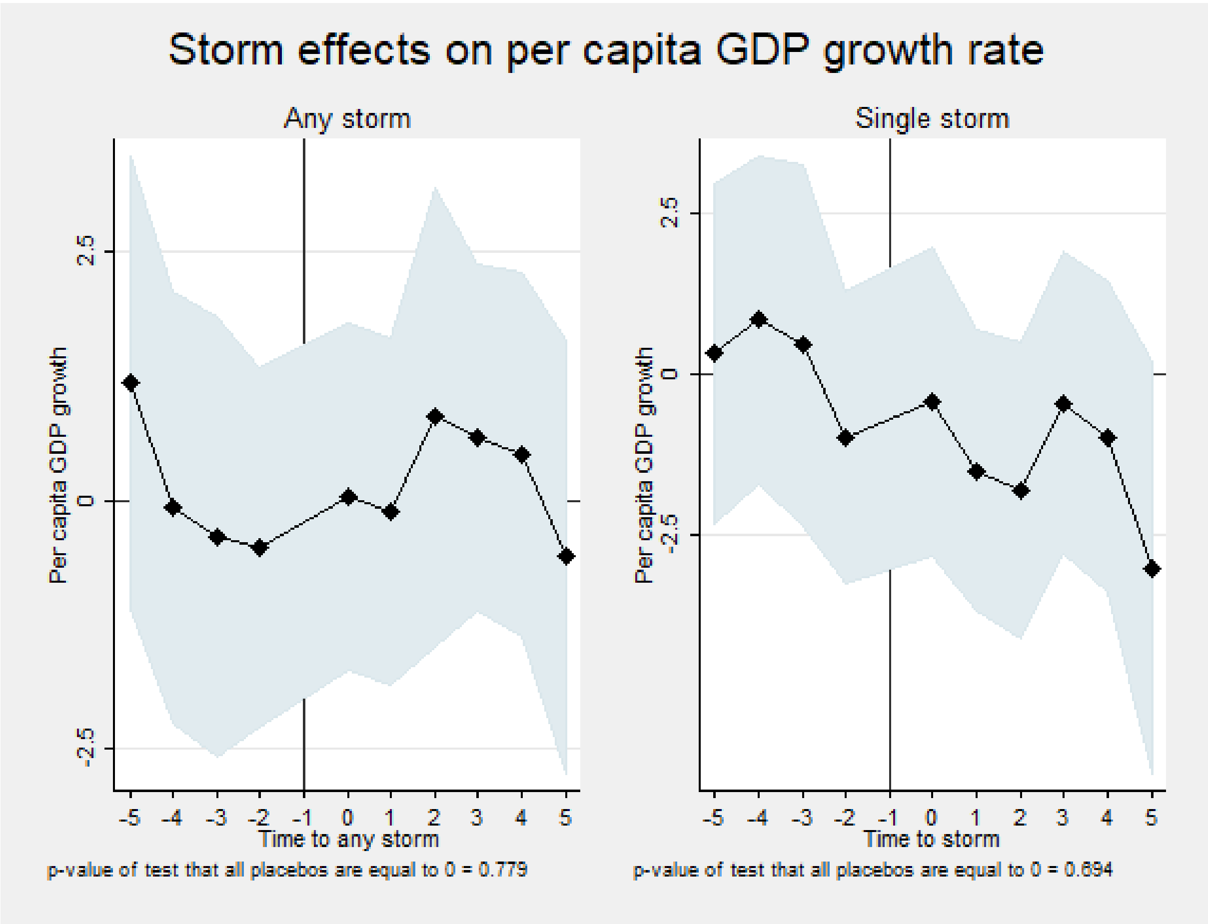


Figure 10: De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on per capita GDP growth rates

5.4 Economic development institutions

Dis-aggregating the effects of storms on saving rates by groups of economic development institutions sheds additional light on the treatment effects observed earlier in the aggregate results (Figure 4). While Figure 11 highlights flat post-storm saving rates in developed countries (statistically insignificant decreases), Figure 12 reveals a propensity to dis-save in developing countries. The strike of a single storm in developing countries leads to a drop in saving rates by 2.28 and 2.26 percentage points four and five years after the storm, respectively (Figure A.3, Panel B). Although placebo effects are jointly significant at the 10% level, the finding that saving rates are worsened by single storms only in developing countries (Figure 12) makes us not to rule out the potential mechanism of economic development institutions behind the impact of storm events on saving rates.

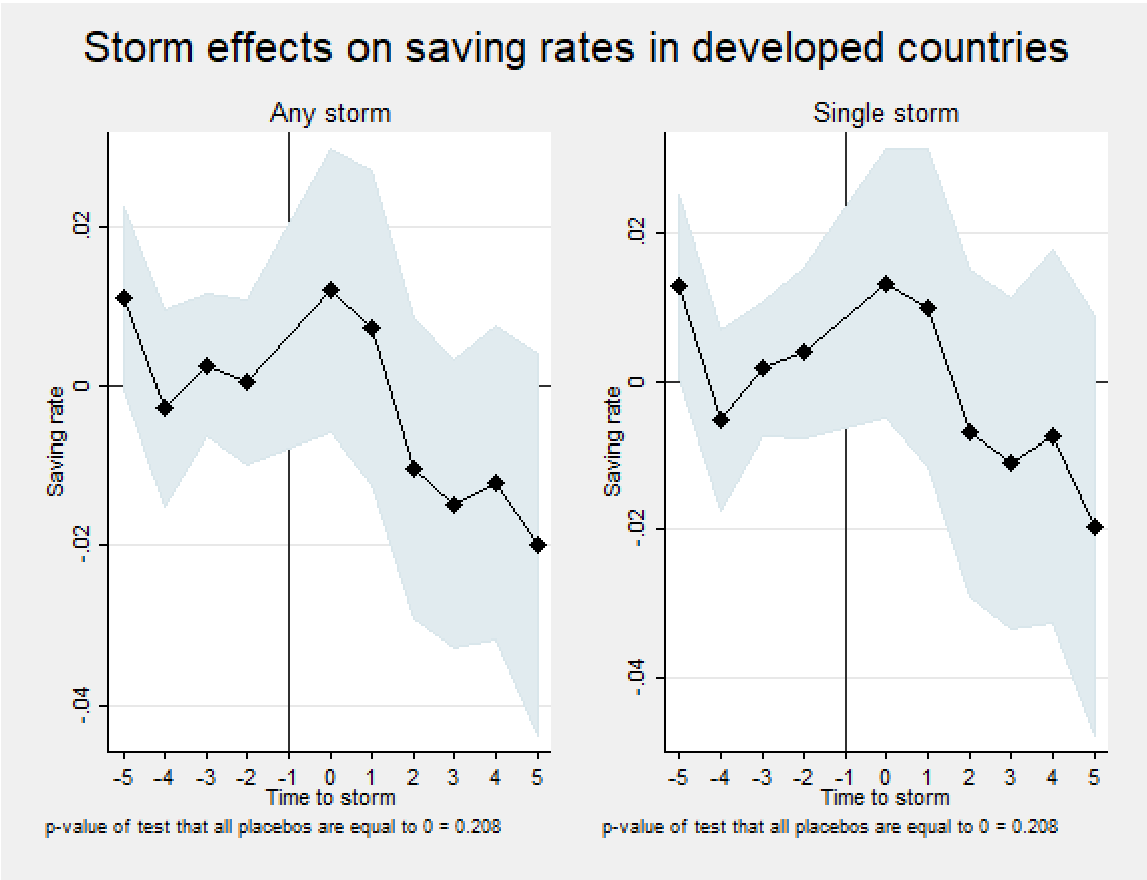


Figure 11: De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on saving rates in developed countries

