

# ESSAYS ON THE ECONOMIC IMPACT OF NATURAL DISASTERS: THE CASE OF STORMS

by

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(Under the Direction of Mateusz Filipski)

## ABSTRACT

This dissertation consists of three essays that seek to evaluate the impact of storms on prices, saving rates, and the economy. Essay 1 addresses a twofold question: Do market prices change in times of catastrophe? Are the changes in prices efficient (in that they reflect changes in supply and demand)? Building upon an optimal pricing model, the paper derives the efficient price changes and lays out an empirical framework to test whether observed price changes fall within the efficiency range. It applies the framework to assess price changes for toilet paper, batteries, milk, and bottled water due to Hurricane Harvey in the United States, using a large dataset of more than 250,000 observations on consumer demand, supplemented with hurricane and housing assistance data. It uncovers that prices change a little in the hurricane season, and these small changes are efficient in most cases. Essay 2 describes the static impact of storms on the economy using a balanced panel of 55 countries in a 69-year long period. The findings suggest no robust evidence that storms have a static effect on per capita GDP growth, private saving rate, inflation rate, depreciation rate of capital stock, employment rate, per capita labor income growth, and real internal rate of return. Essay 3 empirically investigates the dynamic impact of storm events occurring as the sole natural disaster type in a given country-year on private saving rates. It employs an event study design using a panel of 176 countries in a 69-year long period. It finds a decrease in annual private saving rates by 1.85 and 2.29 percentage points four and five years following storms, respectively, and examines four main channels that may lead to these results. 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.

INDEX WORDS: Catastrophe, Disaster, Economic growth, Event study design, Optimal pricing, Price gouging, Savings

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# DEDICATION

This dissertation is dedicated to:

My daughter, Edna Georgia Mensah, for giving me joy when I most needed.

My spouse, Ida Nadia Djenontin, for constantly inspiring me.

My parents, siblings, uncles, and grandma, for your moral support.

May you all appreciate the fruit of the resilience you taught me.

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# CHAPTER I

## INTRODUCTION

Over the globe, we live in societies in which natural disasters constantly make news, as most countries of the world cannot escape the wrath of Mother Nature. Every year bears its share of disaster damages, and people are affected by disasters in several ways (injured, homeless, needing emergency items, missing, or dead). As recorded in the country-year emergency events database collected by the Centre for Research on the Epidemiology of Disasters (CRED), the EM-DAT database,<sup>1</sup> from 1950 to 2019, there have been 14,532 natural disasters worldwide: floods represent 33% of natural disaster events, storms—23%, epidemics—12%, earthquakes—8%, droughts—7%, landslides—6%, extreme temperatures—5%, wildfires—3%, volcanic activities—2%, and others (dry mass movements, insect infestations, etc.)—1%. Figure 1.1 points to storms as not the deadliest natural disasters, but as the costliest natural disasters since storms account for 42% of the estimated disaster damage in the world from 1950 to 2019 (EM-DAT).

Due to the economic importance of storms, this dissertation addresses its economic impact by focusing on the effects of storms on prices, saving rates, and economic growth. The causal link might look obvious, but it is not simple and clear-cut. A rise in production and transaction costs of the goods in the storm season (negative supply shock), or a relief to disaster-affected consumers (positive demand shock), or both, can make prices surge. Inflation, whether it arises on the food side, fuel side, or on other sides of consumer goods, can reduce consumer spending, thereby limiting the growth of the gross domestic product (GDP). However, consumers facing soaring prices can be reluctant to curb consumption and rely on their savings.

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<sup>1</sup><http://www.emdat.be/database>

The private saving rate, as a ratio of the consumer’s disposable income, falls as long as savings are depleted faster than the disposable income grows. These economic consequences of storms can be instantaneous or linger for years.

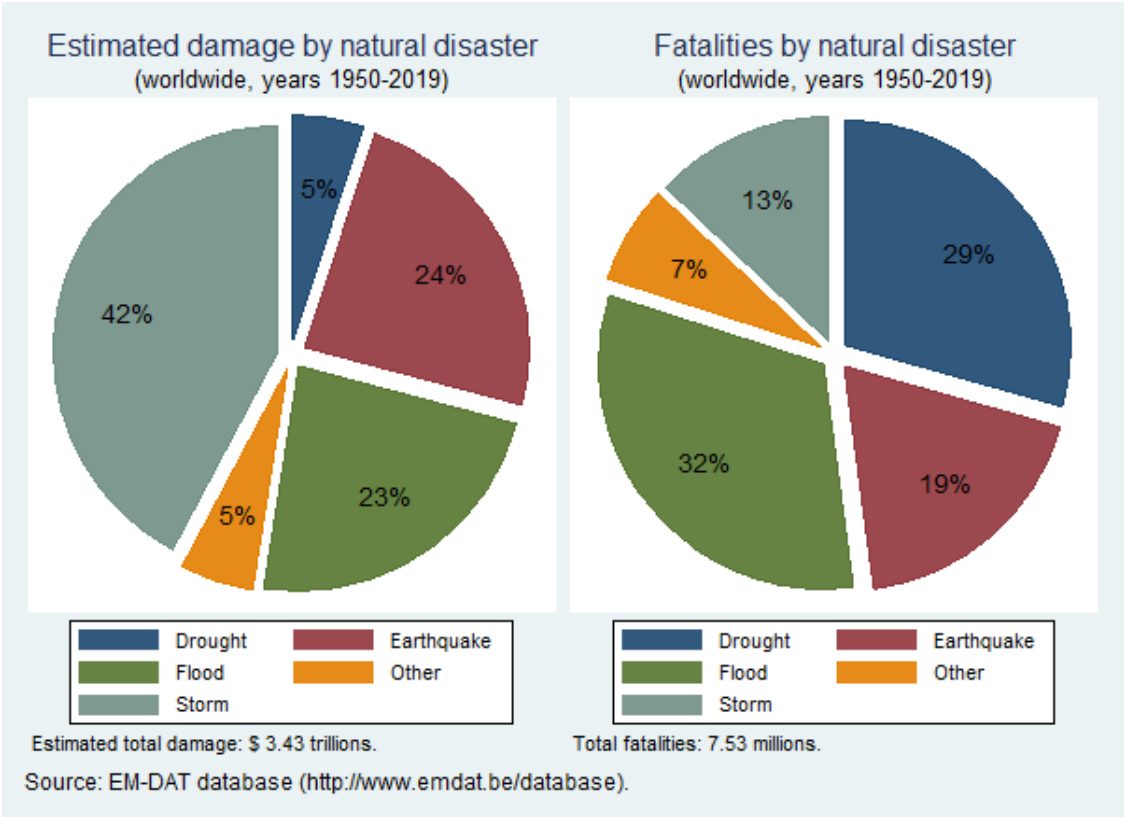


Figure 1.1: Distribution of disaster damage and deaths around the world by disaster type in years 1950 through 2019

Several studies have addressed the political economy or economics of natural disasters (Adam, 2013; Albala-Bertrand, 1993; Botzen et al., 2019; E. Cavallo et al., 2013; Cavallo & Noy, 2009; Clarke & Dercon, 2016; Felbermayr & Gröschl, 2014; Hallegatte & Przulski, 2010; Kellenberg & Mobarak, 2011; Noy & duPont IV, 2018). Rather than bogging down in this extensive literature, this dissertation focuses on storms and opts for three empirical highlights of their economic imprint, with policy outlook.

The United States experienced storm events in 68 out of 70 years between 1950 and 2019, making it the country with the most storm occurrences (EM-DAT). Due to this, Chapter 2 focuses on the United States and addresses a recurrent policy issue of “price gouging” laws in times of disasters. Price gouging is referred to as the seller’s practice of suddenly raising prices far above the pre-disaster price. Price gouging laws remain topical in the United States, with the latest Price Gouging Prevention Act of 2022<sup>2,3</sup> and Consumer Fuel Price Gouging Prevention Act,<sup>4</sup> both introduced in May 2022 to authorize the Federal Trade Commission (FTC) and state authorities to forbid unconscionably excessive price increases. That such laws, similarly to price controls, can actually improve economic efficiency while decreasing prices depend upon (1) the extent to which market power is actually present—thus, making quantities go down and prices go up—and (2) the level at which the price cap is actually placed to avoid causing shortages. Since price gouging laws occlude the objective assessment of post-disaster price increases as well as do not clarify the appropriate price caps, Chapter 2 theoretically and empirically investigates whether (1) market prices do rise in times of a storm and (2) are efficient (in that they reflect changes in supply and demand). Theoretically, it uses an optimal pricing model to show a possibly wide range of efficient price increases driven by changes in the price elasticity of demand. Empirically, it determines small price changes due to the 2017 Atlantic tropical storm Harvey—which are efficient in most cases—and suggests objective price caps for four studied products (toilet paper, batteries, milk, and bottled water).

Chapter 3 offers an empirical, multi-country insight into the immediate economic impact of storms and compares it to that of other natural disasters, such as floods, earthquakes, droughts, and epidemics. Some empirical studies uncover economic effects that are slow to manifest and do not appear in the instantaneous period in which the disaster hits (Cunado & Ferreira, 2014; Fomby et al., 2013)—which would not persuade into urging disaster management policies, due to the delayed effects. Other empirical studies estimate the contemporaneous economic effects using standard OLS or fixed effects methods (Murlidharan & Shah, 2001; Noy, 2009), which are not robust to heterogeneity in treatment effects

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<sup>2</sup><https://www.govinfo.gov/app/details/BILLS-117s4214is>

<sup>3</sup><https://www.warren.senate.gov/newsroom/press-releases/warren-baldwin-schakowsky-colleagues-introduce-bicameral-legislation-to-crack-down-on-corporate-price-gouging>

<sup>4</sup><https://www.congress.gov/bill/117th-congress/house-bill/7688/text>

(Borusyak et al., 2021; De Chaisemartin & d’Haultfoeuille, 2020b; Goodman-Bacon, 2021). Following recent methodological advances (Baker et al., 2022; De Chaisemartin & D’Haultfoeuille, 2022; Roth et al., 2022), Chapter 3 uses a heterogeneity-robust estimator (Sun & Abraham, 2021) to evaluate the static, average causal effect of natural disasters on various economic outcomes: per capita GDP growth, saving rate, inflation rate, depreciation rate of capital stock, employment rate, per capita labor income growth, and real internal rate of return. Notwithstanding, Chapter 3 just provides an introductory and descriptive highlight, as its intent is not to dig into the specific mechanisms at play from the disasters to economic growth.

Chapter 4 selects one of the economic outcomes studied in Chapter 3 and sheds light on its impact mechanisms. While it is important to apprehend how people recover from natural disasters, little is known about the role of saving in the process of disaster recovery. In this empirical study, Chapter 4 centers on the dynamic effects of storms on saving rates, applies a difference-in-difference estimator appropriate to switching in and out of storm occurrence over time (De Chaisemartin & d’Haultfoeuille, 2020a), and uses a larger sample than in Chapter 3, to provide a comprehensive story about whether and how storms affect saving rates.

Chapter 5 concludes with a digest of answers to the research questions addressed in this dissertation.

## CHAPTER 2

### FIRST ESSAY

# IMMINENT NATURAL DISASTER AND PRICE GOUGING? EVIDENCE FROM HURRICANE HARVEY IN THE US<sup>1</sup>

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<sup>1</sup>Mensah, E. R., C. Landry, and T. A. Smith, "Imminent natural disaster and price gouging? Evidence from Hurricane Harvey in the US," to be submitted to *American Economic Journal: Applied Economics*.

## 2.1 Abstract

Price gouging on necessity and emergency goods is often decried by the public in times of catastrophe. However, there is little guidance on whether and to what level price increases are reasonable. This paper addresses a twofold question: Do market prices change in times of catastrophe? Are the changes in prices efficient (in that they reflect changes in supply and demand)? Building upon an optimal pricing model, we derive the efficient price changes and lay out an empirical framework to test whether observed price changes fall within the efficiency range. We apply the framework to assess price changes for toilet paper, batteries, milk, and bottled water due to Hurricane Harvey in the United States. We find that prices change a little in the hurricane season, and these small changes are efficient in most cases.

*JEL* Codes: D12, L11, M38, Q54, Q58.

## 2.2 Introduction

In the wake of catastrophic events, such as hurricanes, earthquakes, tornadoes, terrorist attacks, and pandemics, the practice of “price gouging,” which occurs when sellers suddenly raise prices far above the pre-catastrophe price, has long been of concern for buyers and rekindled attention to laws limiting or prohibiting it. Unless costs soar up, buyers find price increases unfair (Boyle, 2009; Kahneman et al., 1986; Meyersohn, 2021) and call for government interventions to control prices (Rotemberg, 2008). While there is no Federal law on price gouging during national emergencies beyond the Defense Production Act, most states have laws explicitly forbidding “excessive” increases during declared disasters (ABA, 2020). The definition of excessive prices, however, takes one of the following three forms (Bae, 2009; Ball, 2011; Brewer, 2007; Rapp, 2005; Skarbek & Skarbek, 2008; Zwolinski, 2008): a threshold price increase ranging from 10 to 25% above the price at the time of the declaration of disaster, or market price, or pre-disaster price (in states such as Alabama, Arkansas, California, Colorado, New Jersey, Oklahoma, and West Virginia); an unconscionable price increase (in states such as Florida, New York, North Carolina, South Carolina, Tennessee, Texas, and Virginia); no price increase (in states such as Georgia, Louisiana, and others).<sup>2</sup> Determining the appropriate price percentage cap for the goods affected by the state price gouging laws is crucial for understanding when price increases should be prosecuted and enacting laws that do not set caps at a level below or above what market efficiency requires.<sup>3</sup>

Several articles have documented the pros and cons of price gouging laws (Ball, 2011; Britschgi, 2021; Dorfman, 2016; Giberson, 2011; Gilbert, 2020; Lee, 2015; Mohammed, 2013, 2017; Noy, 2018; Wilson, 2014). Proponents of price gouging laws claim moral flaws in post-catastrophe price increases on essential

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<sup>2</sup>Violation of the price gouging laws triggers potential civil and criminal penalties, but the laws permit sellers to charge price increases that actually compensate cost increases. The laws also cover various types of goods or services such as food, gasoline, medical and emergency supplies, building materials, housing, storage services, etc. A restriction may apply to the types of sellers covered. The laws are enacted to stay in effect upon a declaration of disaster by the President, or the governor of the state, or even a local authority, for a specified length of time (King & Spalding, 2021; Skarbek & Skarbek, 2008).

<sup>3</sup>As noted by the U.S. Federal Trade Commission (FTC, 2006), ‘when the question of defining unconscionability has been presented in court, judges typically have based decisions on a case-by-case factual analysis, and those decisions have not been particularly consistent (p. 191) ... given the uncertainty about what constitutes an unconscionable, excessive, or exorbitant price, and the paucity of decisions on the issue, statutes based on any of these terms are likely to be difficult to enforce (p. 192).’

goods that harm the worse-off buyers (Snyder, 2009) as well as post-catastrophe market failures due to a short-term collapse of electronic payment systems in catastrophe-affected areas and the use of biased heuristics in decision-making by the market actors (Rapp, 2005). Price gouging laws, in that view, ensure necessity goods are accessible to buyers on familiar prices in times of a catastrophe, and foster distributive justice by not marketing-out the poorest classes of the catastrophe-affected communities (Snyder, 2009). In addition, the laws would work towards reducing sellers' overestimation of the market impacts of a catastrophe (Rapp, 2005). Furthermore, under certain circumstances, price gouging laws are said to increase economic welfare, but do not maximize it (Fleck, 2014): such circumstances occur when the consumption of the target good (e.g., flu vaccinations) is socially beneficial, whereas the price gouging law is expected to induce shortages in times of high demand; then rational consumers increase consumption, which generates a higher social surplus.

Opponents of price gouging laws also make ethical and economic arguments for price flexibility following a catastrophe. First, there are immorality issues in price gouging laws because the legislation stipulates unclear and imprecise price caps that fail to account for the morality behind price mechanisms. By instituting price ceiling, the laws demonstrate a lack of respect for both sellers and buyers by preventing them from engaging in economic exchanges at the market-clearing price (Zwolinski, 2008, 2009). Second, there are no market failure issues in price gouging for multiple reasons. The fact that the most economically vulnerable buyers are willing to pay higher prices on the goods they need the most in times of catastrophe is a signal that the demand is elastic and that the market works well (Brewer, 2007; Skarbek, 2007; Zwolinski, 2008). Near-rational behavior in the economy does not preclude Pareto-optimality (Akerlof & Yellen, 1985) and optimal pricing (Dixon, 2020). The fact that market actors lack perfect information on their electronic resources and perfect rationality in their decision-making in the wake of catastrophes does not mean that the market fails but that perfect competition breaks. Because the market is a dynamic process of discovery, when it is free, even in the absence of perfect competition, allocative efficiency is still possible so long as prices call forth supply and communicate scarcity and demand of the goods that are exchanged (Brewer, 2007; Skarbek, 2007; Zwolinski, 2010). Higher prices spur sellers to increase supply

and entrepreneurs to innovate in catastrophe-affected areas (Culpepper & Block, 2008; Skarbek & Skarbek, 2008). Rather than maximizing welfare, price restrictions create economic losses (Horwich, 1990; Montgomery et al., 2007) and exacerbate shortages (Brewer, 2007). Capped prices amplify panic buying during a catastrophe, leading to hoarding (Chakraborti & Roberts, 2021; Culpepper & Block, 2008; Noda & Teramoto, 2021), even though this might apply only to better-off buyers (Noy, 2018).

That prices should change with spikes in demand or interruptions to supply is standard economic theory; textbooks clearly indicate how market prices respond in predictable ways to such shocks. Nonetheless, price as a mechanism for resource allocation over the short, medium, and long run can be assessed from perspectives of efficiency or equity. The question of market performance and government intervention in allocating essential goods and services has been explored in the domain of housing (Rosen & Smith, 1983), healthcare (Smith, 2005), water (Rogers et al., 2002), transportation (Levinson, 2010), in addition to many other areas. No doubt, proponents of trade liberalism will continue to debate advocates for economic and social justice on proper mechanisms for managing the economy (Baiman, 2001, 2002; Baldwin & Evenett, 2020). Rather than wade into this deep and entrenched literature, in this paper, we take a different approach to analyzing price changes in the wake of a disaster.