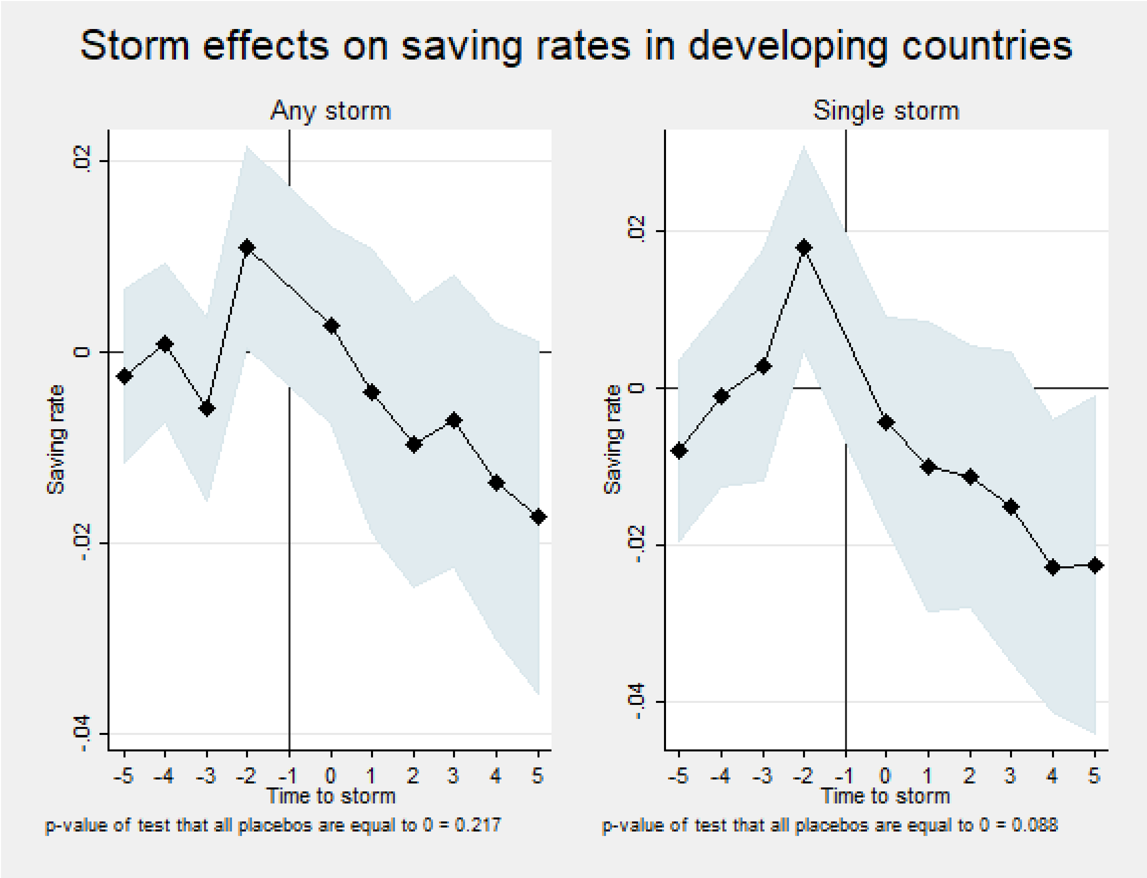


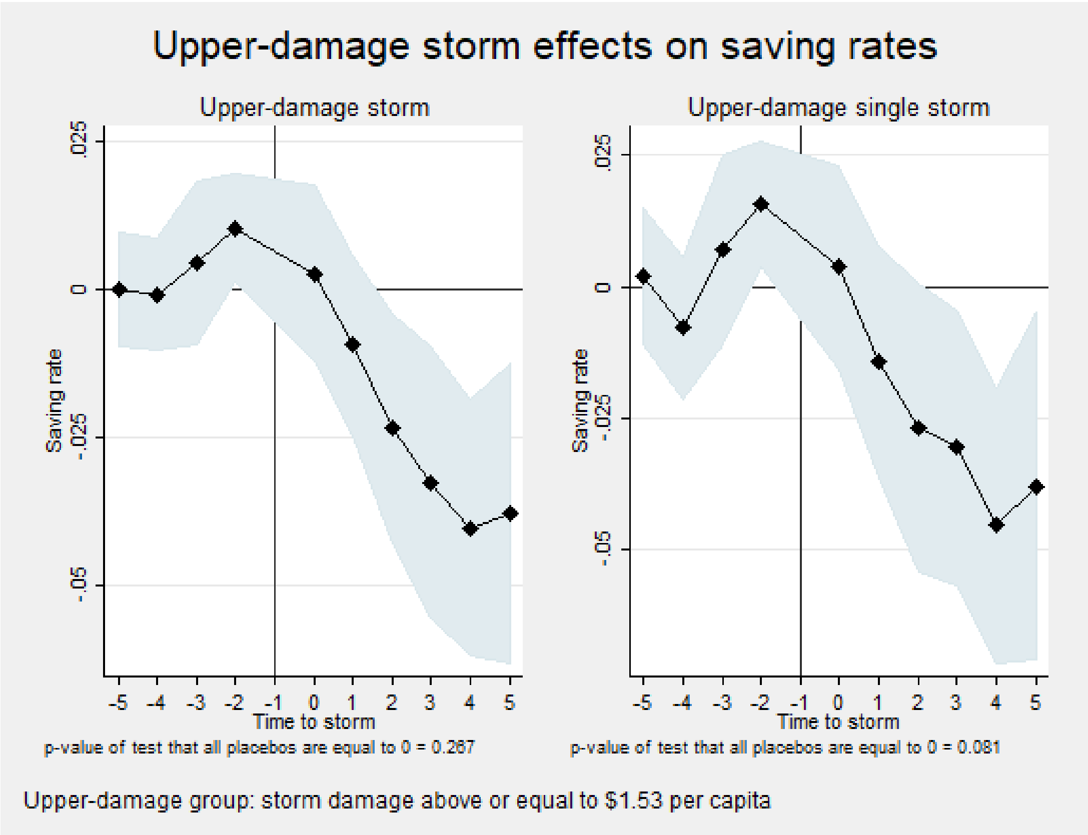
Figure 12: De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on saving rates in developing countries