Recognizing both efficiency and equity concerns, we build on current optimal pricing literature (Adams & Williams, 2019; Badra & Gaumont, 2020; DellaVigna & Gentzkow, 2019; N. Yang & Zhang, 2021) to characterize efficient price changes in times of emergency. We show how optimal pricing relates to price elasticity of demand, and we lay out the framework for testing whether observed price changes are within a range of efficient, expected responses to market forces or whether price changes fall within a range of values that are likely to be excessive and inefficient (as generally assumed with price gouging). Simple simulations for necessity goods with a marginal cost of \$1 or \$2 show that current state price gouging laws setting price percentage caps of 0 to 25% for those goods are problematic; these laws enable efficient post-disaster prices only when the disaster induces very small changes in the parameter of the price elasticity of demand. Strikingly, the simulations reveal that efficient price percentage caps are above 100% in most situations, and price increases by up to nineteen times the pre-disaster prices can still be efficient in certain

situations. The practical implications are that most lawsuits in cases of price gouging could be allegations not backed up by economic evidence of excessive price increases.<sup>4</sup>

Beside the simulations, we illustrate the framework using the case of Hurricane Harvey in Texas. Estimation results across all counties in the state indicate that, relative to pre-hurricane price levels, toilet paper prices are expected to rise by up to 3.087 times at warning, 2.922 times during landfall, and 2.749 times in the first three weeks following the strike, while battery prices are expected not to change in any period examined. Milk prices are expected to rise only during landfall and by up to 1.545 times their pre-hurricane levels. For bottled water, efficiency rules that relative to the pre-hurricane levels, prices are expected to rise by up to 1.375 times at warning, 2.831 times during landfall, and 3.847 times after the strike. Although the efficient cap is above 200% for toilet paper, we observe demand-driven toilet paper price increases, not exceeding 3% in all periods examined. The efficient zero-cap for battery price increases is also achieved in all periods except during landfall when prices rise inefficiently by about 3%. Results for changes in observed milk prices indicate insignificant price differences at warning and in the three weeks following landfall, as expected; the deviation from efficiency occurs during landfall, with prices falling by less than 2%. Results for bottled water also indicate price increases by less than 3% at warning, during landfall, and after the strike, which all fall within expected increases.

While it can make sense for catastrophes such as pandemics to be generalized to the entire state, hurricanes and other natural disasters are catastrophes with localized incidences. As noted by the National Oceanic and Atmospheric Administration (NOAA), hurricane-stricken areas are localized in coastal counties. Focusing on coastal counties, we observe that toilet paper prices rise by 0.4% at warning, 8.4% during landfall, and 0.3% after strike, which all fall below the allowable proportional price increase of 22% in all periods examined for this good. Because changes in price elasticity of demand suggest that battery prices should remain constant in all periods examined, we find the rise by 12.2% above pre-hurricane levels

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<sup>4</sup>One of the price gouging lawsuits in the state of Texas in 2017 was about a retailer raising the price of regular-grade gasoline from \$2.29 per gallon on August 30, 2017, to \$6.99 per gallon five days after Hurricane Harvey's landfall (Oller, 2017). The U.S. Federal Trade Commission also reported cases where gasoline prices increased by 200 to 300% after Hurricane Katrina (FTC, 2006) (p. ix). Zwolinski, 2008 (p. 363) reported a case of price gouging during the strike of Hurricane Fran in the state of North Carolina, in which the merchant purchased ice for \$1.70 per bag, but sold it six times more for \$12 per bag.

after the strike inefficient; there are no significant difference between observed and efficient changes in battery prices at warning and during landfall. Because milk prices are expected to soar only during landfall and by up to 85.9% relative to the pre-catastrophe price levels, the observed price increases at warning (4.9%), during landfall (8.4%), and after strike (7.3%), are all demand-driven. For bottled water, efficiency rules that demand-driven prices should drop by up to 43.2% at warning, 39.9% during landfall, and 36.3% after strike. We observe that bottled water prices fall by 2.2% and 0.3% during landfall and after the strike, respectively, which is within the range of expected drops in prices at those periods; however, at warning, bottled water prices rise by 2.3%, while efficiently, they should drop in that period.

Our paper contributes to document insights about price responses to a catastrophe and the assessment of price gouging in three ways. First, our findings about modest price changes due to Hurricane Harvey add to prior evidence of pervasive, unapparent changes in retail prices due to hurricanes (Beatty et al., 2021; Gagnon & López-Salido, 2020), snowstorms (Gagnon & López-Salido, 2020), earthquakes (A. Cavallo et al., 2014), peak demand seasons (Chevalier et al., 2003; MacDonald, 2000), and weekends of peak demand seasons (Warner & Barsky, 1995). We complement this literature by showing that the relatively stable prices in the hurricane season studied remain demand-driven because the observed price changes mostly fall within the efficiency ranges allowed by the changes in the price elasticity of demand. Thus, we provide supportive evidence to the absence of price gouging for the four goods studied. Second, by combining a structural demand model with a reduced-form analysis, we suggest allowable price caps, which are good-specific, period-wise, and location-specific. Finally, our methodology elaborates a framework for testing price response to a catastrophe, which is applicable not only to hurricanes but also to events, such as floods, earthquakes, pandemics, and other disasters that may affect demand within a short-term horizon.

Section 2.3 exposes the basic logic of this paper, which can be expressed as the impact of the exposure to a catastrophe on the optimal pricing of a necessity good, led by changes in the price elasticity of demand. Section 2.4 describes the data and explains the methodology used for the empirical analyses. Section 2.5 presents the estimation results, and robustness analyses follow in Section 2.6. The conclusion is in Section 2.7. All appendix material is in Appendix A.

## 2.3 Theoretical framework

### 2.3.1 Optimal pricing model

To construct a test for price efficiency, we adapt the optimal pricing model from DellaVigna and Gentzkow, 2019 to capture expected market price changes between pre- and post-catastrophe periods. In this simple model, we assume a retailer  $r$  selling a product  $j$  in its store  $s$  in period  $t \in \{0, 1, 2, 3, 4, 5\}$  chooses monopolistically competitive prices  $P_{rsjyw}$  in week  $w$  of year  $y$  to maximize total profits. The periods are defined as follows:  $t = 0$  is the pre-catastrophe period standing for non-catastrophe years,  $t = 1$  is the period before warning for the catastrophe,  $t = 2$  is the period of warning for the catastrophe,  $t = 3$  is the instantaneous period when the catastrophe hits,  $t = 4$  is the immediate aftermath of the catastrophe (short-term), and  $t = 5$  is the medium-to-long aftermath of the catastrophe.<sup>5</sup>

Residual demand for product  $j$  in store  $s$  in week  $w$  is described by a semi-log function of product price<sup>6</sup>:  $Q_{rsjyw} = k_{sj}e^{\eta_{sj}(t)} = k_{sj}e^{b_{sj}(t)*P_{rsjyw}}$ , where  $Q_{rsjyw}$  represents the quantity of product  $j$  sold in store  $s$  in week  $w$  of year  $y$ ,  $k_{sj}$  is a product-store-specific scale term, and  $\eta_{sj}(t) = b_{sj}(t) * P_{rsjyw}$  is the price elasticity of demand that is linear in product price. The elasticity parameter  $b_{sj}(t)$  is assumed to change between periods as the exposure level of the location of the store to a catastrophe varies across periods.

The stores for each retailer have identical product-specific marginal cost  $c_j$  but different fixed costs  $C_{sj}$ . The retailer's problem is

$$\max_{P_{rsjyw}} \sum_{s,j} (P_{rsjyw} - c_j) * Q_{rsjyw}(P_{rsjyw}) - \sum_s C_{sj}. \quad (\text{Eq.2.1})$$

<sup>5</sup>Post-catastrophe period refers to catastrophe years ( $t > 0$ ) even though the catastrophe strikes from  $t = 3$ .

<sup>6</sup>We depart from DellaVigna and Gentzkow, 2019 who use a log-log demand functional form, implying a constant price elasticity of demand. Using a log-log demand functional form, optimal prices are defined for elasticities that do not fall within the range  $[-1, 0]$ , while values between -1 and 0 cannot be ruled out for the price elasticity of demand for necessity and emergency goods such as those studied in the present paper.

The first-order condition to this maximization problem yields the optimal price  $P_{rsjyw}^*$ , which satisfies

$$P_{rsjyw}^* = c_j - \frac{1}{b_{sj}^*(t)} \quad , \quad b_{sj}^*(t) \neq 0. \quad (\text{Eq.2.2})$$

### 2.3.2 Comparative statics and simulations

By allowing the store-product price elasticity parameter to vary across time, comparative statics on Eq.2.2 generates one insightful result expressed as follows:

#### **Result**

Efficient price changes between periods satisfy the condition:

$$\frac{dP_{rsjyw}^*}{dt} = \frac{\frac{\partial b_{sj}^*(t)}{\partial t}}{(b_{sj}^*(t))^2} \quad , \quad b_{sj}^*(t) \neq 0. \quad (\text{Eq.2.3})$$

**Proof:** see Appendix A.I.

This theoretical result is a basic conclusion from the implicit function theorem,<sup>7</sup> suggesting how prices are sensitive to the catastrophe. Using Eq.2.3, we can assess optimal price dynamics as long as the store-product demand is initially not perfectly inelastic, *i.e.*  $b_{sj}^*(t) \neq 0$ . Efficient price changes between periods respond to changes in the parameter of the price elasticity of demand between periods, in the same direction:  $Sign\left(\frac{dP_{rsjyw}^*}{dt}\right) = Sign\left(\frac{\partial b_{sj}^*(t)}{\partial t}\right)$ . If the elasticity parameter does not vary between periods, then prices are not expected to change between periods.

Simulations based on Eq.2.3 offer practical tools to assess price gouging claims. As an illustrative example, consider the following cases: (1) price gouging during the strike of Hurricane Fran in the state of North Carolina, in which the merchant purchased ice for \$1.70 per bag, but sold it six times more for \$12 per bag (Zwolinski, 2008) (p. 363); (2) price gouging lawsuits in the state of Texas in 2017 about a retailer raising the price of regular-grade gasoline from \$2.29 per gallon on August 30, 2017, to \$6.99 per

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<sup>7</sup>Although  $t$  is defined in a discrete form,  $t$  increases in a continuous exposure level of the location of the store. This makes it possible not to perform monotone comparative statics, which would require additional assumptions on supermodularity and monotonicity (Milgrom & Shannon, 1994).

gallon five days after Hurricane Harvey landfall (Oller, 2017). A natural question in these cases is “Can such goods, with marginal cost of about \$1 or \$2, be priced twofold or more as a hurricane wreaks havoc?” Inferences drawn from our theoretical result provide an answer to this question: “Yes! Depending on the values of the pre-hurricane parameter of the price elasticity of demand  $b_{sj}^*(t = 0)$  and the values of the change in the parameter of the price elasticity of demand  $\frac{\partial b_{sj}^*(t)}{\partial t}$  from  $t = 0$  to the post-catastrophe period of interest, proportional price increases could reach 100%, and above.” Figure 2.1 for  $c_j = 1$  and Figure 2.2 for  $c_j = 2$  indicate that current state price gouging laws setting price percentage caps of 0 to 25% for those goods are problematic; these laws enable efficient post-disaster prices only when the hurricane induces very small changes in the parameter of the price elasticity of demand. Strikingly, the simulations reveal that price increases by up to nineteen (Figure 2.1) and eighteen (Figure 2.2) times the pre-disaster levels can still be efficient in certain situations. Thus, unless product-specific marginal cost and demand (pre-disaster and changes) are known, most lawsuits in cases of price gouging could be allegations not backed up by economic evidence of excessive price increases.

### 2.3.3 Implications for price efficiency testing strategy

Building upon the optimal pricing model and comparative statics above, we can lay out our price efficiency testing strategy. For each product  $j$ , we can estimate a semi-log demand equation using store-fixed effects—as price gouging is a store-based practice (Cabral & Xu, 2021)—and obtain the elasticity parameter  $b_{sj}^*$  at a given period  $t = 0$ . If the estimation sample includes several periods, then the demand equation can also be specified such as to make it flexible to obtain  $\frac{\partial b_{sj}^*(t)}{\partial t}$ , that is the change in the elasticity parameter from period  $t = 0$  to a specific period of interest, say  $t = 2, 3, 4$ .

		Values of the pre-disaster parameter of the price elasticity of demand $b_{sj}^*(t = 0)$																		
		-0.05	-0.10	-0.15	-0.20	-0.25	-0.30	-0.35	-0.40	-0.45	-0.50	-0.55	-0.60	-0.65	-0.70	-0.75	-0.80	-0.85	-0.90	-0.95
Values of the change in the parameter of the price elasticity of demand	0.00	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
	0.05	95%	45%	29%	21%	16%	13%	11%	9%	8%	7%	6%	5%	5%	4%	4%	3%	3%	3%	3%
	0.10	190%	91%	58%	42%	32%	26%	21%	18%	15%	13%	12%	10%	9%	8%	8%	7%	6%	6%	5%
	0.15	286%	136%	87%	63%	48%	38%	32%	27%	23%	20%	18%	16%	14%	13%	11%	10%	10%	9%	8%
	0.20	381%	182%	116%	83%	64%	51%	42%	36%	31%	27%	23%	21%	19%	17%	15%	14%	13%	12%	11%
	0.25	476%	227%	145%	104%	80%	64%	53%	45%	38%	33%	29%	26%	23%	21%	19%	17%	16%	15%	13%
	0.30	571%	273%	174%	125%	96%	77%	63%	54%	46%	40%	35%	31%	28%	25%	23%	21%	19%	18%	16%
	0.35	667%	318%	203%	146%	112%	90%	74%	63%	54%	47%	41%	36%	33%	29%	27%	24%	22%	20%	19%
	0.40	762%	364%	232%	167%	128%	103%	85%	71%	61%	53%	47%	42%	37%	34%	30%	28%	25%	23%	22%
	$\frac{\partial b_{sj}^*(t)}{\partial t}$	0.45	857%	409%	261%	188%	144%	115%	95%	80%	69%	60%	53%	47%	42%	38%	34%	31%	29%	26%
0.50		952%	455%	290%	208%	160%	128%	106%	89%	77%	67%	59%	52%	47%	42%	38%	35%	32%	29%	27%
0.55		1048%	500%	319%	229%	176%	141%	116%	98%	84%	73%	65%	57%	51%	46%	42%	38%	35%	32%	30%
0.60		1143%	545%	348%	250%	192%	154%	127%	107%	92%	80%	70%	63%	56%	50%	46%	42%	38%	35%	32%
0.65		1238%	591%	377%	271%	208%	167%	138%	116%	100%	87%	76%	68%	61%	55%	50%	45%	41%	38%	35%
0.70		1333%	636%	406%	292%	224%	179%	148%	125%	107%	93%	82%	73%	65%	59%	53%	49%	45%	41%	38%
0.75		1429%	682%	435%	313%	240%	192%	159%	134%	115%	100%	88%	78%	70%	63%	57%	52%	48%	44%	40%
0.80		1524%	727%	464%	333%	256%	205%	169%	143%	123%	107%	94%	83%	75%	67%	61%	56%	51%	47%	43%
0.85		1619%	773%	493%	354%	272%	218%	180%	152%	130%	113%	100%	89%	79%	71%	65%	59%	54%	50%	46%
0.90		1714%	818%	522%	375%	288%	231%	190%	161%	138%	120%	106%	94%	84%	76%	69%	63%	57%	53%	49%
0.95	1810%	864%	551%	396%	304%	244%	201%	170%	146%	127%	111%	99%	89%	80%	72%	66%	60%	56%	51%	
1.00	1905%	909%	580%	417%	320%	256%	212%	179%	153%	133%	117%	104%	93%	84%	76%	69%	64%	58%	54%	

Figure 2.1: Efficient price increases on necessity and emergency goods produced at a marginal cost of \$1

Notes: This figure shows the computed proportional price increases in percentage  $\frac{dP_{rsjyw}^*}{dt} * \frac{100}{P_{rsjyw}^*}$  for  $c_j = 1$  and different values of  $b_{sj}^*(t = 0) \in (-1, 0)$  and  $\frac{\partial b_{sj}^*(t)}{\partial t} \in [0, 1]$ , with  $\frac{dP_{rsjyw}^*}{dt}$  given by the formula in Eq.2.3 and  $P_{rsjyw}^*$  given by the formula in Eq.2.2. For example, if  $c_j = 1$ ,  $b_{sj}^*(t = 0) = -0.65$ , and  $\frac{\partial b_{sj}^*(t)}{\partial t} = 0.55$ , it follows that  $P_{rsjyw}^* = \$2.54$ ,  $\frac{dP_{rsjyw}^*}{dt} = \$1.30$ , and  $\frac{dP_{rsjyw}^*}{dt} * \frac{100}{P_{rsjyw}^*} = 51\%$ . The figure indicates situations where efficient, threshold price percentage caps do not exceed 25% and those situations where the thresholds are 100% and above.

		Values of the pre-disaster parameter of the price elasticity of demand $b_{sj}^*(t = 0)$																		
		-0.05	-0.10	-0.15	-0.20	-0.25	-0.30	-0.35	-0.40	-0.45	-0.50	-0.55	-0.60	-0.65	-0.70	-0.75	-0.80	-0.85	-0.90	-0.95
Values of the change in the parameter of the price elasticity of demand	0.00	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%	0%
	0.05	91%	42%	26%	18%	13%	10%	8%	7%	6%	5%	4%	4%	3%	3%	3%	2%	2%	2%	2%
	0.10	182%	83%	51%	36%	27%	21%	17%	14%	12%	10%	9%	8%	7%	6%	5%	5%	4%	4%	4%
	0.15	273%	125%	77%	54%	40%	31%	25%	21%	18%	15%	13%	11%	10%	9%	8%	7%	7%	6%	5%
	0.20	364%	167%	103%	71%	53%	42%	34%	28%	23%	20%	17%	15%	13%	12%	11%	10%	9%	8%	7%
	0.25	455%	208%	128%	89%	67%	52%	42%	35%	29%	25%	22%	19%	17%	15%	13%	12%	11%	10%	9%
	0.30	545%	250%	154%	107%	80%	63%	50%	42%	35%	30%	26%	23%	20%	18%	16%	14%	13%	12%	11%
	0.35	636%	292%	179%	125%	93%	73%	59%	49%	41%	35%	30%	27%	23%	21%	19%	17%	15%	14%	13%
	0.40	727%	333%	205%	143%	107%	83%	67%	56%	47%	40%	35%	30%	27%	24%	21%	19%	17%	16%	15%
	0.45	818%	375%	231%	161%	120%	94%	76%	63%	53%	45%	39%	34%	30%	27%	24%	22%	20%	18%	16%
	0.50	909%	417%	256%	179%	133%	104%	84%	69%	58%	50%	43%	38%	33%	30%	27%	24%	22%	20%	18%
	0.55	1000%	458%	282%	196%	147%	115%	92%	76%	64%	55%	48%	42%	37%	33%	29%	26%	24%	22%	20%
	0.60	1091%	500%	308%	214%	160%	125%	101%	83%	70%	60%	52%	45%	40%	36%	32%	29%	26%	24%	22%
	0.65	1182%	542%	333%	232%	173%	135%	109%	90%	76%	65%	56%	49%	43%	39%	35%	31%	28%	26%	24%
	0.70	1273%	583%	359%	250%	187%	146%	118%	97%	82%	70%	61%	53%	47%	42%	37%	34%	31%	28%	25%
	0.75	1364%	625%	385%	268%	200%	156%	126%	104%	88%	75%	65%	57%	50%	45%	40%	36%	33%	30%	27%
	0.80	1455%	667%	410%	286%	213%	167%	134%	111%	94%	80%	69%	61%	54%	48%	43%	38%	35%	32%	29%
	0.85	1545%	708%	436%	304%	227%	177%	143%	118%	99%	85%	74%	64%	57%	51%	45%	41%	37%	34%	31%
	0.90	1636%	750%	462%	321%	240%	188%	151%	125%	105%	90%	78%	68%	60%	54%	48%	43%	39%	36%	33%
0.95	1727%	792%	487%	339%	253%	198%	160%	132%	111%	95%	82%	72%	64%	57%	51%	46%	41%	38%	34%	
1.00	1818%	833%	513%	357%	267%	208%	168%	139%	117%	100%	87%	76%	67%	60%	53%	48%	44%	40%	36%	

Figure 2.2: Efficient price increases on necessity and emergency goods produced at a marginal cost of \$2

Notes: This figure shows the computed proportional price increases in percentage  $\frac{dP_{rsjyw}^*}{dt} * \frac{100}{P_{rsjyw}^*}$  for  $c_j = 2$  and different values of  $b_{sj}^*(t = 0) \in (-1, 0)$  and  $\frac{\partial b_{sj}^*(t)}{\partial t} \in [0, 1]$ , with  $\frac{dP_{rsjyw}^*}{dt}$  given by the formula in Eq.2.3 and  $P_{rsjyw}^*$  given by the formula in Eq.2.2. For example, if  $c_j = 2$ ,  $b_{sj}^*(t = 0) = -0.65$ , and  $\frac{\partial b_{sj}^*(t)}{\partial t} = 0.55$ , it follows that  $P_{rsjyw}^* = \$3.54$ ,  $\frac{dP_{rsjyw}^*}{dt} = \$1.30$ , and  $\frac{dP_{rsjyw}^*}{dt} * \frac{100}{P_{rsjyw}^*} = 37\%$ . The figure indicates situations where efficient, threshold price percentage caps do not exceed 25% and those situations where the thresholds are 100% and above.