6 Robustness checks

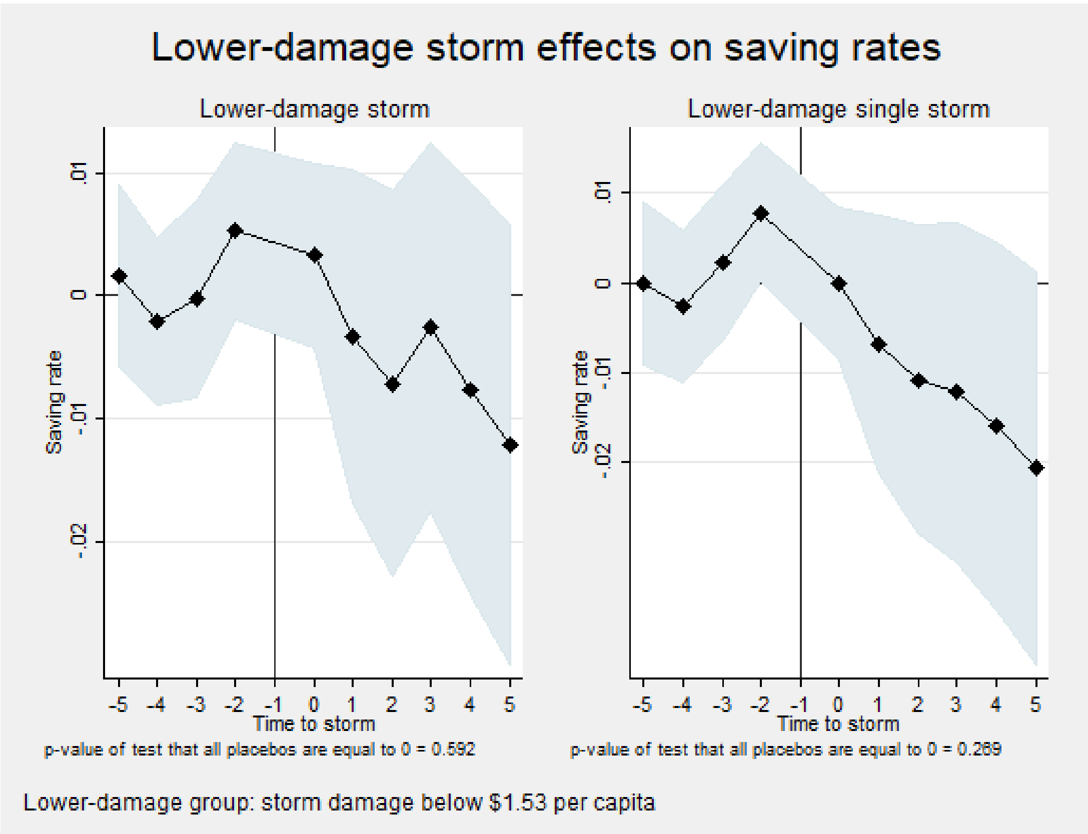
6.1 Storm effects by damage groups

To verify whether the heterogeneity in storm effects on saving rates by storm damage groups is robust to the cutoff used in creating the damage groups, we now use the second tercile of total damage per capita and ratio of damage to GDP instead of the third quartile in precedent results (Subsection 5.1, part a).

With this new cutoff, saving rates respond to upper-damage storms (all inclusive) one year earlier than previously. The treatment effects for upper-damage storms (all inclusive) are -2.36, -3.26, -4.03, and -3.79 percentage points in the second, third, fourth, and fifth post-storm year, respectively. Falling saving rates due to upper-damage single storms are observed for three years, with treatment effects of -3.07, -4.54, and -3.80 percentage points in the third, fourth, and fifth post-storm year, respectively, but parallel pre-trends hold at the 10% level (Figure 13a).

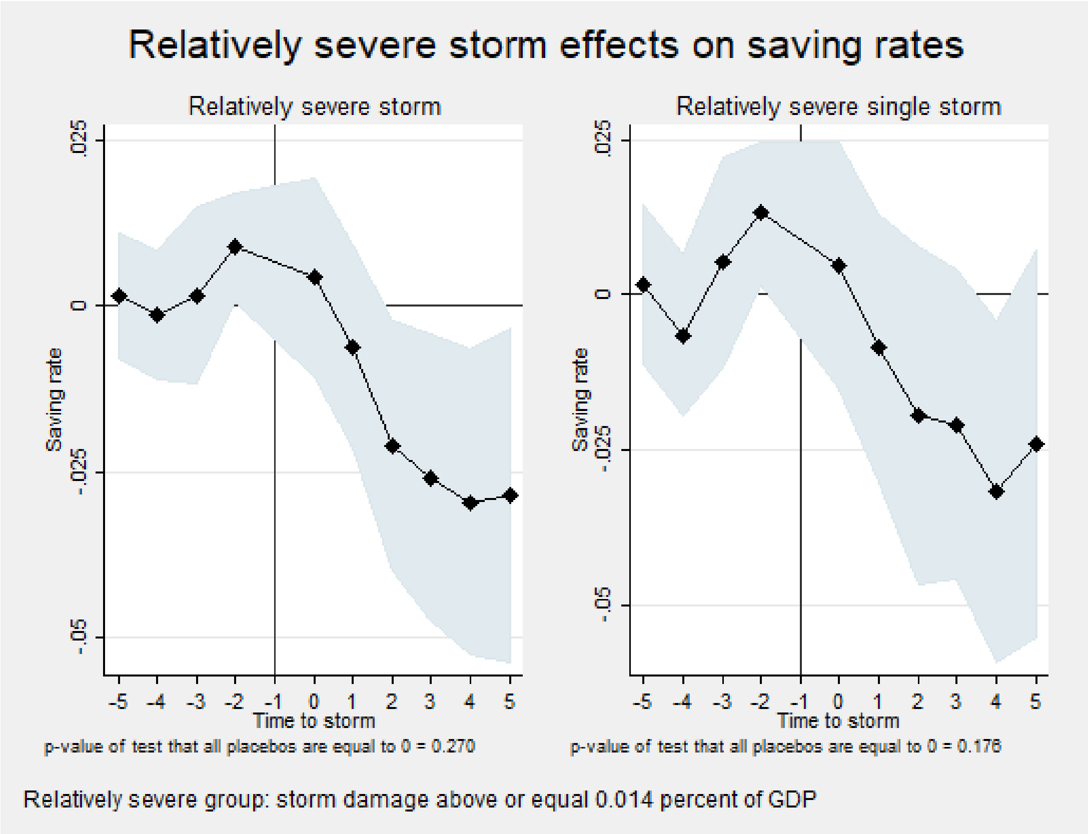


(a) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of upper-damage storms on saving rates