Provided that we reject the equality of the elasticity parameter  $b_{sj}^*(t = 0)$  to zero, we can compute  $\frac{dP_{rsjyw}^*}{dt}$  following the theoretical result in Eq.2.3 and assess price efficiency in any of the three following cases.

**Case 1:**  $\frac{\partial b_{sj}^*(t)}{\partial t} > 0$ .

If  $\frac{\partial b_{sj}^*(t)}{\partial t} > 0$ , then observed price differences from period  $t = 0$  to the period of interest  $t = 2, 3, 4$  falling within the range  $\left[0, \frac{dP_{rsjyw}^*}{dt}\right]$  are demand-driven and efficient. In this case, if observed price differences are strictly higher than  $\frac{dP_{rsjyw}^*}{dt}$ , then we have an inefficient price increase. If observed price differences are strictly negative (drop in observed prices from  $t = 0$  to the post-catastrophe period of interest), then we do not have price increase even though this is inefficient.

**Case 2:**  $\frac{\partial b_{sj}^*(t)}{\partial t} = 0$ .

If  $\frac{\partial b_{sj}^*(t)}{\partial t} = 0$ , then we do not expect differences in observed prices from period  $t = 0$  to the period of interest  $t = 2, 3, 4$ . Significant differences in observed prices, in any direction, deviate from this rule and indicate inefficiency. In this case, if observed price differences are strictly positive, then we have an inefficient price increase. If observed price differences are strictly negative, then we do not have price increase even though this remains inefficient.

**Case 3:**  $\frac{\partial b_{sj}^*(t)}{\partial t} < 0$

If  $\frac{\partial b_{sj}^*(t)}{\partial t} < 0$ , then observed price differences from period  $t = 0$  to the period of interest  $t = 2, 3, 4$  falling within the range  $\left[\frac{dP_{rsjyw}^*}{dt}, 0\right]$  are efficient. In this case, if observed price differences are strictly positive, then we have an inefficient price increase. if observed price differences are strictly lower than  $\frac{dP_{rsjyw}^*}{dt}$ , then we do not have price increase even though this remains inefficient.

Now, how do we get the observed price differences from period  $t = 0$  to a period of interest  $t = 2, 3, 4$ ? Using a regression of observed prices on indicator variables for the periods and store-fixed effects, an estimate of the difference in observed prices between  $t = 0$  and the post-catastrophe period of interest can

be obtained. We illustrate this price efficiency testing strategy for Hurricane Harvey. Focusing on toilet paper, batteries, milk, and bottled water, we assess price efficiency at multiple times in the hurricane year.

## **2.4 Empirical application**

### **2.4.1 Data description**

An empirical test of the result from the optimal pricing model presented above requires residual demand and catastrophe data. Residual demand data can be pulled from either retail or consumer data. The Nielsen Company (US), LLC, via the Kilts Center at the University of Chicago, provides extensive store-level scanner data, which can be used to track store panel data for several years that can span pre- and post-periods relative to selected catastrophes occurring prior to 2018. Besides the retail scanner data, the Nielsen Company (US), LLC, also provides extensive consumer-level scanner data, which can be aggregated at the store level for the same exercise. Our study employs the consumer approach provided by the Nielsen Company (US), LLC, to recover residual demand data. Product candidates for price gouging analysis are necessity and emergency goods (staple foods, household essentials, and drugs). Gasoline, toilet paper, hand sanitizer, batteries, flashlights, bottled water, milk, and bread are a few examples, and we choose to focus on toilet paper, batteries, milk, and bottled water.

Catastrophe data to merge with residual demand data can be pulled from various sources on the basis of the target catastrophes. Johns Hopkins Coronavirus Resource Center provides detailed COVID-19 case tracker at zip code level. The National Oceanic and Atmospheric Administration (NOAA) provides timely ocean-related hazard data, with a possibility of using archived way back machine data. The Federal Emergency Management Agency (FEMA) also provides several proxy measures of exposure to a diverse range of catastrophes (hurricanes, severe storms, floods, earthquakes, tornados, fires, typhoons, volcanoes, etc.) in years 2002-2020. FEMA's online portal grants open access to weekly state, county, or zip code-level data on the number of applications to housing assistance programs or other individual assistance programs, and the number of applications reporting a need for emergency items or food. With our focus

on hurricanes, we source hurricane warning data from NOAA, via its National Centers for Environmental Information (NCEI), which we supplement by NOAA way back machine data and Google search with date range customized to the hurricane season of interest. Our data source for applications to individual assistance programs and applications reporting needs is FEMA.

Our empirical data covers four consecutive years (2014-2017) and six states (Florida, Georgia, North Carolina, South Carolina, Texas, and Virginia) within the United States as shown in Figure 2.3. FEMA reported zero individual assistance (IA) applications in the period 2014 through 2015,<sup>8</sup> but several IA applications in the period 2016 through 2017 in the states of Florida, Georgia, North Carolina, South Carolina, Texas, Virginia, Puerto Rico, and U.S. Virgin Islands.<sup>9</sup> In the period 2016 through 2017 that defines catastrophe period in the sample, NOAA reported coastal watches, warnings, monetary damages, and deaths associated with five hurricanes in the 2016 and 2017 Atlantic seasons in the United States.

In the 2017 Atlantic season, Hurricanes Harvey, Irma, and Maria occurred between 17 August and 30 September, and were the costliest hurricanes to affect the United States. Hurricane Harvey<sup>10</sup> decimated Florida, Georgia, North Carolina, Texas, and other states from 17 August to 1 September. Hurricane Irma<sup>11</sup> hit Florida, Georgia, South Carolina, and other states from 30 August to 12 September. Hurricane Maria<sup>12</sup> struck Florida, North Carolina, Virginia, and other states from 16 to 30 September.

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<sup>8</sup>FEMA also reported zero IA applications in years 2006, 2007, 2009, and 2013. While the period 2014 through 2015 defines a non-catastrophe period following FEMA data source, NOAA reported coastal watches and warnings in Florida, Georgia, North Carolina, South Carolina, and Virginia for hurricane Arthur from 1 to 5 July in 2014 ([https://www.nhc.noaa.gov/data/tcr/AL012014\\_Arthur.pdf](https://www.nhc.noaa.gov/data/tcr/AL012014_Arthur.pdf)).

<sup>9</sup>The consumer-level scanner data provided by Nielsen Company (US), LLC, has no households representing Puerto Rico, and U.S. Virgin Islands.

<sup>10</sup>[https://www.nhc.noaa.gov/data/tcr/AL092017\\_Harvey.pdf](https://www.nhc.noaa.gov/data/tcr/AL092017_Harvey.pdf)

<sup>11</sup>[https://www.nhc.noaa.gov/data/tcr/AL112017\\_Irma.pdf](https://www.nhc.noaa.gov/data/tcr/AL112017_Irma.pdf)

<sup>12</sup>[https://www.nhc.noaa.gov/data/tcr/AL152017\\_Maria.pdf](https://www.nhc.noaa.gov/data/tcr/AL152017_Maria.pdf)

Distribution of Household-UPC-Year-Day purchase observations by state in the sample

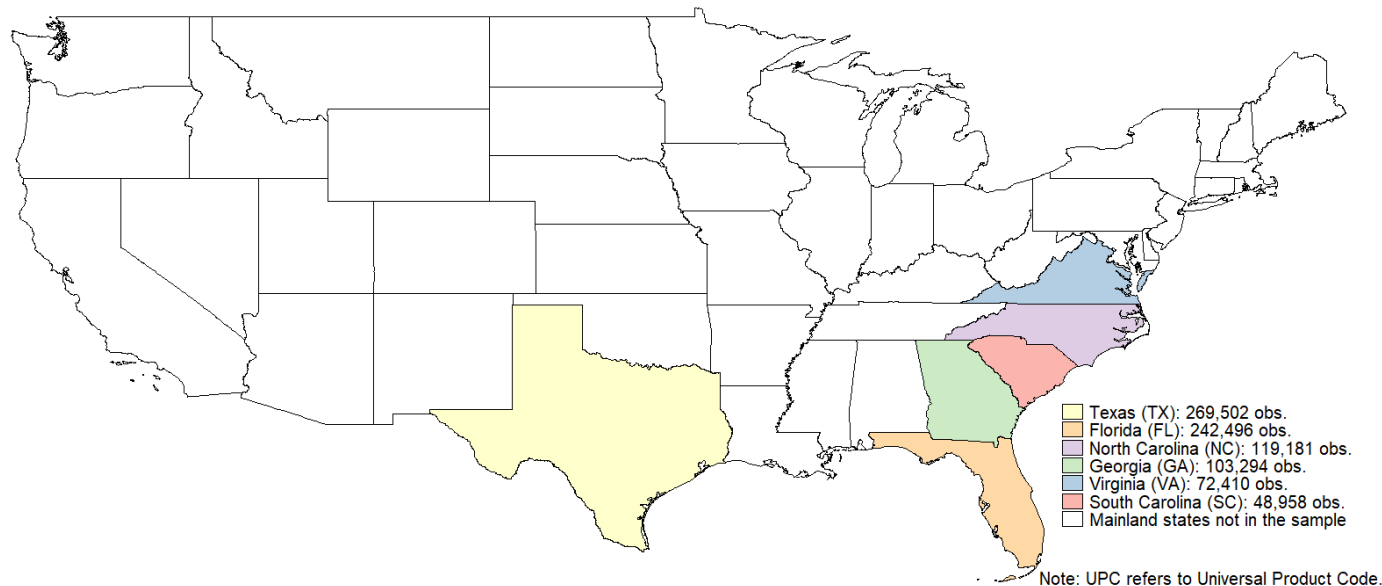


Figure 2.3: Map of the study area

*Notes:* This map shows the states where warning was issued for Hurricanes Harvey, Irma, Maria, Hermine, and Matthew, in our sample. Harvey warning state includes only Texas. Harvey season runs from August 17th through September 1st in the year 2017. Irma warning states include Florida, Georgia, and the Carolinas. Irma season runs from August 30th through September 12th in the year 2017. Maria warning states include North Carolina and Virginia. Maria season runs from September 16th through 30th in the year 2017. Hermine warning states include Florida, Georgia, the Carolinas, and Virginia. Hermine season runs from August 28th through September 3rd in the year 2016. Matthew warning states also include Florida, Georgia, the Carolinas, and Virginia. Matthew season runs from September 28th through October 9th in the year 2016.

In the 2016 Atlantic season, Hurricanes Hermine and Matthew caused monetary damages and deaths in approximately the same period as hurricanes Harvey, Irma, and Maria: between 28 August and 9 October. Hurricane Hermine<sup>13</sup> hit Florida, Georgia, North Carolina, South Carolina, Virginia, and other states from 28 August to 3 September. Hurricane Matthew<sup>14</sup> struck Florida, Georgia, North Carolina, South Carolina, Virginia, and other states from 28 September to 9 October.

Table 2.1 presents the sample size in Harvey warning state, Texas, disaggregated by the studied emergency and necessity products (toilet paper, batteries, milk, and bottled water). In the the household sample (Panel A or B) and store sample (Panel C or D), purchase observations on bottled water and toilet paper represent more than 80% of the sample. Each purchase year represents about one fourth of the household sample (Panel A or B) and store sample (Panel C or D). Pre-hurricane observations (years 2014 and 2015) and post-hurricane observations (years 2016 and 2017) are equally represented in the sub-sample of each product. Coastal counties represent about 30% of the sample of complete observations in each panel and for each product. In Harvey season, warnings are issued in many counties, most of which are coastal counties as shown in Figure 2.4. In the store sample, in Harvey season, the average proportion of applications to housing assistance is about 13% in coastal counties, which is seventeen times the annual proportion in coastal counties and twenty-six times the average proportion in non-coastal counties. Applications reporting a need for shelter, special accommodations, and emergency items or food, are also concentrated in coastal counties in Harvey season (Table A.1).

We use product purchase observations in the household sample to compute price as the ratio of household's expenditure to units purchased. Units are count for toilet paper and batteries, but ounces for milk and bottled water. To standardize prices, we use Aguiar and Hurst, 2007's price index, as suggested in Beatty, 2010. Price indices are centered around one as expected from the index construction (Table A.2), which facilitates the computation of the price elasticity of a semi-log demand.

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<sup>13</sup>[https://www.nhc.noaa.gov/data/tcr/AL092016\\_Hermine.pdf](https://www.nhc.noaa.gov/data/tcr/AL092016_Hermine.pdf)

<sup>14</sup>[https://www.nhc.noaa.gov/data/tcr/AL092016\\_Hermine.pdf](https://www.nhc.noaa.gov/data/tcr/AL092016_Hermine.pdf)

Table 2.1: Sample size by product in the state of Texas

	Toilet paper (N)	Batteries (N)	Milk (N)	Bottled water (N)	All (N)
<b>Panel A: Household sample, All counties</b>					
HH-UPC-Year-Day observations	89739	38063	20366	122509	270677
Complete observations for price and quantity	89524	38061	19831	122086	269502
-----Year: 2014	22733	9507	4789	28237	65266
-----Year: 2015	24367	10187	5817	32782	73153
-----Year: 2016	23611	9950	5081	32385	71027
-----Year: 2017	18813	8417	4144	28682	60056
Complete observations for Harvey: 17 Aug 2017 - 1 Sep 2017	826	527	160	1588	3101
-----Warning for Hurricane Harvey	182	250	47	512	991
-----No warning for Hurricane Harvey	644	277	113	1076	2110
<b>Panel B: Household sample, Coastal counties</b>					
HH-UPC-Year-Day observations	26146	11262	6000	36642	80050
Complete observations for price and quantity	26077	11262	5760	36471	79570
-----Year: 2014	6711	2744	1374	7925	18754
-----Year: 2015	7110	2980	1814	9871	21775
-----Year: 2016	7037	3051	1427	10127	21642
-----Year: 2017	5219	2487	1145	8548	17399
Complete observations for Harvey: 17 Aug 2017 - 1 Sep 2017	203	224	38	547	1012
-----Warning for Hurricane Harvey	108	176	26	352	662
-----No warning for Hurricane Harvey	95	48	12	195	350
<b>Panel C: Store sample, All counties</b>					
Store-UPC-Year-Week observations	83428	37250	17944	106946	245568
Complete observations for price and quantity	83229	37248	17564	106590	244631
-----Year: 2014	21083	9325	4237	24714	59359
-----Year: 2015	22604	9960	4956	28678	66198
-----Year: 2016	21911	9715	4567	28086	64279
-----Year: 2017	17631	8248	3804	25112	54795
Complete observations for Harvey: 17 Aug 2017 - 1 Sep 2017	792	518	157	1417	2884
-----Warning for Hurricane Harvey	180	243	47	478	948
-----No warning for Hurricane Harvey	612	275	110	939	1936
<b>Panel D: Store sample, Coastal counties</b>					
Store-UPC-Year-Week observations	24291	11069	5230	31701	72291
Complete observations for price and quantity	24226	11069	5080	31559	71934
-----Year: 2014	6248	2694	1251	6893	17086
-----Year: 2015	6568	2936	1482	8453	19439
-----Year: 2016	6532	2989	1284	8746	19551
-----Year: 2017	4878	2450	1063	7467	15858
Complete observations for Harvey: 17 Aug 2017 - 1 Sep 2017	197	218	38	486	939
-----Warning for Hurricane Harvey	107	171	26	331	635
-----No warning for Hurricane Harvey	90	47	12	155	304

*Notes:* This table shows the distribution of the household and store samples by product. Observation level is household x UPC x year x day in Panel A (all counties of the state of Texas) and Panel B (coastal counties of the state of Texas). Observation level is store zipcode3 x UPC x year x week in Panel C (all counties of the state of Texas) and Panel D (coastal counties of the state of Texas). Observations cover all 52 weeks in years 2014 to 2017. Store zipcode3 is the 3-digit zipcode that localizes the store. Nielsen protects the confidentiality of individual stores by providing only the first 3 digits of the full zip code. Store zipcode3 is also provided only for a subset of purchase trips. Since Nielsen does not also give an exact store location (it is inferred from the closest store to the zip code of the consumer home), we replace any missing store zipcode3 by the consumer home zipcode3. UPC refers to Universal Product Code. Data source for hurricane warning is NOAA/NCEI, supplemented by NOAA way back machine data and Google search with date range customized to Harvey season.