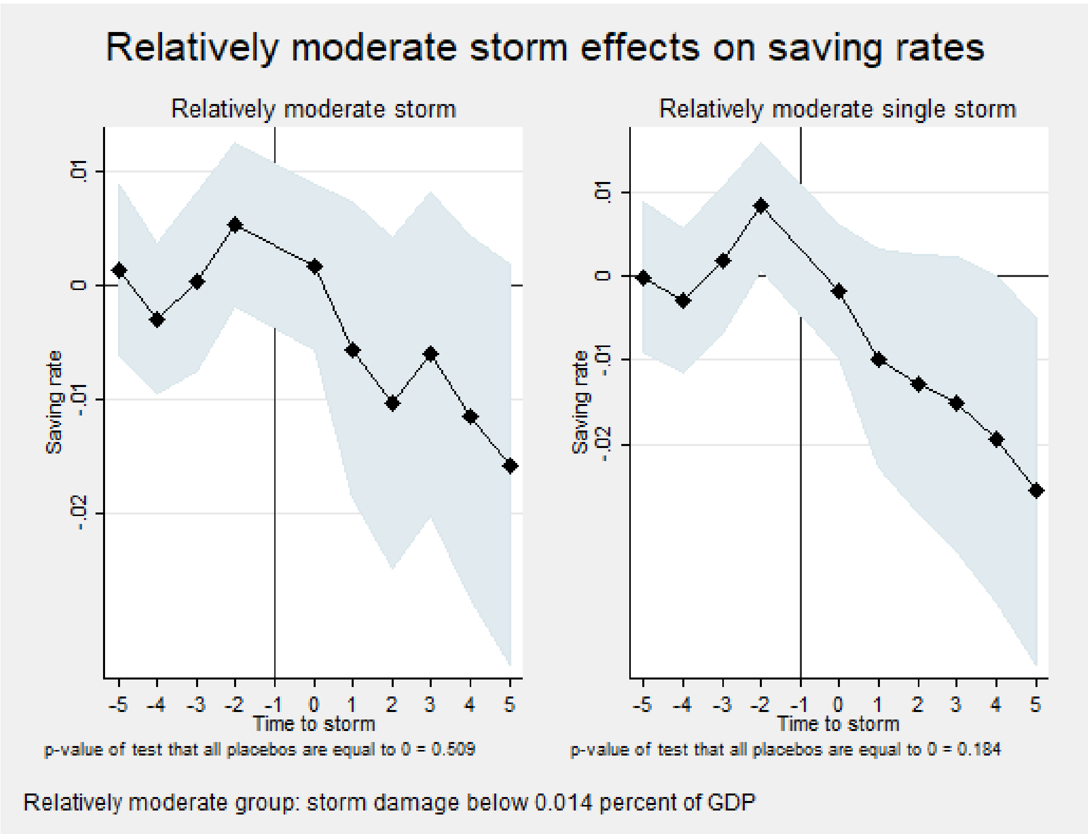


(b) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of lower-damage storms on saving rates

Figure 13: Average causal effect of storms on saving rates by per capita damage groups (second tercile as cutoff)



(a) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of relatively severe storms on saving rates



(b) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of relatively moderate storms on saving rates

Figure 14: Average causal effect of storms on saving rates by groups of ratio of damage to GDP (second tercile as cutoff)

Using the second tercile of the ratio of damage to GDP as a cutoff to distinguish relatively severe storms from relatively moderate ones, we still find that the storm effects on saving rates decrease with the ratio of damage to GDP. The treatment effects for relatively severe storms (all inclusive) are -2.11, -2.58, -2.95, and -2.84 percentage points in the second, third, fourth, and fifth post-storm year, respectively (Figure 14a). While saving rates for relatively severe single storms reduce by 3.17 percentage points in the fourth post-storm year as compared to the year preceding the storms (Figure 14a), the treatment effects of relatively moderate single storms on saving rates are also negative but have a lower magnitude of 1.94 and 2.56 percentage points in the fourth and fifth post-storm year, respectively (Figure 14b).

6.2 Storm effects on GDP growth rate

If the occurrence of storms leads to changes in population dynamics or to international migration, the treatment effects on per capita GDP growth rates would be different from those on GDP growth rates. To further check this point and conclude on the potential disaster aid channel for the decline in saving rates, the dynamic treatment effects on GDP growth are presented in Figure 15. The results look similar to those in Figure 10, although there is evidence of a slower reduction in GDP growth by 3.34 percentage points in the fifth year following single storms (Figure 15). Because the evidence is weak—occurring after a four-year delay—, it lends little support to the non-rejection of the disaster aid channel. Robust results in Subsection 6.3 lead to finally rule out the disaster aid channel.

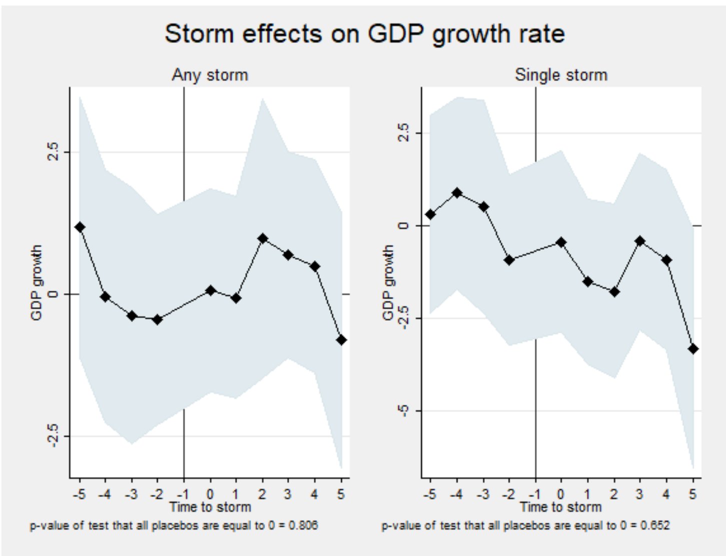


Figure 15: De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on GDP growth rates

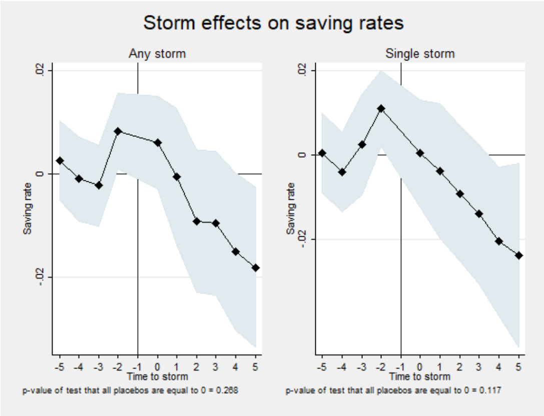
6.3 Storm effects on saving rates and mechanisms in a more balanced country-year panel sample

Because the study panel sample covers 176 countries with different lengths of years (between 14 and 69), we assess the robustness of the findings using a more balanced panel sample that includes 151 countries with non-missing data for at least 46 years (two thirds of the years covered in the estimations above).[12]

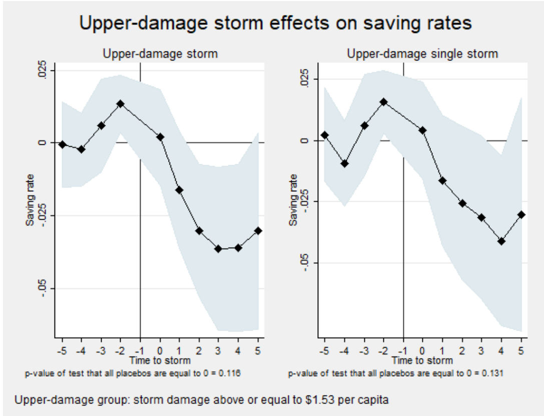
Focusing on storm events (all inclusive), the robust findings using a more balanced sample indicate these events are not saving-free and result from facing a higher storm damage per capita. We find that GDP growth rates are not influenced by storms (Figure 16d), thus suggesting that any changes in saving rates derive from changes in savings levels. The evidence about the reduction of saving rates following any storm, irrespective of the storm intensity, is weak: a decrease by 1.81 percentage points occurring after a four-year delay (Figure 16a). The treatment effects on saving rates for storms with damage per capita above or equal to $1.53, however, is strong: a decrease by 3.02, 3.63, 3.60 percentage points in the second, third, and fourth post-storm year, respectively (Figure 16b). For storms (all inclusive), there is no evidence suggesting as impact mechanisms the risk of income loss (Figure 16c) or presence of weak economic development institutions (Figure 16e).

Regarding single storm events, robust findings using a more balanced sample direct towards the risk of income loss, high storm damage per capita, and the presence of weak economic development institutions, as the mechanisms driving falling saving rates. Strikingly, in the aftermath of storms as the sole natural disaster events in a year, per capita labor income growth rates fall by 2.75 and 2.68 percentage points in the first and second post-storm year, respectively (Figure 16c). It follows a drop in saving rates by 2.05 and 2.38 percentage points in the fourth and fifth post-storm year, respectively (Figure 16a)—a drop pertaining to savings levels (Figure 16d). These treatment effects on saving rates double down for upper-damage single storms, with a decrease by 4.09 percentage points in the fourth post-storm year (Figure 16b). Single storms also lead to falling saving rates by 2.45 percentage points in the fourth post-storm year in developing countries (Figure 16e), while there is no such evidence in developed countries (Figure 16f).

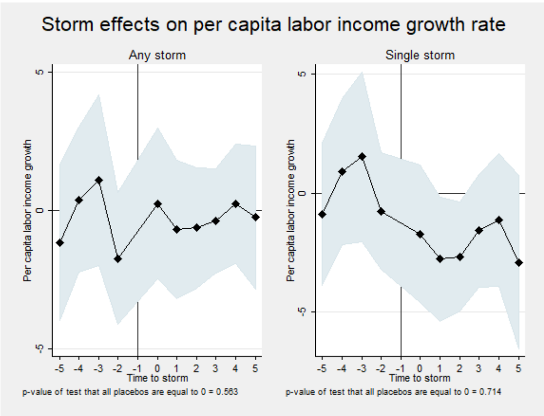
[12] We do not use the fully balanced panel sample of 55 countries with non-missing data for years 1951 through 2019 due to low statistical power issues in dynamic estimations with switching treatment. Because De Chaisemartin and d'Haultfoeuille (2020a)'s estimator compares switchers to non-switchers, estimations using a large sample size are preferred. Treatment effects and placebo coefficients estimated on very few switchers are less reliable than on many more switchers.



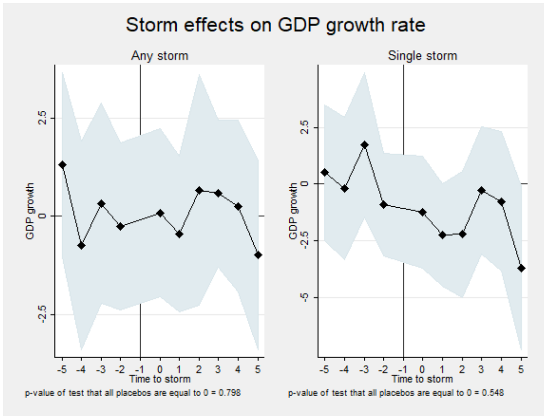
(a) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on saving rates



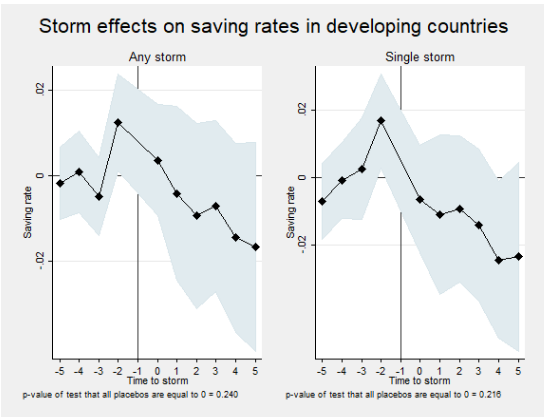
(b) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effects of upper-damage storms on saving rates



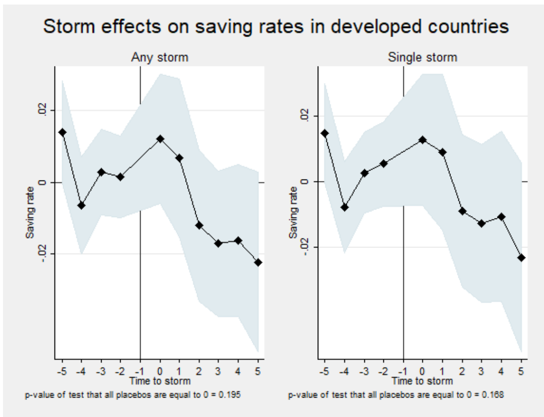
(c) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on per capita labor income growth rate



(d) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on GDP growth rates



(e) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on saving rates in developing countries



(f) De Chaisemartin and d'Haultfoeuille (2020a)'s estimator for the average causal effect of storms on saving rates in developed countries

Figure 16: Summary effects of storms using a more balanced panel sample

7 Conclusion

Despite a growing evidence about the macroeconomic impact of natural disasters and a substantial body of theoretical foundations on saving behavior, much of the empirical literature has not provided rigorous, global evidence about the dynamic effect of storms on saving behavior. Using a panel data event study design, this paper has investigated the propensity to save prior to and in the aftermath of storms. Aggregate results across all countries based on a ten-year event time window indicate flat saving rates for four years prior to the occurrence of single storms—those occurring as the sole natural disasters in a given country-year—and declining savings rates observed four years following the storm. In the fourth and fifth post-storm years, saving rates are reduced by more than a twentieth of the average saving rate in non-storm years.