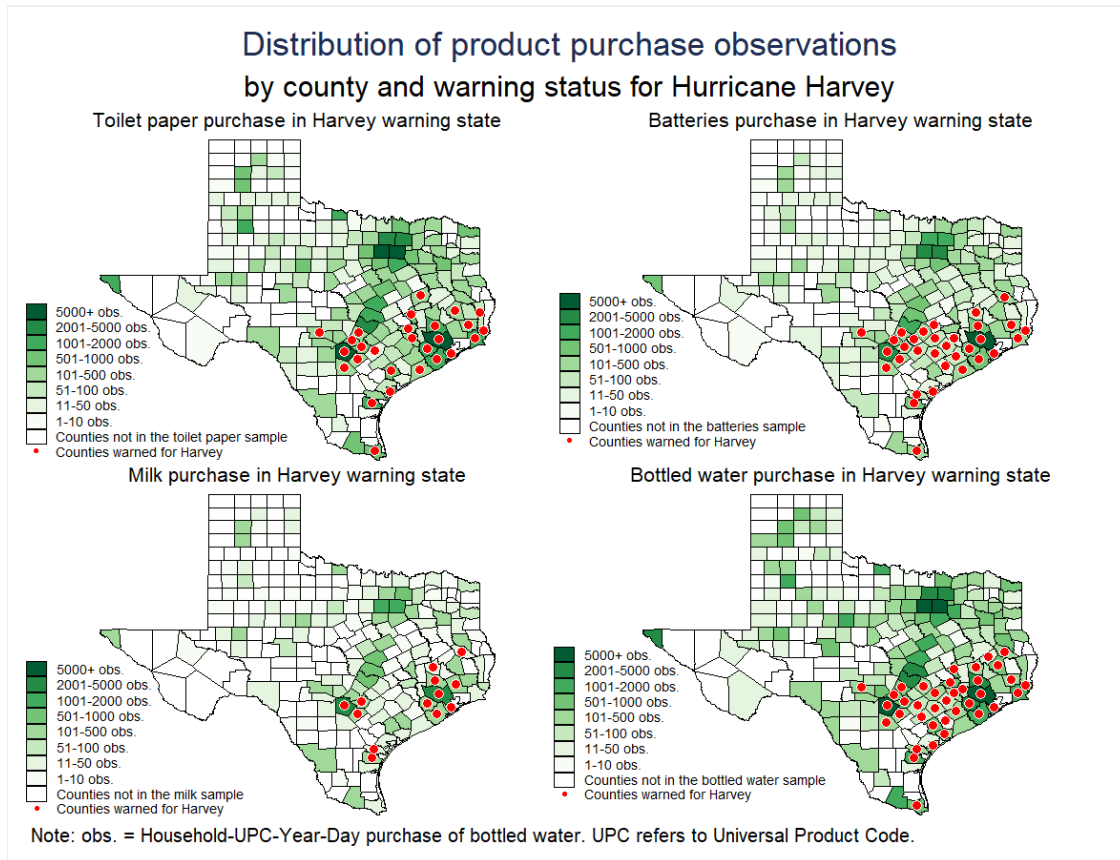


Figure 2.4: Map for product purchase

*Notes:* This map presents how the household-UPC-year-day observations for product purchase are distributed across counties warned and those not warned for Hurricane Harvey in the state of Texas. Products are toilet paper, batteries, milk, and bottled water. Harvey season runs from August 17th through September 1st in the year 2017. Counties referenced as “not in a product sample” are those unobserved in the purchase data for that product. Counties that are in a product sample but have no purchase observation in Harvey season could not be pointed as warned or not warned for Harvey. Data source for hurricane warning is NOAA/NCEI, supplemented by NOAA way back machine data and Google search with date range customized to Harvey season.

We average household-UPC-day prices across households in the store-week to obtain prices in the store sample, and we sum household-UPC-day quantities across households in the store-week to obtain quantities in the store sample. While across Harvey warning groups, there is very little variation in total quantities purchased for all products (Figure A.1 through Figure A.4), this is not the case for prices as observed graphically from the distributions of product prices in Hurricane Harvey season – covering weeks 33 through 35 in the third quarter of year 2017. To reduce the effect of extreme values, we winsorize price index at the 2nd and 98th percentiles. Since the distribution of price index is symmetric, winsorizing maintains an unbiased estimate of the mean. Moreover, as we will see later in the assessment of pre-hurricane price elasticity of demand in areas most affected by the Harvey, the winsorization helps to

enable price gouging testing for toilet paper and batteries. Summary statistics of original and winsorized variables in Table A.2 indicate why winsorization at the 2nd and 98th percentiles would be more effective than at the 1st and 99th percentiles.

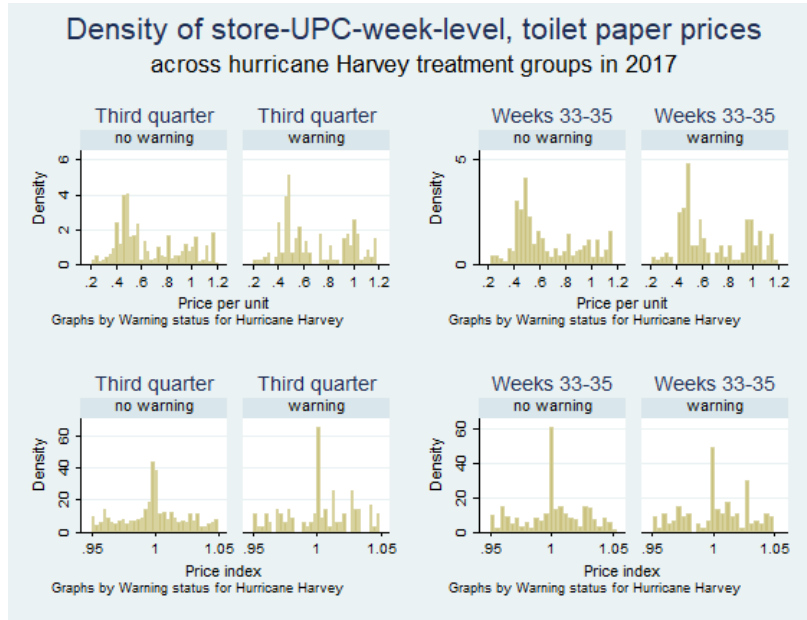


Figure 2.5: Density of toilet paper prices by warning status in the Harvey season

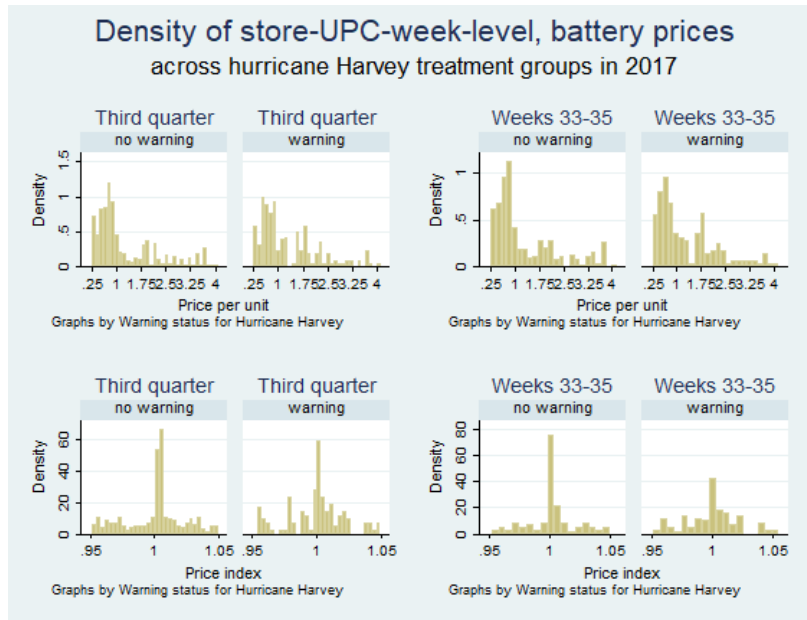


Figure 2.6: Density of battery prices by warning status in the Harvey season

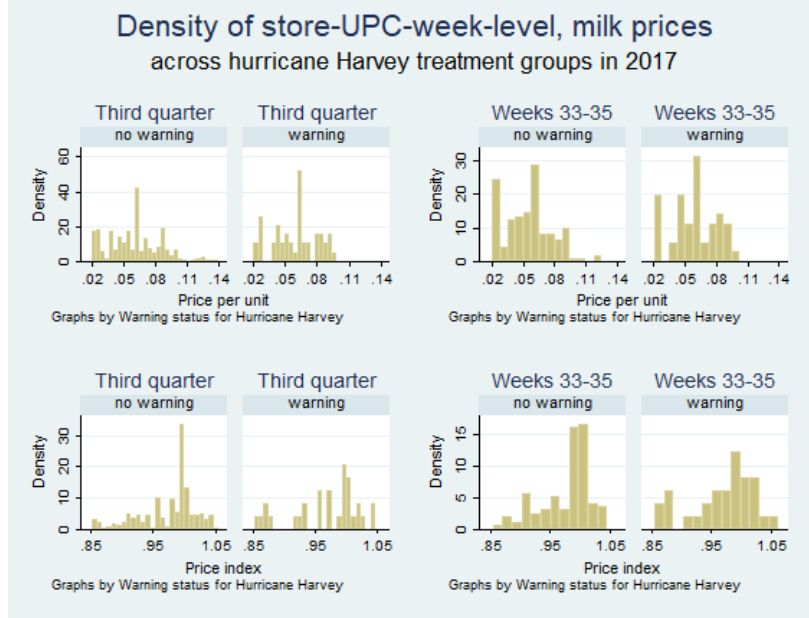


Figure 2.7: Density of milk prices by warning status in the Harvey season

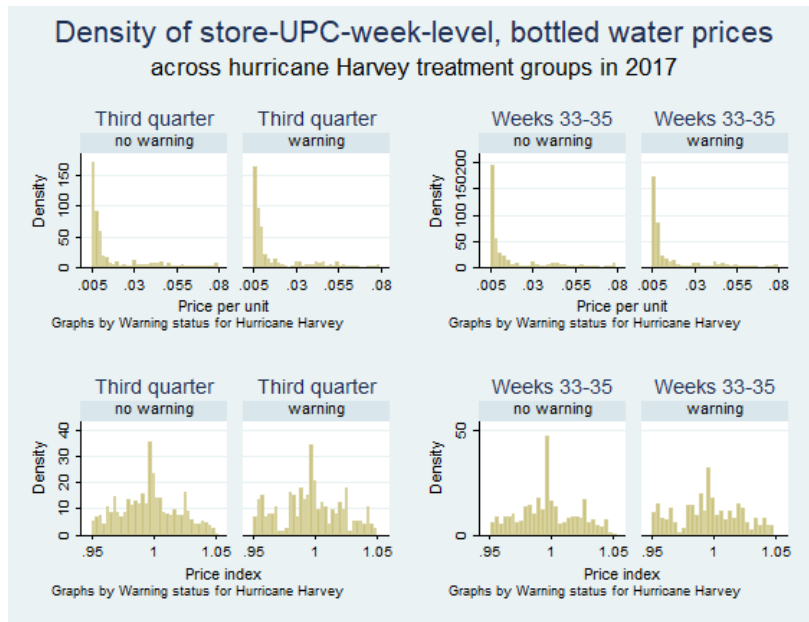


Figure 2.8: Density of bottled water prices by warning status in the Harvey season

## 2.4.2 Estimation methods

### Step 1: Checking pre-catastrophe demand is elastic

Given that the price efficiency testing strategy relies upon an optimal pricing obtained from the price elasticity of demand, it is important for the empirical application to verify that the demand is elastic. As shown in Eq.2.3, one critical assumption of the efficiency condition is that, at the starting period  $t = 0$ , the elasticity parameter is not equal to zero.

For each product  $j$ , we estimate the price elasticity of demand,  $\eta_{sj}$ , using a semi-log demand functional form with year and week fixed effects (Eq.2.4):

$$\log(Q_{rsjyw}) = \mu + \vartheta_{sj} P_{rsjyw}^{index} + \alpha_y + \gamma_w + \varepsilon_{rsjyw}. \quad (\text{Eq.2.4})$$

The estimation sample comprises observations for pre-catastrophe years (2014-2015) across all counties in the state of Texas. Using Eq.2.4, the elasticity coefficient is  $\eta_{sj} = \vartheta_{sj} * \overline{P_{rsjyw}^{index}}$  evaluated at the estimation sample mean. We conduct two tests: (a) the test of the significance of the elasticity, which assesses if the product demand is perfectly inelastic, *i.e.*  $\eta_{sj} = 0$ ; (b) the test of unitary elasticity, which assesses if the the product demand is unit elastic, *i.e.*  $\eta_{sj} = -1$ . We expect  $-1 < \eta_{sj} < 0$  for the necessity and emergency goods studied in this empirical application.

### Step 2: Assessing changes in price elasticity of demand due to Hurricane Harvey

This step establishes the expected or efficient price changes. Because in a catastrophe year, price changes can occur at multiple critical times, we are interested in the efficient price changes (a) from the pre-hurricane period ( $t = 0$ ) to the period of warning for the hurricane ( $t = 2$ ), (b) from the pre-hurricane period ( $t = 0$ ) to the period of landfall ( $t = 3$ ), and (c) from the pre-hurricane period ( $t = 0$ ) to a short-term period after the landfall ( $t = 4$ ) which we set to three weeks following the landfall.<sup>15</sup> The timing

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<sup>15</sup>Assessing medium- to long-term changes in demand is not possible in this study because we only analyze demand. We believe that from the fourth week following the landfall ( $t = 5$ ), damages inflicted by the hurricane start to get controlled in the affected localities, thus allowing for more supplies to get delivered to these localities.

is illustrated in Figure 2.9. Pre-warning period ( $t = 1$ ) is not a pure base, hence not used as a baseline for the assessment: its observations include those of year 2016 where there were Hurricanes Hermine and Matthew, and those of year 2017 where Harvey was not imminent but alert stores might manipulate prices before disaster declaration as hurricane season approaches to raise the pre-disaster price levels. Using demand and prices in the pre-hurricane period ( $t = 0$ ) as a baseline for the assessment offers an appropriate starting point for the comparisons (a), (b), and (c).

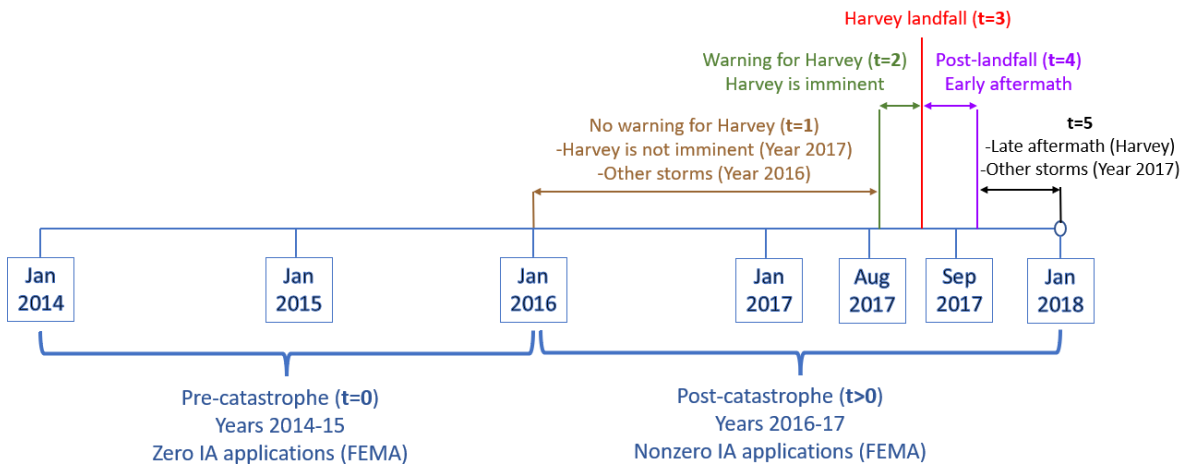


Figure 2.9: Time rollout

For each product  $j$ , we use the observations for all years (2014-2017) across all counties in state of Texas to estimate a semi-log demand equation with store-fixed effects (Eq.2.5) that is flexible enough to account for changes in demand (a) due to the warning for Hurricane Harvey, (b) during the landfall, and (c) in the shorter aftermath of the strike (1-3 weeks post-landfall). We use store-fixed effects because price gouging practice is potentially correlated with seller's characteristics (Cabral & Xu, 2021), while we do not use data on store managers. In Eq.2.5, we replace year-fixed effects with an indicator variable for post-catastrophe years,  $Post$ , which dissociates demand in pre-catastrophe years (2014-2015) from post-catastrophe years (2016-2017) by allowing for a change in demand intercept between  $t = 0$  and  $t > 0$ . We do not use week fixed effects because we are estimating price differences between hurricane-free weeks and hurricane weeks. We also control for a vector of confounders that include applications to assistance for housing, shelter, special accommodations, and emergency items and food (in percent of population) as well as the

indicators for the periods of past hurricanes (Hermine and Matthew) in 2016 and future hurricanes (Irma and Maria) in 2017. As they potentially affect demand for the goods studied, applications to assistance for housing, shelter, special accommodations, and emergency items and food, if omitted in Eq.2.5, could bias the estimates of price elasticity of demand. Because the changes in price elasticity of demand are obtained using the full sample, we control for the periods of past hurricanes in 2016 so that prices and quantities in the past hurricane periods do not contaminate the price elasticity of demand before the warning for Harvey. We also control for the periods of future hurricanes in 2017 so that prices and quantities in these periods do not contaminate the price elasticity of demand in the medium-to-long aftermath of Hurricane Harvey.

$$\begin{aligned}
\log(Q_{rsjyw}) = & \mu + \vartheta_{sj}^{pre} P_{rsjyw}^{index} + \alpha_{sj} Post_y + \beta_{sj}^{post} (P_{rsjyw}^{index} \times Post_y) \\
& + \beta_{sj}^{\varpi} (P_{rsjyw}^{index} \times Warning_{rsyw}) + \beta_{sj}^h (P_{rsjyw}^{index} \times HarveyLandfall_{rsyw}) \\
& + \beta_{sj}^{\S} (P_{rsjyw}^{index} \times AfterStrike_{rsyw}) + Confounders'_{rsyw} \Psi + \gamma_s + \varepsilon_{rsjyw}. \quad (Eq.2.5)
\end{aligned}$$

In the specification in Eq.2.5,  $\beta_{sj}^{post}$  captures the change in elasticity parameter between the pre-catastrophe period and all post-catastrophe periods not of interest (conflating  $t = 1$  and  $t = 5$  after removing the effect of confounding periods);  $\beta_{sj}^{\varpi}$  captures the change in elasticity parameter between the post-catastrophe periods not of interest and the period of warning for Hurricane Harvey in counties warned. Similarly,  $\beta_{sj}^h$  captures the change in elasticity parameter between the post-catastrophe periods not of interest and the period of Hurricane Harvey landfall.  $\beta_{sj}^{\S}$  captures the change in elasticity parameter between the post-catastrophe periods not of interest and the first three weeks post-Harvey landfall. The coefficients of interest are: the parameter of price elasticity of demand in pre-catastrophe period ( $\vartheta_{sj}^{pre}$ ), the change in elasticity parameter due to warning ( $\beta_{sj}^{\varpi} + \beta_{sj}^{post}$ ), the change in elasticity parameter during the landfall ( $\beta_{sj}^h + \beta_{sj}^{post}$ ), and the short-term change in elasticity parameter after the landfall ( $\beta_{sj}^{\S} + \beta_{sj}^{post}$ ). We conduct the test of significance of the price elasticity of demand in pre-catastrophe years ( $\vartheta_{sj}^{pre} * \overline{P_{rsjyw}^{index}}$ ) and the test of significance of the changes in elasticity parameter at the times of interest. Following Eq.2.3, the expected price changes due to warning are  $\lambda_{sj}^{\varpi} = \frac{\beta_{sj}^{\varpi} + \beta_{sj}^{post}}{(\vartheta_{sj}^{pre})^2}$ , those during the landfall are  $\lambda_{sj}^h = \frac{\beta_{sj}^h + \beta_{sj}^{post}}{(\vartheta_{sj}^{pre})^2}$ ,

and those after the landfall are  $\lambda_{sj}^{\S} = \frac{\beta_{sj}^{\S} + \beta_{sj}^{post}}{(\theta_{sj}^{pre})^2}$ . As described in Section 2.3.3 (*Case 2*), if we do not reject that the change in elasticity parameter at a given period is zero, then the expected price change at that period is zero.