The unchanged saving rates in anticipation of storms highlights the need to raise awareness about the importance of countries' disaster preparedness strategies. Flat pre-disaster saving rates, but initially relatively high, would prepare for swift disaster recovery when a disaster strikes. In contrast, flat pre-disaster saving rates, but initially relatively low, would make the recovery process last longer after countries get hit by a disaster. Regardless of the initial magnitude of the saving rates, flat pre-disaster saving rates in countries highly-exposed to severe natural disasters would be preferred to falling pre-disaster saving rates, unless countries invest in better protections against natural disasters.

The paper discusses potential mechanisms behind post-storm saving behavior. It rules out the post-storm effects of disaster aid, inflation rates, depreciation of capital stock, and employment rates. However, it finds no robust evidence to reject the perceived disaster risk induced by the post-storm economic damages and the loss of labor income per capita, in addition to economic development institutions as the prevailing mechanisms. On one hand, with regard to the disaster-induced risk perceptions, single storms lead to a decline in per capita labor income growth by almost 3 percentage points in the first two post-storm years. From the third post-storm year, single storms that generate at least \$4.65 damage per capita or damage worth 0.04% of GDP lead to a decrease in saving rates by about a tenth of the average saving rate in non-storm years. On the other hand, dis-aggregate results by economic development status reveal divergences in post-storm saving behavior. Developed countries have institutions that help build up resilience against storms without significantly deteriorating private saving rates in response to storms. In contrast, four years after the storm in developing countries, there is a significantly higher propensity to dis-save by about a fifteenth of the average saving rate in non-storm years. Thus, fostering reconstruction jobs especially in countries prone to severe storm damages, as well as establishing functioning insurance markets and easing access to consumer credit in developing countries, are imperative to avoid a depletion of private savings following storm disasters.

In this paper, saving rates are observed at the country-year level, and the estimations have exploited variation in the timing and composition of natural disasters in the country-year when these disasters occur. It is possible that some storms have short-term effects in very few months around the event, which are canceled out or reinforced by exogenous shocks other than natural disasters occurring in the same country-year as the storm but in different months. It is also possible that other storms have no effects but occur in the same country-years as exogenous shocks other than natural disasters which bring the observed effect. Due to these limitations, the paper cannot conclude beyond the scope of the cumulative effect of exogenous shocks arising in years of storms on private saving rates.

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A Appendix

List of Tables

Table A.1: Differences in country's demographic, economic, and disaster characteristics across storm occurrence groups

Variable	(1) Storm occurred N/[Clusters]	(1) Storm occurred Mean/SD	(2) Storm did not occur N/[Clusters]	(2) Storm did not occur Mean/SD	T-test Difference (1)-(2)	Normalized difference (1)-(2)
Population (in millions)	1370	68.435	8347	15.950	52.486	0.559
	[145]	(1350.565)	[174]	(262.692)		
Real private savings at current PPPs (in billions 2011 USD)	1370	256.222	8347	47.152	209.070*	0.583
	[145]	(4104.043)	[174]	(632.279)		
Expenditure-side real GDP at current PPPs (in billions 2011 USD)	1370	647.057	8347	132.700	514.357**	0.667
	[145]	(8685.345)	[174]	(1748.160)		
Per-capita expenditure-side real GDP at current PPPs (2011 USD p.c.)	1370	15486.119	8347	12763.938	2722.181	0.139
	[145]	(62150.873)	[174]	(1.20e+05)		
Private savings rate	1370	0.341	8347	0.351	-0.010	-0.086
	[145]	(0.309)	[174]	(0.663)		
Total deaths	1370	598.367	8347	0.000	598.367	0.162
	[145]	(15070.591)	[174]	(0.000)		
Total affected (in millions)	1370	0.526	8347	0.000	0.526	0.343
	[145]	(13.134)	[174]	(0.000)		
Total damage (in millions USD)	1370	240.775	8347	0.000	240.775***	0.666
	[145]	(2854.447)	[174]	(0.000)		
Total damage (in thousands USD) per capita	1370	0.251	8347	0.000	0.251**	0.188
	[145]	(3.813)	[174]	(0.000)		
Ratio of total damage to GDP	1370	0.011	8347	0.000	0.011***	0.274
	[145]	(0.133)	[174]	(0.000)		
Intense storm	1370	0.318	8347	0.000	0.318***	1.537
	[145]	(1.340)	[174]	(0.000)		
Severe storm	1370	0.070	8347	0.000	0.070***	0.708
	[145]	(0.433)	[174]	(0.000)		
Probability of being hit by a storm disaster	1370	0.326	8347	0.108	0.217***	1.348
	[145]	(1.314)	[174]	(0.763)		
Flood occurred (1=yes, 0=no)	1370	0.447	8347	0.205	0.241***	0.565
	[145]	(1.360)	[174]	(1.231)		
Epidemic occurred (1=yes, 0=no)	1370	0.132	8347	0.086	0.046***	0.159
	[145]	(0.576)	[174]	(0.779)		
Earthquake occurred (1=yes, 0=no)	1370	0.109	8347	0.051	0.058**	0.245
	[145]	(1.075)	[174]	(0.835)		
Drought occurred (1=yes, 0=no)	1370	0.100	8347	0.052	0.048***	0.203
	[145]	(0.646)	[174]	(0.468)		
Other disaster type occurred (1=yes, 0=no)	1370	0.234	8347	0.091	0.143***	0.455
	[145]	(1.199)	[174]	(0.830)		

Notes: Storm intensity and severity are dummy variables defined following Fomby *et al.* (2013) and based on the count of fatalities and the people affected by the disaster. For an intense storm, total deaths plus 30% of people affected is higher than 0.01% of the country's population. For a severe storm, total deaths plus 30% of people affected is higher than 1% of the country's population. The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at variable countrycode. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