### Step 3: Assessing changes in prices due to Hurricane Harvey

The final step of the price efficiency testing strategy compares observed price differences and efficient price changes. For each product  $j$ , we regress price index on the indicator variable for the post-catastrophe periods not of interest (*Post*), warning treatment, week of landfall, and shorter aftermath of the strike (1-3 weeks post-landfall), and the same vector of confounders described above with store-fixed effects (Eq.2.6).

$$P_{rsjyw}^{index} = \nu + \ell_{sj}Post_y + \varpi_{sj}Warning_{rsyw} + \hbar_{sj}HarveyLandfall_{rsyw} + \S_{sj}AfterStrike_{rsyw} + Confounders'_{rsyw} \Upsilon + \gamma_s + \epsilon_{rsjyw}. \quad (\text{Eq.2.6})$$

In this specification, the constant term  $\nu$  represents the average price in pre-hurricane period, which should converge to one by index construction;  $\ell_{sj}$  represents the price differential between pre-hurricane period and the conflated periods not of interest (after removing the effect of confounding periods). The parameters  $\varpi_{sj}$ ,  $\hbar_{sj}$ ,  $\S_{sj}$  capture the price differentials between the the conflated periods not of interest and the period of warning, landfall, and immediate aftermath, respectively. The coefficients of interest are the observed changes in prices due to warning ( $\varpi_{sj} + \ell_{sj}$ ), those during the landfall ( $\hbar_{sj} + \ell_{sj}$ ), and those after the landfall ( $\S_{sj} + \ell_{sj}$ ). We conduct the efficiency test:  $\lambda_{sj}^T = T + \ell_{sj}$ , for  $T = \varpi_{sj}, \hbar_{sj}, \S_{sj}$ .

## 2.5 Results

### 2.5.1 Demand in pre-catastrophe years (2014-2015)

Table 2.2 presents the results from Step 1 of the price efficiency testing strategy using all counties (Table 2.2a) and coastal counties (Table 2.2b). In Table 2.2a, we find that the demand for each product studied is elastic

in pre-catastrophe years (2014-2015) in the state of Texas. The parameter of price elasticity of demand is significant at the 1% level for milk and bottled water, 5% level for toilet paper, and 10% level for batteries (Table 2.2a, columns one through four). Using the winsorized price index, the parameter of price elasticity of demand for batteries becomes significant at the 1% level (Table 2.2a, column six). As expected for necessity and emergency goods, the elasticity coefficient initially falls within the range  $(-1, 0)$ .

In Table 2.2b, we find a more elastic demand in coastal counties, in columns five through eight where we use the winsorized price index. It is important to check that demand pre-catastrophe is elastic in coastal counties. As noted by NOAA, the incidence of ocean-related hazards such as hurricanes is localized in coastal areas<sup>16</sup>, while the coverage of the potential impact of catastrophes such as pandemics would generalize to all counties. Therefore, in the results that follow, we rely on the changes in demand and prices occurring in coastal counties. Changes in demand and prices across all counties in the state of Texas are presented in Appendix and discussed when they differ from the core insights in hurricane-stricken areas.

While the demand is elastic pre-disaster for all products, it becomes flat in the short-term hurricane period for all products. In Table A.3 Panel A (across all counties), we find an insignificant elasticity coefficient in Hurricane Harvey period for all products at the 5% level. We also find no change in demand intercept in Hurricane Harvey period for all products, except bottled water for which the weekly store-UPC-level quantity purchased drops by half an ounce of water at the 5% level (column eight). The estimations across coastal counties in Panel B emphasize that, in Hurricane Harvey period, the demand is not responsive to prices and there is no shift in demand for any studied product. This preliminary evidence sets the stage to investigate whether the result holds for the specific periods of interest (at warning, at landfall, and 1-3 weeks post-landfall).

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<sup>16</sup>“The U.S. population most directly affected by the coast resides in a standard suite of Coastal Shoreline Counties that are directly adjacent to the open ocean, major estuaries, and the Great Lakes, which due to their proximity to these waters, bear a great proportion of the full range of effects from coastal hazards and host the majority of economic production associated with coastal and ocean resources. (p. 2).” <https://coast.noaa.gov/data/digitalcoast/pdf/qrt-coastal-county-definitions.pdf>.

Table 2.2: Demand estimations in pre-catastrophe years (2014-2015)

(a) Panel A. All counties								
VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Price index ( $\vartheta$ )	-0.100** (0.049)	-0.120* (0.070)	-0.490*** (0.111)	-0.462*** (0.063)	-0.122** (0.054)	-0.280*** (0.085)	-0.605*** (0.129)	-0.660*** (0.086)
Observations	43,687	19,285	9,193	53,392	43,687	19,285	9,193	53,392
R-squared	0.004	0.013	0.011	0.006	0.004	0.013	0.011	0.006
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Year and Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Elasticity coefficient=	-0.100	-0.120	-0.483	-0.459	-0.121	-0.278	-0.596	-0.653
Testing elasticity coefficient=0, p-value=	0.042	0.088	0.000	0.000	0.025	0.001	0.000	0.000
Testing elasticity coefficient=-1, p-value=	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.000
(b) Panel B. Coastal counties								
VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Price index ( $\vartheta$ )	-0.099 (0.097)	-0.171 (0.120)	-0.681*** (0.186)	-0.647*** (0.108)	-0.206** (0.101)	-0.418*** (0.151)	-0.790*** (0.225)	-0.946*** (0.152)
Observations	12,816	5,630	2,733	15,346	12,816	5,630	2,733	15,346
R-squared	0.008	0.024	0.055	0.013	0.008	0.025	0.054	0.014
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Year and Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Elasticity coefficient=	-0.098	-0.172	-0.666	-0.641	-0.203	-0.418	-0.772	-0.934
Testing elasticity coefficient=0, p-value=	0.309	0.156	0.000	0.000	0.043	0.006	0.000	0.000
Testing elasticity coefficient=-1, p-value=	0.000	0.000	0.066	0.001	0.000	0.000	0.299	0.659

*Notes:* This table reports the results from demand estimations in non-warning years using a semi-log functional form (Eq.2.4). The estimation sample includes all counties in the state of Texas in Panel A, and coastal counties in the state of Texas in Panel B. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. Dependent variable is log store-UPC-level total quantity purchased. Using Eq.2.4, the elasticity coefficient is  $\eta = \vartheta * \bar{P}$  evaluated at the estimation sample mean. The product is toilet paper (TP) in columns 1 and 5, batteries (BT) in columns 2 and 6, milk (MK) in columns 3 and 7, and bottled water (BW) in columns 4 and 8. In columns 5 through 8, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Year and week fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

### 2.5.2 Changes in price elasticity of demand due to Hurricane Harvey

The efficiency rule at warning, landfall, and after strike is provided in Step 2 of the price efficiency testing strategy for each of the four products studied. Table 2.3 shows  $\lambda^{\varpi} = \lambda^h = \lambda^s = 0$  for toilet paper (column one) and batteries (column two) in coastal counties, as the change in elasticity parameter from pre-hurricane period to hurricane warning, or landfall, or after strike period, is insignificant. Columns one, two, five, and six of Table A.4 also give the same value for these parameters, if it is considered that the effect of Hurricane Harvey is extended to all counties of the state. Based on these results, as suggested by Section 2.3.3 (*Case 2*), any increase in observed prices for toilet paper and batteries at warning, or landfall, or after strike, deviates from efficiency.

For milk, post-estimation test results in column three of Table 2.3 indicate a 0.759-unit rise in the elasticity parameter from pre-hurricane period to the week of landfall, which is significant at the 5% level and suggests that milk prices could soar by up to 85.9% during Harvey landfall, while remaining efficient in coastal counties. Similar results are found in Table A.4, with a 0.514-unit to 0.529-unit rise in the elasticity parameter at landfall, leading to allowable increases in milk prices by up to 101.1% (column seven) or 154.5% (column three) across all counties. However, in the other periods examined (warning and after landfall), milk prices are expected to stay at their pre-hurricane level in coastal counties, and even in the entire state.

For bottled water, in coastal counties, demand-driven changes indicate that prices should remain the same until landfall where they can rise by up to 79.2%, and in the three weeks following landfall where they can rise by up to 127.4%, relative to prices in years 2014 and 2015 (Table 2.3, column four). Ignoring the focus on coastal counties, however, post-estimation test results in columns four and eight of Table A.4 illustrate *Case 1* of the implications for price efficiency (Section 2.3.3) in all periods examined. Across all counties, efficiency rules that relative to the pre-hurricane level, bottled water prices are expected to rise by up to 1.375 times (at warning for Harvey), 2.831 times (during Harvey landfall), and 3.847 times (after the strike), in column four. The conservative estimates in column eight indicate  $\lambda^{\varpi} = 0$ ,  $\lambda^h = 1.316$ , and  $\lambda^s = 1.817$  for efficient bottled water proportional price increases in the state.

Table 2.3: Demand changes due to warning for Harvey, during landfall, and after strike - Coastal counties in the state of Texas

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW
Price index ( $\partial^{pre}$ )	-0.208** (0.101)	-0.478*** (0.151)	-0.940*** (0.235)	-0.954*** (0.153)
Post-catastrophe year ( $\alpha$ )	-0.053 (0.135)	0.026 (0.213)	-0.402 (0.288)	0.038 (0.216)
Price index x Post ( $\beta^{post}$ )	-0.043 (0.135)	0.017 (0.211)	0.339 (0.290)	-0.052 (0.216)
Price index x Warning for actual Harvey ( $\beta^{\omega}$ )	-0.068 (0.109)	-0.087 (0.134)	-0.344** (0.164)	0.366*** (0.133)
Price index x During actual Harvey landfall ( $\beta^h$ )	0.161 (0.231)	0.061 (0.164)	0.420 (0.259)	0.773*** (0.209)
Price index x After actual Harvey strike (1-3 weeks) ( $\beta^s$ )	-0.098 (0.547)	-0.496 (0.456)	-0.558*** (0.145)	1.212*** (0.181)
Applications to Housing Programs (IHP/HA) (%)	0.073 (0.046)	0.004 (0.063)	0.076 (0.084)	-0.104* (0.059)
Applications reporting a need for shelter (%)	-0.151** (0.076)	0.025 (0.084)	-0.176 (0.239)	0.118* (0.066)
Applications reporting a need for special accommodations (%)	0.692 (0.753)	-0.957 (0.835)	-1.188 (1.596)	-0.668 (0.804)
Applications reporting a need for emergency items or food (%)	-0.074 (0.067)	0.043 (0.096)	0.033 (0.086)	0.115 (0.093)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	0.136* (0.073)	0.011 (0.163)	0.139 (0.151)	0.378*** (0.131)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.140** (0.058)	0.012 (0.132)	0.052 (0.126)	0.169 (0.110)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	-0.065 (0.089)	0.085 (0.123)	-0.085 (0.138)	-0.143 (0.107)
Hurricane Maria period (16 - 30 Sep 2017)	0.090 (0.068)	0.077 (0.113)	0.123 (0.087)	0.093 (0.099)
Observations	24,148	11,041	5,023	31,417
R-squared	0.015	0.007	0.041	0.015
Counties	Coastal	Coastal	Coastal	Coastal
Price index winsorized at the 2nd and 98th percentiles	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes
Elasticity in pre-catastrophe=	-0.207	-0.477	-0.924	-0.947
Testing elasticity in pre-catastrophe=0, p-value=	0.039	0.002	0.000	0.000
Change in elasticity parameter at warning ( $\beta^{post} + \beta^{\omega}$ )=	-0.111	-0.070	-0.005	0.314
Testing ( $\beta^{post} + \beta^{\omega}$ ) = 0, p-value=	0.519	0.775	0.987	0.211
Efficient price change at warning ( $\lambda^{\omega}$ )=	0.000	0.000	0.000	0.000
Change in elasticity parameter at landfall ( $\beta^{post} + \beta^h$ )=	0.118	0.077	0.759	0.721
Testing ( $\beta^{post} + \beta^h$ ) = 0, p-value=	0.659	0.768	0.049	0.017
Efficient price change at landfall ( $\lambda^h$ )=	0.000	0.000	0.859	0.792
Change in elasticity parameter after strike ( $\beta^{post} + \beta^s$ )=	-0.141	-0.480	-0.219	1.160
Testing ( $\beta^{post} + \beta^s$ ) = 0, p-value=	0.803	0.335	0.493	0.000
Efficient change after strike ( $\lambda^s$ )=	0.000	0.000	0.000	1.274

Notes: This table reports the results from demand estimations using a flexible, semi-log functional form (Eq.2.5). The estimation sample covers all four years (2014-2017) and includes coastal counties in the state of Texas. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. Dependent variable is log store-UPC-level total quantity purchased. Using Eq.2.5, the elasticity coefficient in pre-catastrophe years is  $\eta = (\partial^{pre}) * \bar{P}$  evaluated at the estimation sample mean. Efficient price changes are set to zero if the change in elasticity parameter is not significant at the 5% level. The product is toilet paper (TP) in column 1, batteries (BT) in column 2, milk (MK) in column 3, and bottled water (BW) in column 4. Price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

### 2.5.3 Changes in prices due to Hurricane Harvey

Now, we proceed to Step 3 of the price efficiency testing strategy, where we compare the observed price changes to the efficiency benchmark established in Table 2.3 and Table A.4.

For toilet paper, we find that during Harvey landfall, observed prices go up by 12% in coastal counties (Table 2.4, column one) and 3.3% to 3.6% across all counties in the state (Table A.5, columns one and five), which is inefficient because toilet paper price is expected to be constant during Harvey landfall ( $\lambda^h = 0$ ). At warning and in the aftermath of the strike, we do not reject the hypothesis that observed price differences are those expected by the efficiency condition.

While we find that batteries and toilet paper follow the same pattern of price increases generally across all counties in the state (Table A.5, columns two and six), battery prices respond to Hurricane Harvey in coastal counties not only during Harvey landfall but also in the aftermath of the strike. We find that observed battery prices go up by 5.6% and 9.8% during Harvey landfall and in the aftermath of the strike, respectively (Table 2.4, column two), which is inefficient because battery prices are not expected to rise above their pre-hurricane levels ( $\lambda^h = \lambda^s = 0$ ). At warning, we observe an increase in battery prices by 0.5%, which is not significantly different from the expected price change, and thus, efficient.

For milk, we observe a 4.9% increase in prices at warning and a 7.3% increase in prices after strike in coastal counties (Table 2.4, column three). However, these price differences are not statistically significant at the 5% level to conclude that there is inefficient price increase at warning and after strike. During Harvey landfall, where we expect up to a 85.9% increase in milk prices in coastal counties, we rather observe an 8.4% increase, which falls within the range of efficient price rises. Estimation results using all counties yield a different conclusion because observed milk prices drop by 1.8% while it is expected an increase in milk prices during Harvey landfall (Table A.5, columns three and seven).

For bottled water, while estimation results using all counties suggest that the observed price changes are within the range of efficient price increases (Table A.5, column four), results in Harvey-stricken counties suggest otherwise in all periods examined. In coastal counties, we observe an increase in prices by 2.4% at

warning, followed by a decrease in prices by 1.3% during landfall and by 0.5% immediately after the strike (Table 2.4, column four), which fall modestly outside the efficient ranges expected.

Table 2.4: Assessment of price changes due to warning for Harvey, during landfall, and after strike - Coastal counties in the state of Texas

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW
Post-catastrophe year ( $\ell$ )	0.012*** (0.001)	-0.005* (0.003)	0.012*** (0.003)	0.012*** (0.001)
Warning for actual Harvey ( $\varpi$ )	-0.010 (0.013)	0.010 (0.013)	0.037 (0.029)	0.011 (0.012)
During actual Harvey landfall ( $h$ )	0.108*** (0.031)	0.061** (0.028)	0.073 (0.072)	-0.025 (0.015)
After actual Harvey strike (1-3 weeks) ( $\xi$ )	-0.010 (0.020)	0.103** (0.041)	0.062 (0.045)	-0.018 (0.018)
Applications to Housing Programs (IHP/HA) (%)	-0.001 (0.005)	-0.001 (0.006)	-0.025 (0.016)	0.006 (0.004)
Applications reporting a need for shelter (%)	-0.011 (0.008)	0.003 (0.009)	0.065*** (0.023)	-0.004 (0.004)
Applications reporting a need for special accommodations (%)	0.020 (0.083)	-0.054 (0.094)	-0.235 (0.274)	0.161*** (0.047)
Applications reporting a need for emergency items or food (%)	0.003 (0.006)	0.002 (0.009)	0.021 (0.020)	-0.015*** (0.005)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.007 (0.010)	-0.009 (0.018)	-0.016 (0.021)	-0.006 (0.007)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.008 (0.010)	0.007 (0.017)	-0.003 (0.019)	0.001 (0.006)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.010 (0.010)	-0.000 (0.011)	-0.051* (0.028)	0.017* (0.009)
Hurricane Maria period (16 - 30 Sep 2017)	-0.004 (0.007)	0.001 (0.015)	0.050** (0.022)	0.005 (0.006)
Constant	0.988*** (0.001)	1.005*** (0.002)	0.982*** (0.002)	0.985*** (0.001)
Observations	24,148	11,041	5,023	31,417
R-squared	0.008	0.006	0.037	0.009
Counties	Coastal	Coastal	Coastal	Coastal
Price index winsorized at the 2nd and 98th percentiles	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes
Efficient price change at warning ( $\lambda^{\varpi}$ )=	0.000	0.000	0.000	0.000
Observed price change at warning ( $\varpi + \ell$ )=	0.002	0.005	0.049	0.024
Testing observed price change at warning ( $\varpi + \ell$ ) = $\lambda^{\varpi}$ , p-value=	0.848	0.705	0.091	0.046
Efficient price change at landfall ( $\lambda^h$ )=	0.000	0.000	0.859	0.792
Observed price change at landfall ( $h + \ell$ )=	0.120	0.056	0.084	-0.013
Testing observed price change at landfall ( $h + \ell$ ) = $\lambda^h$ , p-value=	0.000	0.043	0.000	0.000
Efficient change after strike ( $\lambda^{\xi}$ )=	0.000	0.000	0.000	1.274
Observed price change after strike ( $\xi + \ell$ )=	0.003	0.098	0.073	-0.005
Testing observed price change after strike ( $\xi + \ell$ ) = $\lambda^{\xi}$ , p-value=	0.898	0.017	0.104	0.000

Notes: This table reports the results from price estimations using Eq.2.6. The estimation sample covers all four years (2014-2017) and includes coastal counties in the state of Texas. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. Dependent variable is store-UPC-level price index winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. The product is toilet paper (TP) in column 1, batteries (BT) in column 2, milk (MK) in column 3, and bottled water (BW) in column 4. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Observing relatively, slightly lower or higher prices in the wake of a hurricane for certain products in coastal counties is not uncommon in the literature. Beatty et al., 2021 study hurricane seasons in years 2004 through 2008 in the U.S. and find a modest decline in post-landfall gasoline price margins, concluding that price gouging following disasters is not as usual as it is claimed to be. Studying hurricanes and snowstorms in the U.S., Gagnon and López-Salido, 2020 also find no apparent price response.

## 2.6 Robustness checks

### 2.6.1 Instrumental variables estimations

Because we assess price response to hurricanes using changes in price elasticity of demand, concerns about price endogeneity in the demand estimations will question the robustness of the evidence. Although we avoid a simultaneity bias in Eq.2.5 as our coefficients of interest relate to short-term demand dynamics where supply is constant, two potential sources of price endogeneity remain. First, omitted variables bias is possible due to unobserved characteristics, such as household's price search determinants, quantity discounting, and product quality. Households employ a price search process that is spatially-specific and cost-effective (Gauri et al., 2008), which may lead to endogenous prices if the price search process is correlated with household's demand preferences. Poor households also shop more on products presenting quantity discounts (Beatty, 2010), which makes household's wealth correlated with prices paid, and thus, prices endogenous with demand. There is also the standard problem of unit value bias requiring to remove product quality component from prices in demand analysis (Cox & Wohlgenant, 1986; Crawford et al., 2003; Deaton, 1988). Second, measurement errors in prices are likely to be present in hurricane-affected areas during hurricane seasons. Power outages during hurricane seasons may contribute to the mis-reporting of household's purchases, and hurricane warnings may make purchase scanning not having priority over survival strategies of hurricane-affected households (Beatty et al., 2019).

To address price endogeneity issues, we use the instrumental variables method. For each household, following Hausman-Nevo instrument strategy (Hausman, 1996; Nevo, 2001), we instrument for  $P_{rsjyw}^{index}$

using the average price index of the same product  $j$  at the same retailer chain  $r$ , in the same year  $y$ , but outside of the household's Nielsen Designated Market Area (DMA). Next, in cases where instruments are missing—because the chain operates in a single DMA in the given year—we apply the average price index of the same product  $j$  at the same retailer chain  $r$ , in the same year  $y$ , but outside of the state. Next, in cases where instruments are missing—because the chain operates in a single state in the given year—we apply the average price index of the same product  $j$  at the same retailer chain  $r$ , in the same year  $y$ , but outside of the household's county. Next, in cases where instruments are missing—because the chain operates in a single county in the given year—we apply the average price index of the same product  $j$  at the same retailer chain  $r$ , in the same year  $y$ , but outside of the household's zip-code<sup>3</sup>. If the chain operates in a single zip-code<sup>3</sup> in the given year, then we are not able to compute the instrument for those values of price index because applying the average price index of neighboring retailer chains in the same year would violate the chain-level uniform pricing across stores reported in (DellaVigna & Gentzkow, 2019).

The iterative process of leave-market-out chain-level average price of the same UPC in the same year as an instrument makes it possible to compute price index instruments in the household-UPC-year-day dataset for 96% of toilet paper observations, 93% of battery observations, 95% of milk observations, and 93% of bottled water observations. We then average price index instruments across households to obtain the instruments in the store-UPC-year-week dataset for 96% of toilet paper observations, 93% of battery observations, 94% of milk observations, and 92% of bottled water observations. The instrument addresses price search concerns as it exploits variation in price index within surrounding geographic areas, which incorporates some supply-side cost. Moreover, by using Aguiar and Hurst, 2007's price index as in Beatty, 2010 and instrumenting for it, we account for quantity discounting concerns. We also deal with the unit value bias concerns because our demand analysis is at the UPC level. Furthermore, the extent of measurement errors is reduced in the instrument because about 70% of the sample locate in non-coastal counties, and most of these observations—after excluding those within the same market as a given observation—are used in the computation of the average price index as instrument.

We estimate Eq.2.5 using the instrumental variables strategy, with first-stage equations specified in Eq.2.7-Eq.2.11. There are five first-stage equations due to five potential endogenous variables:  $P_{rsjyw}^{index}$ ,  $(P_{rsjyw}^{index} \times Post_y)$ ,  $(P_{rsjyw}^{index} \times Warning_{rsyw})$ ,  $(P_{rsjyw}^{index} \times HarveyLandfall_{rsyw})$ , and  $(P_{rsjyw}^{index} \times AfterStrike_{rsyw})$  as dependent variables in Eq.2.7, Eq.2.8, Eq.2.9, Eq.2.10, and Eq.2.11, respectively. It is expected  $\delta^1 > 0$  in Eq.2.7,  $\delta^3 > 0$  in Eq.2.8,  $\delta^4 > 0$  in Eq.2.9,  $\delta^5 > 0$  in Eq.2.10, and  $\delta^6 > 0$  in Eq.2.11. We present the first-stage regression results for toilet paper, batteries, milk, and bottled water, respectively, in coastal counties (Table 2.5 through Table 2.8) and across all counties in the state of Texas (Table A.6 through Table A.9). Based on the F-test of excluded instruments in these tables, the instrument relevance condition is strongly satisfied—in a lesser extent for milk—suggesting that for a given store-UPC-week observation, the average price index in surrounding markets for the same retailer does influence the price index of the observation. We can also argue that for a given store-UPC-week observation, the average price index in surrounding markets for the same retailer can only affect demand through the price index of the given observation. While an alternative channel for the instrument to affect demand would be through supply changes, our analysis focuses on parameter estimates at a time of fixed supply. Nevertheless, we recognize that the validity of the exclusion restriction of spatial instruments remains subject to debate in the literature (Allcott et al., 2019; Betz et al., 2018; Sundquist, 2021).

$$\begin{bmatrix} P_{rsjyw}^{index} \\ (P_{rsjyw}^{index} \times Post_y) \\ (P_{rsjyw}^{index} \times Warning_{rsyw}) \\ (P_{rsjyw}^{index} \times HarveyLandfall_{rsyw}) \\ (P_{rsjyw}^{index} \times AfterStrike_{rsyw}) \end{bmatrix} = \begin{bmatrix} \zeta + \delta_{sj}^1 P_{rsjyw}^{instr} + \delta_{sj}^2 Post_y + \delta_{sj}^3 (P_{rsjyw}^{instr} \times Post_y) \\ \zeta + \delta_{sj}^1 P_{rsjyw}^{instr} + \delta_{sj}^2 Post_y + \delta_{sj}^3 (P_{rsjyw}^{instr} \times Post_y) \\ \zeta + \delta_{sj}^1 P_{rsjyw}^{instr} + \delta_{sj}^2 Post_y + \delta_{sj}^3 (P_{rsjyw}^{instr} \times Post_y) \\ \zeta + \delta_{sj}^1 P_{rsjyw}^{instr} + \delta_{sj}^2 Post_y + \delta_{sj}^3 (P_{rsjyw}^{instr} \times Post_y) \\ \zeta + \delta_{sj}^1 P_{rsjyw}^{instr} + \delta_{sj}^2 Post_y + \delta_{sj}^3 (P_{rsjyw}^{instr} \times Post_y) \end{bmatrix}$$

$$+ \begin{bmatrix} \delta_{sj}^4(P_{rsjyw}^{instr} \times Warning_{rsyw}) + \delta_{sj}^5(P_{rsjyw}^{instr} \times HarveyLandfall_{rsyw}) \\ \delta_{sj}^4(P_{rsjyw}^{instr} \times Warning_{rsyw}) + \delta_{sj}^5(P_{rsjyw}^{instr} \times HarveyLandfall_{rsyw}) \\ \delta_{sj}^4(P_{rsjyw}^{instr} \times Warning_{rsyw}) + \delta_{sj}^5(P_{rsjyw}^{instr} \times HarveyLandfall_{rsyw}) \\ \delta_{sj}^4(P_{rsjyw}^{instr} \times Warning_{rsyw}) + \delta_{sj}^5(P_{rsjyw}^{instr} \times HarveyLandfall_{rsyw}) \\ \delta_{sj}^4(P_{rsjyw}^{instr} \times Warning_{rsyw}) + \delta_{sj}^5(P_{rsjyw}^{instr} \times HarveyLandfall_{rsyw}) \end{bmatrix}$$

$$+ \begin{bmatrix} \delta_{sj}^6(P_{rsjyw}^{instr} \times AfterStrike_{rsyw}) + Confounders'_{rsyw} \Omega + \gamma_s + \xi_{rsjyw} \\ \delta_{sj}^6(P_{rsjyw}^{instr} \times AfterStrike_{rsyw}) + Confounders'_{rsyw} \Omega + \gamma_s + \xi_{rsjyw} \\ \delta_{sj}^6(P_{rsjyw}^{instr} \times AfterStrike_{rsyw}) + Confounders'_{rsyw} \Omega + \gamma_s + \xi_{rsjyw} \\ \delta_{sj}^6(P_{rsjyw}^{instr} \times AfterStrike_{rsyw}) + Confounders'_{rsyw} \Omega + \gamma_s + \xi_{rsjyw} \\ \delta_{sj}^6(P_{rsjyw}^{instr} \times AfterStrike_{rsyw}) + Confounders'_{rsyw} \Omega + \gamma_s + \xi_{rsjyw} \end{bmatrix} \cdot \quad (\text{Eq.2.7-Eq.2.II})$$

Table 2.5: First-stage regression results from IV demand estimations for toilet paper - Coastal counties in the state of Texas

VARIABLES	(1) TP (Eq.7)	(2) TP (Eq.8)	(3) TP (Eq.9)	(4) TP (Eq.10)	(5) TP (Eq.11)
Price index instrument ( $\delta^1$ )	0.3725*** (0.0349)	0.0001 (0.0004)	-0.0000 (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
Price index instrument x Post ( $\delta^3$ )	-0.0119 (0.0477)	0.3621*** (0.0325)	-0.0031 (0.0019)	-0.0008 (0.0014)	-0.0001 (0.0001)
Price index instrument x Warning for actual Harvey ( $\delta^4$ )	-0.0049 (0.0136)	-0.0056 (0.0135)	0.9992*** (0.0116)	-0.0005 (0.0008)	0.0001 (0.0001)
Price index instrument x During actual landfall ( $\delta^5$ )	0.0638** (0.0250)	0.0640** (0.0250)	0.0310 (0.0251)	1.0150*** (0.0199)	-0.0000 (0.0001)
Price index instrument x After actual strike (1-3 weeks) ( $\delta^6$ )	-0.0129 (0.0140)	-0.0129 (0.0143)	0.0029 (0.0021)	0.0000 (0.0001)	1.0007*** (0.0061)
Post-catastrophe year ( $\delta^2$ )	0.0198 (0.0473)	0.6383*** (0.0324)	0.0031 (0.0019)	0.0007 (0.0014)	0.0001 (0.0001)
Applications to Housing Programs (IHP/HA) (%)	0.0014 (0.0039)	0.0003 (0.0039)	0.0003 (0.0031)	-0.0004 (0.0007)	-0.0000 (0.0000)
Applications reporting a need for shelter (%)	-0.0063 (0.0068)	-0.0067 (0.0070)	-0.0024 (0.0040)	-0.0026 (0.0025)	0.0000 (0.0000)
Applications reporting a need for special accommodations (%)	-0.0091 (0.0789)	-0.0008 (0.0791)	-0.0097 (0.0547)	0.0374 (0.0289)	0.0000 (0.0001)
Applications reporting a need for emergency items or food (%)	0.0000 (0.0054)	0.0013 (0.0054)	0.0004 (0.0040)	-0.0006 (0.0013)	0.0000 (0.0000)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.0059 (0.0104)	-0.0057 (0.0104)	-0.0001** (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.0060 (0.0088)	-0.0058 (0.0088)	-0.0001** (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.0135 (0.0105)	0.0139 (0.0106)	-0.0018 (0.0018)	0.0001 (0.0001)	0.0000 (0.0000)
Hurricane Maria period (16 - 30 Sep 2017)	-0.0004 (0.0064)	-0.0005 (0.0064)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Observations	23,164	23,164	23,164	23,164	23,164
Method	IV	IV	IV	IV	IV
Counties	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	Yes	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes
F test of excluded instruments: F-stat=	49.407	26.812	2217.579	1271.491	9378.361
F test of excluded instruments: p-value=	0.000	0.000	0.000	0.000	0.000

Notes: This table reports the first-stage regression results from IV demand estimations (Eq.2.7-Eq.2.11) across coastal counties in the state of Texas. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. The product is toilet paper (TP) in all columns. Price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table 2.6: First-stage regression results from IV demand estimations for batteries - Coastal counties in the state of Texas

VARIABLES	(1) BT (Eq.7)	(2) BT (Eq.8)	(3) BT (Eq.9)	(4) BT (Eq.10)	(5) BT (Eq.11)
Price index instrument ( $\delta^1$ )	0.4385*** (0.0361)	-0.0002 (0.0003)	-0.0000 (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)
Price index instrument x Post ( $\delta^3$ )	0.0110 (0.0629)	0.4512*** (0.0515)	-0.0104** (0.0047)	-0.0015 (0.0024)	-0.0000 (0.0000)
Price index instrument x Warning for actual Harvey ( $\delta^4$ )	0.0181* (0.0102)	0.0177* (0.0102)	1.0148*** (0.0103)	-0.0012 (0.0028)	-0.0017 (0.0013)
Price index instrument x During actual landfall ( $\delta^5$ )	0.0470* (0.0243)	0.0467* (0.0243)	0.0291 (0.0240)	1.0234*** (0.0182)	0.0004 (0.0003)
Price index instrument x After actual strike (1-3 weeks) ( $\delta^6$ )	0.1033** (0.0416)	0.1027** (0.0437)	-0.0055 (0.0068)	0.0000 (0.0005)	1.0807*** (0.0363)
Post-catastrophe year ( $\delta^2$ )	-0.0164 (0.0627)	0.5465*** (0.0513)	0.0106** (0.0048)	0.0015 (0.0024)	0.0000 (0.0000)
Applications to Housing Programs (IHP/HA) (%)	-0.0050 (0.0036)	-0.0055 (0.0035)	-0.0063* (0.0037)	-0.0037 (0.0024)	0.0001 (0.0001)
Applications reporting a need for shelter (%)	-0.0002 (0.0055)	-0.0002 (0.0055)	0.0055 (0.0046)	-0.0027 (0.0044)	-0.0001 (0.0001)
Applications reporting a need for special accommodations (%)	-0.0571 (0.0747)	-0.0234 (0.0746)	-0.0404 (0.0709)	0.0258 (0.0310)	0.0007 (0.0009)
Applications reporting a need for emergency items or food (%)	0.0085* (0.0049)	0.0073 (0.0049)	0.0073 (0.0047)	0.0046* (0.0026)	-0.0001 (0.0001)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.0204 (0.0137)	-0.0211 (0.0136)	-0.0004 (0.0002)	-0.0000 (0.0001)	0.0000 (0.0000)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	0.0139 (0.0172)	0.0131 (0.0172)	-0.0003* (0.0002)	-0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	-0.0024 (0.0094)	-0.0015 (0.0094)	-0.0007 (0.0042)	0.0005 (0.0012)	0.0012 (0.0009)
Hurricane Maria period (16 - 30 Sep 2017)	0.0023 (0.0155)	0.0025 (0.0156)	-0.0003 (0.0002)	-0.0000 (0.0000)	0.0000 (0.0000)
Observations	10,257	10,257	10,257	10,257	10,257
Method	IV	IV	IV	IV	IV
Counties	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	Yes	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes
F test of excluded instruments: F-stat=	48.179	18.843	2364.288	646.373	212.339
F test of excluded instruments: p-value=	0.000	0.000	0.000	0.000	0.000

Notes: This table reports the first-stage regression results from IV demand estimations (Eq.2.7-Eq.2.11) across coastal counties in the state of Texas. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. The product is batteries (BT) in all columns. Price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table 2.7: First-stage regression results from IV demand estimations for milk - Coastal counties in the state of Texas

VARIABLES	(1) MK (Eq.7)	(2) MK (Eq.8)	(3) MK (Eq.9)	(4) MK (Eq.10)	(5) MK (Eq.11)
Price index instrument ( $\delta^1$ )	0.2045*** (0.0440)	0.0154*** (0.0042)	-0.0003 (0.0004)	-0.0000 (0.0000)	-0.0000 (0.0000)
Price index instrument x Post ( $\delta^3$ )	-0.1218 (0.0838)	0.0687 (0.0711)	-0.0695 (0.0504)	0.0010 (0.0007)	-0.0016 (0.0015)
Price index instrument x Warning for actual Harvey ( $\delta^4$ )	0.0121 (0.0379)	0.0135 (0.0378)	0.9155*** (0.0788)	-0.0007 (0.0022)	0.0091 (0.0068)
Price index instrument x During actual landfall ( $\delta^5$ )	0.0859 (0.0729)	0.0858 (0.0728)	0.0413 (0.0659)	1.1168*** (0.0391)	-0.0004 (0.0011)
Price index instrument x After actual strike (1-3 weeks) ( $\delta^6$ )	0.0692 (0.0522)	0.0728 (0.0520)	0.0874 (0.0739)	0.0001 (0.0003)	1.0519*** (0.0433)
Post-catastrophe year ( $\delta^2$ )	0.1353 (0.0831)	0.9291*** (0.0702)	0.0688 (0.0500)	-0.0010 (0.0007)	0.0015 (0.0015)
Applications to Housing Programs (IHP/HA) (%)	-0.0333** (0.0165)	-0.0371** (0.0163)	-0.0190 (0.0172)	-0.0238*** (0.0090)	-0.0007 (0.0008)
Applications reporting a need for shelter (%)	0.0674*** (0.0246)	0.0696*** (0.0237)	0.0238 (0.0372)	0.0133** (0.0059)	0.0015 (0.0018)
Applications reporting a need for special accommodations (%)	-0.2270 (0.2816)	-0.2154 (0.2771)	0.2310 (0.3018)	0.0467 (0.0719)	-0.0159 (0.0145)
Applications reporting a need for emergency items or food (%)	0.0308 (0.0221)	0.0342 (0.0217)	0.0102 (0.0203)	0.0239** (0.0100)	0.0009 (0.0010)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.0166 (0.0224)	-0.0160 (0.0226)	-0.0003 (0.0009)	-0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	0.0090 (0.0166)	0.0102 (0.0165)	0.0002 (0.0008)	-0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	-0.0499* (0.0292)	-0.0504* (0.0292)	-0.0095 (0.0141)	0.0001 (0.0006)	-0.0023 (0.0018)
Hurricane Maria period (16 - 30 Sep 2017)	0.0496** (0.0222)	0.0503** (0.0223)	-0.0015 (0.0013)	0.0000 (0.0000)	0.0000 (0.0001)
Observations	4,663	4,663	4,663	4,663	4,663
Method	IV	IV	IV	IV	IV
Counties	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	Yes	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes
F test of excluded instruments: F-stat=	5.290	3.858	134.058	192.526	125.753
F test of excluded instruments: p-value=	0.000	0.002	0.000	0.000	0.000

Notes: This table reports the first-stage regression results from IV demand estimations (Eq.2.7-Eq.2.11) across coastal counties in the state of Texas. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. The product is milk (MK) in all columns. Price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table 2.8: First-stage regression results from IV demand estimations for bottled water - Coastal counties in the state of Texas