List of Figures

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0059719	.0085109	-.0107095	.0226533	3739	74
Effect_1	-.0065635	.0083939	-.0230155	.0098885	2988	57
Effect_2	-.0163528	.0103019	-.0365445	.0038389	2712	52
Effect_3	-.0267845	.0133589	-.0529679	-.0006011	2067	38
Effect_4	-.0364782	.0138482	-.0636207	-.0093357	1866	34
Effect_5	-.0360327	.0157529	-.0669084	-.0051571	1460	29
Placebo_1	.0094586	.0049683	-.0002794	.0191965	3378	67
Placebo_2	.0055494	.0072932	-.0087453	.019844	3057	65
Placebo_3	-.0033254	.0050054	-.0131359	.0064851	2702	58
Placebo_4	.0027338	.0054313	-.0079115	.0133791	2585	56

p-value of test that all placebos are equal to 0 = 0.28338824

Panel A. Treatment: Upper-damage storm (damage per capita ≥ $4.65). Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0022937	.0109291	-.0191273	.0237146	3114	53
Effect_1	-.012589	.0125148	-.037118	.0119401	2190	37
Effect_2	-.0226785	.0151724	-.0524165	.0070594	1881	32
Effect_3	-.0319167	.0157173	-.0627226	-.0011107	1632	28
Effect_4	-.0498269	.0174292	-.0839882	-.0156657	1377	23
Effect_5	-.0458592	.0210672	-.0871509	-.0045675	1070	20
Placebo_1	.0128384	.0062347	.0006183	.0250585	2769	47
Placebo_2	.007349	.0097215	-.0117051	.0264032	2470	45
Placebo_3	-.0048131	.0073802	-.0192782	.009652	2027	38
Placebo_4	.0035917	.0071678	-.0104572	.0176407	1940	36

p-value of test that all placebos are equal to 0 = 0.20416504

Panel B. Treatment: Upper-damage single storm (damage per capita ≥ $4.65). Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0024971	.003821	-.004992	.0099861	3774	131
Effect_1	-.00418	.0069388	-.01778	.00942	3635	130
Effect_2	-.0088939	.0078671	-.0243135	.0065257	3404	125
Effect_3	-.0059887	.0076305	-.0209445	.0089671	3244	120
Effect_4	-.0109854	.0086181	-.0278768	.005906	3115	119
Effect_5	-.0166689	.0096591	-.0356007	.0022629	2995	116
Placebo_1	.0048831	.0036495	-.0022699	.0120362	3521	124
Placebo_2	.0000886	.0041082	-.0079634	.0081407	3425	122
Placebo_3	-.0015936	.0034691	-.0083931	.0052059	3249	117
Placebo_4	.0007425	.0038184	-.0067416	.0082266	3092	113

p-value of test that all placebos are equal to 0 = 0.69269976

Panel C. Treatment: Lower-damage storm (damage per capita < $4.65). Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	-.0005977	.0043312	-.0090869	.0078915	4199	103
Effect_1	-.0073333	.0072008	-.0214469	.0067802	3929	98
Effect_2	-.0119627	.0085876	-.0287944	.004869	3479	89
Effect_3	-.0135303	.009405	-.0319641	.0049035	3192	81
Effect_4	-.0173519	.0103657	-.0376686	.0029648	2960	79
Effect_5	-.022356	.0110541	-.0440221	-.00069	2798	72
Placebo_1	.0080994	.0039724	.0003136	.0158852	3789	93
Placebo_2	.0022887	.0044127	-.0063602	.0109376	3477	88
Placebo_3	-.0021548	.0042916	-.0105664	.0062568	3301	86
Placebo_4	-.0001343	.0046245	-.0091984	.0089298	2932	78

p-value of test that all placebos are equal to 0 = 0.25114246

Panel D. Treatment: Lower-damage single storm (damage per capita < $4.65). Outcome: Saving rate

Figure A.1: De Chaisemartin and d'Haultfoeuille (2020a)'s robust dynamic treatment effects of storms on saving rates by per capita damage groups

Notes: This is the output of the Stata command *did_multiplegt* for De Chaisemartin and d'Haultfoeuille (2020a). Each row Effect_# is the result of the heterogeneity-robust DID estimating the effect of having switched in treatment for the first time # years ago. Estimate column = estimated effect of the treatment at the time period when first-time switchers switch. SE column = Standard Error computed using 100 bootstrap replications. LB CI and UB CI columns = Lower Bound and Upper Bound of the 95% confidence interval. N column = total number of observations (first-time and not-yet switchers) used in the estimation of the treatment effect. Switchers column = number of first-time switchers the estimate applies to.

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0044907	.0083047	-.0117865	.0207679	3735	74
Effect_1	-.0067703	.008465	-.0233617	.0098212	2936	57
Effect_2	-.0199973	.0102577	-.0401023	.0001077	2758	53
Effect_3	-.0265495	.0116835	-.0494492	-.0036499	2211	43
Effect_4	-.0334489	.0117408	-.0564608	-.0104369	2018	40
Effect_5	-.0296555	.0134607	-.0560385	-.0032724	1696	35
Placebo_1	.0100107	.0047504	.0006998	.0193215	3356	66
Placebo_2	.00403	.0073989	-.010472	.0185319	3040	63
Placebo_3	-.0034277	.0051694	-.0135598	.0067043	2663	56
Placebo_4	.0004688	.0051163	-.0095592	.0104968	2514	54

p-value of test that all placebos are equal to 0 = 0.29119548

Panel A. Treatment: Relatively severe storm (% damage to GDP $\geq$ 0.04). Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0059214	.010815	-.0152761	.0271188	3261	54
Effect_1	-.012315	.0125625	-.0369376	.0123075	2360	37
Effect_2	-.0244428	.0146509	-.0531586	.004273	2151	35
Effect_3	-.0286523	.0136618	-.0554293	-.0018752	1906	32
Effect_4	-.0414951	.0146901	-.0702877	-.0127024	1653	26
Effect_5	-.0320109	.0184622	-.0681967	.004175	1345	22
Placebo_1	.013969	.0060813	.0020497	.0258883	2790	48
Placebo_2	.006975	.0095922	-.0118258	.0257758	2495	45
Placebo_3	-.0034965	.007089	-.0173909	.0103979	2133	39
Placebo_4	.0031265	.0071749	-.0109362	.0171893	2013	37

p-value of test that all placebos are equal to 0 = 0.12138228

Panel B. Treatment: Relatively severe single storm (% damage to GDP $\geq$ 0.04). Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0020022	.0036799	-.0052104	.0092148	3977	132
Effect_1	-.0044672	.0069892	-.0181661	.0092318	3756	127
Effect_2	-.0088658	.007952	-.0244517	.0067202	3518	121
Effect_3	-.0054068	.0077239	-.0205455	.009732	3351	116
Effect_4	-.0109138	.0086137	-.0277967	.005969	3218	115
Effect_5	-.0154127	.0092849	-.0336111	.0027857	3093	112
Placebo_1	.0043372	.0036489	-.0028146	.011489	3720	125
Placebo_2	-.0001741	.0039889	-.0079923	.0076441	3621	123
Placebo_3	-.001699	.0033941	-.0083514	.0049534	3442	119
Placebo_4	.0009345	.0036759	-.0062703	.0081393	3282	115