VARIABLES	(1) BW (Eq.7)	(2) BW (Eq.8)	(3) BW (Eq.9)	(4) BW (Eq.10)	(5) BW (Eq.11)
Price index instrument ( $\delta^1$ )	0.2898*** (0.0215)	-0.0002 (0.0004)	0.0000 (0.0000)	-0.0000 (0.0000)	-0.0000 (0.0000)
Price index instrument x Post ( $\delta^3$ )	-0.0400 (0.0349)	0.2501*** (0.0275)	-0.0076*** (0.0025)	-0.0005* (0.0003)	-0.0005* (0.0003)
Price index instrument x Warning for actual Harvey ( $\delta^4$ )	0.0113 (0.0114)	0.0114 (0.0114)	1.0171*** (0.0090)	0.0001 (0.0007)	-0.0040 (0.0029)
Price index instrument x During actual landfall ( $\delta^5$ )	-0.0314** (0.0143)	-0.0314** (0.0143)	-0.0281* (0.0145)	1.0106*** (0.0096)	0.0012 (0.0008)
Price index instrument x After actual strike (1-3 weeks) ( $\delta^6$ )	-0.0125 (0.0176)	-0.0124 (0.0176)	-0.0139 (0.0116)	-0.0000 (0.0000)	1.0057*** (0.0170)
Post-catastrophe year ( $\delta^2$ )	0.0486 (0.0346)	0.7464*** (0.0273)	0.0076*** (0.0025)	0.0005* (0.0003)	0.0005* (0.0003)
Applications to Housing Programs (IHP/HA) (%)	0.0076* (0.0041)	0.0075* (0.0041)	0.0073* (0.0040)	0.0024* (0.0014)	0.0004 (0.0003)
Applications reporting a need for shelter (%)	-0.0054 (0.0035)	-0.0057* (0.0035)	-0.0055* (0.0031)	-0.0015 (0.0014)	-0.0004 (0.0003)
Applications reporting a need for special accommodations (%)	0.1552*** (0.0471)	0.1574*** (0.0472)	0.1510*** (0.0435)	-0.0042 (0.0145)	0.0005 (0.0009)
Applications reporting a need for emergency items or food (%)	-0.0153*** (0.0051)	-0.0153*** (0.0051)	-0.0151*** (0.0049)	-0.0023 (0.0017)	-0.0002 (0.0001)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	0.0015 (0.0059)	0.0016 (0.0059)	-0.0001** (0.0001)	-0.0000 (0.0000)	-0.0000* (0.0000)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	0.0007 (0.0061)	0.0007 (0.0061)	-0.0001* (0.0000)	-0.0000 (0.0000)	-0.0000* (0.0000)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.0132 (0.0083)	0.0130 (0.0083)	-0.0025 (0.0031)	-0.0000 (0.0002)	0.0013 (0.0010)
Hurricane Maria period (16 - 30 Sep 2017)	0.0082 (0.0062)	0.0082 (0.0062)	-0.0000 (0.0001)	-0.0000 (0.0000)	-0.0000 (0.0000)
Observations	29,204	29,204	29,204	29,204	29,204
Method	IV	IV	IV	IV	IV
Counties	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	Yes	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes
F test of excluded instruments: F-stat=	54.169	18.020	4179.470	2912.071	908.328
F test of excluded instruments: p-value=	0.000	0.000	0.000	0.000	0.000

Notes: This table reports the first-stage regression results from IV demand estimations (Eq.2.7-Eq.2.11) across coastal counties in the state of Texas. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. The product is bottled water (BW) in all columns. Price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Based on the endogeneity test results following the demand estimations using the instrumental variables technique, we fail to reject the null hypothesis that the five presumed endogenous regressors can be treated as exogenous for milk in coastal counties (Table 2.9, column three) and across all counties in the state of Texas (Table A.10, columns three and six). Hence, we rely on the previous results for milk, where observed price increases in coastal counties at warning (4.9%), during landfall (8.4%), and after strike (7.3%), are all demand-driven.

For bottled water, we reject exogeneity of the regressors and rely on the IV estimates of efficient price changes, which illustrate Section 2.3.3 (*Case 3*). Relative to pre-disaster levels, a drop in bottled water prices up to 43.2% at warning, 39.9% during landfall, and 36.3% after strike, would still be demand-driven in coastal counties (Table 2.9, column four). We cannot draw the same conclusion across all counties in the state because the IV estimate of pre-catastrophe price elasticity of demand is not significant (Table A.10, columns four and eight).

For toilet paper and batteries, we also reject exogeneity of the regressors and rely on the IV estimates of efficient price changes. We find that in all periods examined in coastal counties, toilet paper prices are expected to increase by 22% relative to the pre-catastrophe price levels (Table 2.9, column one), while battery prices should remain constant at efficiency Table 2.9, column three). Similarly, estimation results across all counties in the state illustrate Section 2.3.3 (*Case 1*) for toilet paper and Section 2.3.3 (*Case 2*) for batteries. Toilet paper prices are expected to rise by up 3.087 times at warning, 2.922 times during landfall, and 2.749 times after strike (Table A.10, columns one and five), while battery prices are expected not to change in any period examined (Table A.10, columns two and six).

Table 2.9: Demand changes due to warning for Harvey, during landfall, and after strike, using IV estimations - Coastal counties in the state of Texas

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW
Price index ( $\vartheta^{pre}$ )	-4.068*** (0.757)	-1.534*** (0.376)	1.499 (1.701)	-2.895*** (0.772)
Price index x Post ( $\beta^{post}$ )	3.766*** (0.864)	-0.581 (0.557)	-6.614 (6.337)	-4.130*** (1.274)
Price index x Warning for actual Harvey ( $\beta^{\varpi}$ )	-0.052 (0.121)	-0.144 (0.147)	-0.273 (0.294)	0.511*** (0.158)
Price index x During actual landfall ( $\beta^h$ )	-0.037 (0.233)	0.072 (0.190)	0.814* (0.471)	0.787*** (0.211)
Price index x After actual Harvey strike (1-3 weeks) ( $\beta^{\S}$ )	-0.103 (0.548)	-0.388 (0.540)	-0.271 (0.477)	1.085*** (0.237)
Post-catastrophe year ( $\alpha$ )	-3.818*** (0.855)	0.611 (0.558)	6.499 (6.304)	4.120*** (1.264)
Applications to Housing Programs (IHP/HA) (%)	0.078 (0.047)	-0.007 (0.071)	-0.085 (0.228)	-0.055 (0.058)
Applications reporting a need for shelter (%)	-0.142** (0.071)	-0.012 (0.089)	0.147 (0.481)	0.102 (0.069)
Applications reporting a need for special accommodations (%)	0.366 (0.764)	-1.245 (0.811)	-2.367 (2.153)	-0.031 (0.870)
Applications reporting a need for emergency items or food (%)	-0.067 (0.067)	0.094 (0.091)	0.183 (0.230)	0.022 (0.094)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	0.131* (0.073)	-0.020 (0.167)	0.061 (0.191)	0.410*** (0.129)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.127** (0.058)	0.035 (0.148)	0.168 (0.157)	0.137 (0.108)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	-0.076 (0.089)	0.112 (0.132)	-0.314 (0.379)	-0.151 (0.120)
Hurricane Maria period (16 - 30 Sep 2017)	0.071 (0.070)	0.069 (0.117)	0.325 (0.321)	0.138 (0.105)
Observations	23,164	10,257	4,663	29,204
R-squared	-0.084	-0.017	-0.182	-0.084
Method	IV	IV	IV	IV
Counties	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes
Endogeneity test of endogenous regressors: Chi-square=	35.037	29.796	5.823	52.337
Endogeneity test of endogenous regressors: p-value=	0.000	0.000	0.324	0.000
Elasticity in pre-catastrophe=	-4.032	-1.531	1.477	-2.869
Testing elasticity in pre-catastrophe=0, p-value=	0.000	0.000	0.378	0.000
Change in elasticity parameter at warning ( $\beta^{post} + \beta^{\varpi}$ )=	3.714	-0.725		-3.619
Testing ( $\beta^{post} + \beta^{\varpi}$ ) = 0, p-value=	0.000	0.207		0.005
Efficient price change at warning ( $\lambda^{\varpi}$ )=	0.224	0.000		-0.432
Change in elasticity parameter at landfall ( $\beta^{post} + \beta^h$ )=	3.729	-0.509		-3.343
Testing ( $\beta^{post} + \beta^h$ ) = 0, p-value=	0.000	0.375		0.011
Efficient price change at landfall ( $\lambda^h$ )=	0.225	0.000		-0.399
Change in elasticity parameter after strike ( $\beta^{post} + \beta^{\S}$ )=	3.663	-0.970		-3.045
Testing ( $\beta^{post} + \beta^{\S}$ ) = 0, p-value=	0.000	0.198		0.020
Efficient change after strike ( $\lambda^{\S}$ )=	0.221	0.000		-0.363

Notes: This table reports the results from IV demand estimations using a flexible, semi-log functional form (Eq.2.5). The estimation sample covers all four years (2014-2017) and includes coastal counties in the state of Texas. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. Dependent variable is log store-UPC-level total quantity purchased. Using Eq.2.5, the elasticity coefficient in pre-catastrophe years is  $\eta = (\vartheta^{pre}) * \bar{P}$  evaluated at the estimation sample mean. Efficient price changes are set to zero if the change in elasticity parameter is not significant at the 5% level. In column 3, efficient price changes are not computed because  $\vartheta^{pre}$  is not significant at the 5% level. The product is toilet paper (TP) in column 1, batteries (BT) in column 2, milk (MK) in column 3, and bottled water (BW) in column 4. Price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Taking the IV estimates of demand-driven price changes as a benchmark for all products except milk in coastal counties, we estimate Eq.2.6 using the sample of observations with non-missing instruments and conduct the price gouging test. We observe that toilet paper prices rise by 0.4% at warning, 8.4% during landfall, and 0.3% after strike, which all fall below the allowable proportional price increase of 22% for this product (Table 2.10, column one). For batteries, we find no significant difference between observed price changes and efficient price changes, except after the strike, where battery prices rise inefficiently by 12.2% above pre-hurricane prices (Table 2.10, column two). For bottled water, we find that prices fall by 2.2% and 0.3% during landfall and after the strike, respectively, which is within the range of expected drops in prices at those periods; however, at warning, bottled water prices rise by 2.3%, while efficiently, they should drop in that period (Table 2.10, column three).

Altogether, OLS results for milk and IV results for the three other products indicate no excessive manipulation of prices in coastal counties. Observed prices for milk and toilet paper rise by less than 9% even though the efficient, capped price increases are 86% (for milk during landfall) and 22% (for toilet paper). Where demand-driven changes call for zero cap (for batteries), observed prices do follow efficiency, except in the three weeks after the strike when prices soar up by less than 13%. Where changes in demand request a drop in prices (for bottled water), prices do fall as expected, except in the warning period when observed prices rise by less than 3%. These observed price changes would even comply with a standard proportional price increase cap of 25%, suggesting no evidence of price gouging for the four products studied in coastal counties.

There also is no evidence about the practice of price gouging across all counties in the state of Texas. Table A.11 presents IV-based estimation results assessing price changes across all counties in the state for toilet paper and batteries. Although the efficient cap is above 260% for toilet paper, we observe demand-driven toilet paper price increases, not exceeding 3% in all periods examined (columns one and three). The efficient zero-cap for battery price increases is also achieved in all periods except during landfall when prices rise inefficiently by 3% (columns two and four). OLS results for bottled water indicate price increases by less than 3%, with inefficiency only at warning (column eight). OLS results for milk also indicate insignificant

price differences at warning and in the three weeks following landfall, as expected; the deviation from efficiency occurs during landfall, with prices falling by less than 2%, while price increases could be efficiently capped at 101.1%.

Table 2.10: Assessment of price changes due to warning for Harvey, during landfall, and after strike, using IV estimates of demand changes - Coastal counties of the state of Texas

VARIABLES	(1) TP	(2) BT	(3) BW
Post-catastrophe year ( $\ell$ )	0.011*** (0.001)	-0.005* (0.003)	0.010*** (0.001)
Warning for actual Harvey ( $\varpi$ )	-0.007 (0.013)	0.001 (0.011)	0.013 (0.012)
During actual Harvey landfall ( $h$ )	0.073*** (0.027)	0.052* (0.029)	-0.032** (0.014)
After actual Harvey strike (1-3 weeks) ( $\S$ )	-0.008 (0.020)	0.127*** (0.036)	-0.013 (0.018)
Applications to Housing Programs (IHP/HA) (%)	0.000 (0.004)	-0.003 (0.004)	0.008* (0.004)
Applications reporting a need for shelter (%)	-0.009 (0.008)	-0.000 (0.006)	-0.004 (0.004)
Applications reporting a need for special accommodations (%)	0.031 (0.078)	-0.054 (0.085)	0.158*** (0.047)
Applications reporting a need for emergency items or food (%)	0.001 (0.006)	0.007 (0.006)	-0.016*** (0.005)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.006 (0.011)	-0.029** (0.014)	-0.000 (0.006)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.007 (0.010)	0.007 (0.018)	-0.001 (0.006)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.009 (0.010)	0.006 (0.011)	0.013 (0.009)
Hurricane Maria period (16 - 30 Sep 2017)	0.001 (0.006)	-0.001 (0.015)	0.007 (0.006)
Constant	0.988*** (0.001)	1.004*** (0.002)	0.985*** (0.001)
Observations	23,164	10,257	29,204
R-squared	0.008	0.005	0.007
Counties	Coastal	Coastal	Coastal
Price index winsorized at the 2nd and 98th percentiles	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes
Efficient price change at warning ( $\lambda^{\varpi}$ )=	0.224	0.000	-0.432
Observed price change at warning ( $\varpi + \ell$ )=	0.004	-0.004	0.023
Testing observed price change at warning ( $\varpi + \ell$ ) = $\lambda^{\varpi}$ , p-value=	0.000	0.724	0.000
Efficient price change at landfall ( $\lambda^h$ )=	0.225	0.000	-0.399
Observed price change at landfall ( $h + \ell$ )=	0.084	0.047	-0.022
Testing observed price change at landfall ( $h + \ell$ ) = $\lambda^h$ , p-value=	0.000	0.106	0.000
Efficient change after strike ( $\lambda^{\S}$ )=	0.221	0.000	-0.363
Observed price change after strike ( $\S + \ell$ )=	0.003	0.122	-0.003
Testing observed price change after strike ( $\S + \ell$ ) = $\lambda^{\S}$ , p-value=	0.000	0.001	0.000

Notes: This table reports the results from price estimations using Eq.2.6. The estimation sample covers all four years (2014-2017) and includes coastal counties in the state of Texas. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. Dependent variable is store-UPC-level price index winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. The product is toilet paper (TP) in column 1, batteries (BT) in column 2, and bottled water (BW) in column 3. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Taken as a whole, our findings about modest price changes due to actual Hurricane Harvey corroborate prior work showing pervasive, unapparent changes in retail prices during hurricanes in the U.S. (Beatty et al., 2021; Gagnon & López-Salido, 2020), peak demand seasons in the U.S. (Chevalier et al., 2003; MacDonald, 2000), weekends of peak demand seasons in the U.S. (Warner & Barsky, 1995), and earthquakes outside the U.S. (A. Cavallo et al., 2014). We complement this literature by showing that the relatively stable prices in the hurricane season studied remain demand-driven because the observed price changes mostly fall within the efficiency ranges allowed by the changes in the price elasticity of demand.

### 2.6.2 Changes in prices due to Placebo Harvey

So far, we have shown that prices of the necessity and emergency goods studied do change—albeit modestly—in times of actual Hurricane Harvey. Notwithstanding that the observed price changes mostly occur in an efficient way in times of actual Hurricane Harvey, the findings would not be robust if the same price changes could be observed in times of absence of Hurricane Harvey. Figure A.5 through Figure A.8 indicate a pattern of price changes before and after actual Harvey similar to the one of a placebo hurricane occurring exactly 2 years earlier. For clarity, we estimate changes in prices due to Placebo Harvey (occurring exactly 2 years earlier) using Eq.2.12. The specification is similar to Eq.2.6, except that we do not include warning and confounders since the hurricane is a placebo. The estimation sample includes observations for all counties in the pre-hurricane years (2014 and 2015). Because the base year relative to which the Placebo price changes are assessed is 2014, *Post* becomes the indicator for year 2015.

$$P_{rsjyw}^{index} = \nu + \mathbb{1}_{sj}Year2015_y + \beta_{sj}PlaceboLandfall_{rsyw} + \alpha_{sj}AfterStrike_{rsyw} + \gamma_s + v_{rsjyw}. \quad (\text{Eq.2.12})$$

We test two null placebo hypotheses for landfall effects: (1)  $\beta_{sj} + \mathbb{1}_{sj} = 0$ , which stands for no price change at the timing of Placebo Harvey landfall relative to the price level in year 2014; and (2)  $\beta_{sj} + \mathbb{1}_{sj} = \bar{h}_+ \ell_{sj}$ , with  $\bar{h}_{sj}, \ell_{sj}$  obtained in Eq.2.6, which stands for no difference between price change during Placebo landfall and price change during actual landfall. Similarly, we test two null placebo hypotheses for after-

strike effects: (1)  $\alpha_{sj} + \beta_{sj} = 0$ , which stands for no price change after Placebo strike (1-3 weeks) relative to the price level in year 2014; and (2)  $\alpha_{sj} + \beta_{sj} = \delta_{+} \ell_{sj}$ , for  $\delta_{sj}, \ell_{sj}$  obtained in Eq.2.6, which stands for no difference between price change after Placebo strike (1-3 weeks) and price change after actual strike (1-3 weeks). While testing the set of null hypotheses (2) helps answer the question “Should there be no Hurricane Harvey, would we observe the same change in prices as for the actual Hurricane Harvey?,” the set of null hypotheses (1) rather questions the following “Do we observe any price change pertaining to the Placebo hurricane?.” Failure to reject (2) provides another validation that there is no manipulation of prices in actual Hurricane Harvey season. Failure to reject (1) means that the conclusion in (2) does not vary with the Placebo hurricane year.

Estimation results using Eq.2.12 are in Table 2.11. We fail to reject the null hypothesis (2) for landfall effects at the 5% level of significance (all columns). Moreover, we reject the null hypothesis (1) for landfall effects at the 5% level of significance only for toilet paper (column five). Thus, the observed price changes during actual Hurricane Harvey landfall could be seen in a non-hurricane year at the same timing, for all goods except toilet paper.

Besides, we reject the null hypothesis (2) for after-strike effects at the 5% level of significance, only for milk (columns three and seven). In addition, we fail to reject the null hypothesis (1) for after-strike effects at the 5% level of significance (all columns). Hence, price changes occurring in the three weeks following Hurricane Harvey are robust only for milk, and this result is consistent with any Placebo hurricane year.