p-value of test that all placebos are equal to 0 = 0.76224983

Panel C. Treatment: Relatively moderate storm (% damage to GDP < 0.04). Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	-.0012852	.0041713	-.0094609	.0068905	4177	102
Effect_1	-.0094378	.0072758	-.0236983	.0048227	3840	93
Effect_2	-.0122062	.0086845	-.0292278	.0048154	3378	84
Effect_3	-.0146636	.00948	-.0332443	.0039171	3169	79
Effect_4	-.0181378	.0103873	-.0384969	.0022213	2938	77
Effect_5	-.0234272	.0109403	-.0448702	-.0019842	2775	70
Placebo_1	.0073453	.0040964	-.0006836	.0153742	3813	93
Placebo_2	.0024199	.0043782	-.0061614	.0110013	3570	87
Placebo_3	-.0021925	.0042983	-.0106171	.0062321	3405	86
Placebo_4	-.0002068	.0044938	-.0090145	.008601	3035	78

p-value of test that all placebos are equal to 0 = 0.35517886

Panel D. Treatment: Relatively moderate single storm (% damage to GDP < 0.04). Outcome: Saving rate

Figure A.2: De Chaisemartin and d'Haultfoeuille (2020a)'s robust dynamic treatment effects of storms on saving rates by ratio of damage to GDP groups

Notes: This is the output of the Stata command *did_multiplegt* for De Chaisemartin and d'Haultfoeuille (2020a). Each row Effect_# is the result of the heterogeneity-robust DID estimating the effect of having switched in treatment for the first time # years ago. Estimate column = estimated effect of the treatment at the time period when first-time switchers switch. SE column = Standard Error computed using 100 bootstrap replications. LB CI and UB CI columns = Lower Bound and Upper Bound of the 95% confidence interval. N column = total number of observations (first-time and not-yet switchers) used in the estimation of the treatment effect. Switchers column = number of first-time switchers the estimate applies to.

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0027767	.0052971	-.0076057	.013159	2522	101
Effect_1	-.0040772	.0075939	-.0189613	.0108069	2447	101
Effect_2	-.0096967	.0075575	-.0245094	.0051159	2368	101
Effect_3	-.007174	.0078121	-.0224856	.0081377	2270	99
Effect_4	-.0135075	.0084205	-.0300116	.0029966	2194	99
Effect_5	-.0172752	.0093937	-.0356868	.0011364	2124	99
Placebo_1	.0110578	.0054044	.0004652	.0216505	2406	98
Placebo_2	-.0058465	.0048807	-.0154126	.0037196	2325	97
Placebo_3	.0010236	.0042726	-.0073507	.0093979	2160	93
Placebo_4	-.0024478	.0045858	-.011436	.0065403	2078	91

p-value of test that all placebos are equal to 0 = 0.21676583

Panel A. Developing countries. Treatment: Any storm. Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	-.0044831	.0068635	-.0179355	.0089693	2689	73
Effect_1	-.0099948	.0094349	-.0284871	.0084975	2526	70
Effect_2	-.0112939	.0085424	-.0280369	.0054491	2335	66
Effect_3	-.0151504	.0100146	-.0347789	.0044782	2204	63
Effect_4	-.0227775	.0094583	-.0413158	-.0042393	2079	60
Effect_5	-.0225841	.0109964	-.0441371	-.001031	1897	56
Placebo_1	.0178687	.0066006	.0049316	.0308059	2408	65
Placebo_2	.0029025	.0075489	-.0118934	.0176984	2284	63
Placebo_3	-.0011309	.0058529	-.0126025	.0103406	2127	62
Placebo_4	-.0078868	.0058555	-.0193635	.0035899	1921	57

p-value of test that all placebos are equal to 0 = 0.08778749

Panel B. Developing countries. Treatment: Single storm. Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0121133	.0090903	-.0057036	.0299303	660	42
Effect_1	.0073477	.0099938	-.0122401	.0269356	637	42
Effect_2	-.0103932	.0096474	-.0293021	.0085157	617	42
Effect_3	-.0147244	.0092256	-.0328066	.0033578	595	42
Effect_4	-.0119569	.0100333	-.0316223	.0077084	575	42
Effect_5	-.0199656	.0122273	-.043931	.0039999	552	42
Placebo_1	.000461	.0052636	-.0098556	.0107775	620	41
Placebo_2	.0026586	.0045498	-.006259	.0115762	595	40
Placebo_3	-.0026441	.0063027	-.0149974	.0097091	550	38
Placebo_4	.0110838	.0058706	-.0004226	.0225902	543	38

p-value of test that all placebos are equal to 0 = 0.2080667

Panel C. Developed countries. Treatment: Any storm. Outcome: Saving rate

	Estimate	SE	LB CI	UB CI	N	Switchers
Effect_0	.0131562	.0093082	-.005088	.0314003	630	40
Effect_1	.0098475	.0108989	-.0115144	.0312094	584	39
Effect_2	-.0069839	.0112698	-.0290727	.0151049	562	38
Effect_3	-.0110273	.0114165	-.0334035	.011349	529	36
Effect_4	-.0074701	.0128267	-.0326104	.0176702	482	35
Effect_5	-.0195462	.0144325	-.0478338	.0087415	428	32
Placebo_1	.0039126	.005885	-.007622	.0154471	600	39
Placebo_2	.0016938	.0046704	-.0074602	.0108478	570	37
Placebo_3	-.0052326	.0061783	-.0173421	.0068769	529	35
Placebo_4	.0127854	.0062818	.0004731	.0250977	500	34

p-value of test that all placebos are equal to 0 = 0.20762475

Panel D. Developed countries. Treatment: Single storm. Outcome: Saving rate

Figure A.3: De Chaisemartin and d'Haultfoeuille (2020a)'s robust dynamic treatment effects of storms on saving rates by economic development groups

Notes: This is the output of the Stata command *did_multiplegt* for De Chaisemartin and d'Haultfoeuille (2020a). Each row Effect_# is the result of the heterogeneity-robust DID estimating the effect of having switched in treatment for the first time # years ago. Estimate column = estimated effect of the treatment at the time period when first-time switchers switch. SE column = Standard Error computed using 100 bootstrap replications. LB CI and UB CI columns = Lower Bound and Upper Bound of the 95% confidence interval. N column = total number of observations (first-time and not-yet switchers) used in the estimation of the treatment effect. Switchers column = number of first-time switchers the estimate applies to.