To sum up, based on the placebo hypothesis tests, observed price changes in the wake of Hurricane Harvey, for batteries and bottled water, are just the effects of predictable peak demand shocks. This is also true for milk (during Harvey landfall) and toilet paper (in the immediate aftermath of Harvey strike). The observed price changes plausibly attributable to Hurricane Harvey are those for toilet paper (demand-driven increase by 2.3% during Harvey landfall) and milk (demand-driven increase by about 2.9 to 3.1% in the immediate aftermath of Harvey strike) across all counties in the state of Texas. Since the placebo does not affect warning, Harvey warning effects are maintained.

Table 2.11: Price changes during placebo Harvey landfall and after strike - All counties in the state of Texas

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Year 2015 ( $\mathbb{1}$ )	0.006*** (0.001)	0.004 (0.003)	-0.004 (0.003)	-0.002 (0.001)	0.006*** (0.001)	-0.000 (0.002)	-0.003 (0.002)	-0.002* (0.001)
During placebo Harvey landfall ( $\mathbb{1}$ )	0.008 (0.009)	-0.005 (0.022)	0.116 (0.110)	0.002 (0.007)	0.012 (0.008)	-0.004 (0.018)	0.031 (0.036)	0.005 (0.007)
After placebo strike (1-3 weeks) ( $\mathcal{X}$ )	0.001 (0.024)	-0.083 (0.082)	-0.044 (0.035)	-0.006 (0.009)	-0.005 (0.018)	-0.045 (0.053)	-0.045 (0.035)	-0.002 (0.009)
Constant	0.989*** (0.001)	1.000*** (0.002)	0.986*** (0.002)	0.993*** (0.001)	0.987*** (0.001)	0.998*** (0.001)	0.987*** (0.002)	0.990*** (0.001)
Observations	43,678	19,277	9,189	53,375	43,678	19,277	9,189	53,375
R-squared	0.007	0.011	0.026	0.006	0.010	0.015	0.028	0.010
Counties	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observed price change at placebo landfall ( $\mathbb{1} + \mathbb{1}$ )=	0.014	0.000	0.112	0.000	0.018	-0.005	0.028	0.003
Testing observed price change at placebo landfall ( $\mathbb{1} + \mathbb{1}$ ) = 0, p-value=	0.103	0.985	0.305	0.982	0.024	0.786	0.441	0.702
Observed price change at actual landfall ( $\mathbb{1} + \ell$ )=	0.023	0.033	-0.018	0.003	0.023	0.029	-0.018	0.008
Testing equality ( $\mathbb{1} + \mathbb{1}$ ) = ( $\mathbb{1} + \ell$ ), p-value=	0.289	0.128	0.234	0.669	0.492	0.057	0.203	0.459
Observed price change after placebo strike ( $\mathcal{X} + \mathbb{1}$ )=	0.007	-0.079	-0.048	-0.008	0.001	-0.045	-0.048	-0.004
Testing observed price change after placebo strike ( $\mathcal{X} + \mathbb{1}$ ) = 0, p-value=	0.768	0.337	0.172	0.390	0.948	0.393	0.169	0.651
Observed price change after actual strike ( $\mathcal{X} + \ell$ )=	0.003	-0.007	0.029	-0.001	0.007	-0.003	0.031	0.000
Testing equality ( $\mathcal{X} + \mathbb{1}$ ) = ( $\mathcal{X} + \ell$ ), p-value=	0.863	0.382	0.028	0.452	0.750	0.425	0.024	0.651

Notes: This table reports the results from price estimations using Eq.2.12. The estimation sample covers pre-hurricane years (2014-2015) and includes all counties in the state of Texas. Dependent variable is store-UPC-level price index. In columns 5 through 8, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. The product is toilet paper (TP) in columns 1 and 5, batteries (BT) in columns 2 and 6, milk (MK) in columns 3 and 7, and bottled water (BW) in columns 4 and 8. Timing of placebo Harvey landfall in Texas is August 25-26th, 2015. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

### 2.6.3 Actual and Placebo estimations using base year 2014 and dropping out year 2016 of the sample

To make a proper comparison of the effects of actual Harvey (in 2017) to those of placebo Harvey (in 2015), it would be preferable to use the same base year, say 2014. In these new estimations, year 2016 is dropped out of the sample, and the effects of actual and placebo Harvey are interpreted relative to 2014.

Table 2.12 presents price efficiency test results based on the OLS estimates of demand changes between year 2014 and year 2017 (Table A.12). Using non-winsorized (columns one through three) or winsorized (columns four through seven) price index, a first pattern in the results is that the efficiency rule allows for price changes due to actual Hurricane Harvey only for bottled water (columns three and seven), with allowable increases over 110% at warning ( $\lambda^{\varpi}$ ), 160% during landfall ( $\lambda^h$ ), and 200% in the short-term aftermath ( $\lambda^{\S}$ ). Another pattern in these OLS-based results is that there is no deviation from efficiency, except for toilet paper and batteries during landfall. We observe a rise in toilet paper prices by 3.7% (column four), and battery prices by 3.9% (column one) and 3.4% (column five) during landfall. Observed price changes for milk are insignificant at the 5% level at all periods of interest (columns two and six). Increases in bottled water prices are observed only at warning, by 3% in column three and 2.7% in column seven, which all fall within the efficient ranges.

We rely on IV estimates of demand changes between 2014 and 2017 only for batteries. For the other products, we either fail to reject the null hypothesis that the presumed endogenous regressors can be treated as exogenous (milk and bottled water in Table A.13, columns three, four, seven, and eight) or fail to find the IV estimate of the pre-disaster demand elastic (toilet paper, milk, and bottled water in Table A.13, columns one, three through five, and seven through eight). Results for batteries indicate  $\lambda^{\varpi} = \lambda^h = \lambda^{\S} = 0$  (Table A.13, columns two and six), hence no allowable price increase at efficiency. These expected, constant battery prices are observed at warning and in the shorter aftermath of Harvey strike, but not during landfall, where we find significant increases by 3.5% (Table 2.13, column one) and 2.9% (Table 2.13, column two).

Table 2.12: Assessment of price changes due to warning for Harvey, during landfall, and after strike - All counties of the state of Texas, Base year 2014

VARIABLES	(1) BT	(2) MK	(3) BW	(4) TP	(5) BT	(6) MK	(7) BW
Post-catastrophe year ( $\ell$ )	0.002 (0.003)	0.003 (0.004)	0.009*** (0.001)	0.014*** (0.001)	-0.001 (0.002)	0.001 (0.003)	0.009*** (0.001)
Warning for actual Harvey ( $\varpi$ )	0.001 (0.012)	0.004 (0.023)	0.021** (0.010)	0.000 (0.009)	0.004 (0.010)	0.011 (0.021)	0.018** (0.007)
During actual Harvey landfall ( $h$ )	0.037*** (0.014)	-0.020 (0.026)	-0.007 (0.011)	0.024** (0.011)	0.035*** (0.013)	-0.020 (0.025)	-0.002 (0.008)
After actual Harvey strike (1-3 weeks) ( $\S$ )	-0.004 (0.042)	0.026 (0.029)	-0.010 (0.010)	-0.003 (0.014)	-0.001 (0.042)	0.001 (0.026)	0.028 (0.009)
Applications to Housing Programs (IHP/HA) (%)	-0.003 (0.008)	-0.018 (0.016)	0.006 (0.005)	-0.003 (0.004)	-0.001 (0.005)	-0.019 (0.016)	0.007* (0.004)
Applications reporting a need for shelter (%)	0.003 (0.009)	0.061*** (0.023)	-0.006* (0.003)	-0.005 (0.005)	0.002 (0.007)	0.059*** (0.022)	-0.005 (0.003)
Applications reporting a need for special accommodations (%)	-0.073 (0.098)	-0.093 (0.252)	0.171*** (0.061)	0.045 (0.079)	-0.050 (0.091)	-0.086 (0.248)	0.141*** (0.045)
Applications reporting a need for emergency items or food (%)	0.006 (0.011)	0.009 (0.020)	-0.015** (0.006)	0.004 (0.006)	0.002 (0.008)	0.010 (0.019)	-0.014*** (0.005)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.001 (0.009)	-0.026 (0.019)	0.000 (0.006)	-0.001 (0.005)	-0.001 (0.007)	-0.028** (0.014)	0.002 (0.004)
Hurricane Maria period (16 - 30 Sep 2017)	-0.020* (0.012)	0.029* (0.015)	0.000 (0.004)	0.002 (0.003)	-0.014* (0.008)	0.021* (0.012)	0.001 (0.003)
Constant	1.000*** (0.002)	0.987*** (0.002)	0.993*** (0.001)	0.987*** (0.001)	0.998*** (0.001)	0.987*** (0.002)	0.989*** (0.001)
Observations	17,516	8,025	49,521	38,592	17,516	8,025	49,521
R-squared	0.010	0.030	0.005	0.012	0.011	0.042	0.008
Counties	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Efficient price change at warning ( $\lambda^\varpi$ )=	0.000	0.000	2.224	0.000	0.000	0.000	1.137
Observed price change at warning ( $\varpi + \ell$ )=	0.003	0.007	0.030	0.014	0.003	0.012	0.027
Testing observed price change at warning ( $\varpi + \ell$ ) = $\lambda^\varpi$ , p-value=	0.810	0.767	0.000	0.108	0.763	0.568	0.000
Efficient price change at landfall ( $\lambda^h$ )=	0.000	0.000	3.231	0.000	0.000	0.000	1.620
Observed price change at landfall ( $h + \ell$ )=	0.039	-0.017	0.002	0.037	0.034	-0.019	0.007
Testing observed price change at landfall ( $h + \ell$ ) = $\lambda^h$ , p-value=	0.006	0.508	0.000	0.001	0.007	0.459	0.000
Efficient change after strike ( $\lambda^\S$ )=	0.000	0.000	4.063	0.000	0.000	0.000	2.007
Observed price change after strike ( $\S + \ell$ )=	-0.002	0.029	-0.001	0.010	0.001	0.029	0.000
Testing observed price change after strike ( $\S + \ell$ ) = $\lambda^\S$ , p-value=	0.965	0.321	0.000	0.461	0.990	0.260	0.000

Notes: This table reports the results from price estimations using Eq.2.6. The estimation sample covers years 2014 and 2017 and includes all counties in the state of Texas. Dependent variable is store-UPC-level price index. In columns 4 through 7, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. The product is toilet paper (TP) in column 4, batteries (BT) in columns 1 and 5, milk (MK) in columns 2 and 6, and bottled water (BW) in columns 3 and 7. Toilet paper results using non-winsorized price index are not presented because in Table A.12, column 1,  $\vartheta^{PTE}$  is not significant at the 5% level, and efficient price changes are not computed. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table 2.13: Assessment of price changes due to warning for Harvey, during landfall, and after strike, using IV estimates of demand changes - All counties of the state of Texas, Base year 2014

VARIABLES	(1) BT	(2) BT
Post-catastrophe year ( $\ell$ )	0.002 (0.003)	-0.001 (0.002)
Warning for actual Harvey ( $\varpi$ )	-0.005 (0.011)	0.000 (0.009)
During actual Harvey landfall ( $h$ )	0.032** (0.015)	0.030** (0.013)
After actual Harvey strike (1-3 weeks) ( $\S$ )	-0.008 (0.046)	-0.002 (0.047)
Applications to Housing Programs (IHP/HA) (%)	-0.002 (0.004)	-0.003 (0.004)
Applications reporting a need for shelter (%)	-0.004 (0.006)	-0.003 (0.005)
Applications reporting a need for special accommodations (%)	-0.068 (0.085)	-0.042 (0.080)
Applications reporting a need for emergency items or food (%)	0.008 (0.006)	0.007 (0.006)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.005 (0.010)	0.001 (0.007)
Hurricane Maria period (16 - 30 Sep 2017)	-0.029** (0.012)	-0.019** (0.008)
Constant	0.999*** (0.002)	0.997*** (0.001)
Observations	16,143	16,143
R-squared	0.011	0.011
Counties	All	All
Price index winsorized at the 2nd and 98th percentiles	No	Yes
Store fixed effects and sampling weights	Yes	Yes
Efficient price change at warning ( $\lambda^{\varpi}$ )=	0.000	0.000
Observed price change at warning ( $\varpi + \ell$ )=	-0.003	-0.001
Testing observed price change at warning ( $\varpi + \ell$ ) = $\lambda^{\varpi}$ , p-value=	0.801	0.932
Efficient price change at landfall ( $\lambda^h$ )=	0.000	0.000
Observed price change at landfall ( $h + \ell$ )=	0.035	0.029
Testing observed price change at landfall ( $h + \ell$ ) = $\lambda^h$ , p-value=	0.021	0.025
Efficient change after strike ( $\lambda^{\S}$ )=	0.000	0.000
Observed price change after strike ( $\S + \ell$ )=	-0.005	-0.003
Testing observed price change after strike ( $\S + \ell$ ) = $\lambda^{\S}$ , p-value=	0.909	0.950

*Notes:* This table reports the results from price estimations using Eq.2.6. The estimation sample covers years 2014 and 2017 and includes all counties in the state of Texas. Dependent variable is store-UPC-level price index. In columns 3 through 4, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. The product is batteries (BT) in columns 1 and 2. Results are not presented for toilet paper, milk, and bottled water, because in Table A.13, columns 1, 3-5, and 7-8,  $\vartheta^{pre}$  is not significant at the 5% level, and efficient price changes are not computed. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

We turn to the placebo hypothesis tests, where placebo Harvey effects are now compared with actual Harvey effects estimated using 2014 as the base year (those in Table 2.13 for batteries and those in Table 2.12 for the other three products). During landfall, for toilet paper, we reject the null hypothesis of equality of price changes of placebo and actual Harvey, relative to the same base year 2014: while toilet paper prices increase by 1.8% during placebo Harvey landfall, the increase is twofold during actual Harvey landfall

(Table A.14, column four). This reinforces our previous conclusion that the price changes during actual Harvey landfall could be seen in a non-hurricane year at the same timing, for all goods except toilet paper.

After strike, for milk, we reject the null hypothesis of equality of price changes of placebo and actual Harvey, relative to the same base year 2014: contrary to the decrease in milk prices by 4.8% in the immediate aftermath of placebo Harvey strike (1-3 weeks), milk prices increase by 2.9% in the first three weeks following actual Harvey strike (Table A.14, columns two and six). This further strengthens the earlier point that price changes occurring in the three weeks following actual Harvey could be seen in a non-hurricane year at the same timing, for all goods except milk.

## 2.7 Conclusion

In this paper, we study the dynamic response of the prices of necessity and emergency goods to natural disasters to address the use of arbitrary price percentage increase caps in “price gouging” laws and suggest efficient price changes when a natural disaster is imminent. Our theoretical result indicates that a wide range of price percentage increases is possible. The allowable price percentage increases depend on the values of the pre-disaster parameter of the price elasticity of demand and the values of the change in this parameter between the pre-disaster and post-disaster period of interest. Empirical results for Hurricane Harvey reveal that prices change a little during Hurricane Harvey season, and these small changes due to Hurricane Harvey are demand-driven because the observed price changes are mostly within the ranges of efficiency permitted by variations in the price elasticity of demand. In addition, the price changes observed in the aftermath of Hurricane Harvey could be seen in a hurricane-free year at the same time. These findings suggest the need to revise “price gouging” laws, such as those applied in the state of Texas—Texas Business & Commerce Code Section § 17.46 (2019)—, that set a subjective price cap for necessity goods during a declared disaster. Generally, following our theoretical result, the revision should base on the optimal, short-term changes in demand for each target good in the affected locations relative to a period of absence of the disaster. The empirical application to selected necessity goods suggests zero-

increase price cap for batteries and bottled water, but a cap of 22% for toilet paper and 86% for milk in coastal counties of the state of Texas.

CHAPTER 3  
SECOND ESSAY

AS IT RAINS, SO SHOULD WE CARE? DESCRIBING  
THE STATIC IMPACT OF STORMS ON THE ECON-  
OMY USING A BALANCED PANEL<sup>1</sup>

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<sup>1</sup>Mensah, E. R. and M. J. Filipki, "Impact of natural disasters on the economy," to be submitted to *Nature Climate Change*.

### 3.1 Abstract

Do natural disasters have an immediate impact on the economy when they strike a country? The present short paper describes the static impact of storms on the economy using a balanced panel of 55 countries in a 69-year long period. Event study results indicate the impact of the disaster events on per capita GDP growth, saving rate, inflation rate, depreciation rate of capital stock, employment rate, per capita labor income growth, and real internal rate of return. The findings—robust to heterogeneity, confounders, and pre-trends—suggest no evidence that storms have a static effect on per capita GDP growth, inflation rate, employment rate, and per capita labor income growth. Other natural disasters (floods, earthquakes, droughts, and epidemics) are storm-like in terms of the above findings. However, while storms and droughts do not affect the other outcomes, floods lead to a faster depreciation rate of capital stock (+0.18 pp) and rising saving rates (+2.30 pp). Earthquakes also lead to a faster depreciation rate of capital stock (+0.30 pp). Epidemics lead to a falling real internal rate of return of businesses (-1.87 pp).

*JEL Codes:* Q54, O44, C23.

## 3.2 Introduction

Do natural disasters have an immediate impact on the economy when they strike a country? Beside the direct economic costs in terms of physical asset damage that natural disasters inflict on the society, standard growth theories suggest that the loss of capital stock due to natural disasters can impact the economy by affecting output per capita, savings rate, employment, sectoral composition of economic activities, inflation, and other outcomes (Hallegatte & Dumas, 2009; Hallegatte & Przulski, 2010; Strulik & Trimborn, 2019). Yet, empirical evidence about the macroeconomic effects of natural disasters is not conclusive (Cavallo & Noy, 2009; Kellenberg & Mobarak, 2011; Noy & duPont IV, 2018). On economic growth—the real growth rate of (per capita) GDP—, while some studies support the growth-reducing effect of natural disasters (Felbermayr & Gröschl, 2014; Hochrainer, 2009; Hsiang & Jina, 2014; Raddatz, 2007; Strobl, 2012), other studies find the opposite effect (Albala-Bertrand, 1993; Skidmore & Toya, 2002), or mixed effects by type of disaster and economic development status (Cavallo et al., 2021; Cunado & Ferreira, 2014; Fomby et al., 2013; Kellenberg & Mobarak, 2008; Loayza et al., 2012; Noy, 2009; Panwar & Sen, 2019).

Moreover, there is presence of delayed impacts on growth and inconsistent contemporaneous impacts in the literature, which does not urge disaster management policies. Evidence-based policy-making requires not only the use of rigorous methodologies in establishing evidence to avoid misleading policies but also evidence that sets a motion to action. In a policy perspective, evidence about instantaneous or long-lasting repercussions of natural disasters would set the tone for the need for urgent policy efforts to curb the problem. Yet, empirical studies uncover growth effects that are slow to manifest and do not appear in the instantaneous period in which the disaster hits (Cunado & Ferreira, 2014; Fomby et al., 2013). Empirical studies also estimate contemporaneous economic effects using standard OLS or fixed effects methods (Murlidharan & Shah, 2001; Noy, 2009), which are not robust to heterogeneity in treatment effects (Borusyak et al., 2021; De Chaisemartin & d'Haultfoeuille, 2020b; Goodman-Bacon, 2021; Shabnam, 2014; Słoczyński, 2022).

This paper revisits the contemporaneous effects of natural disasters on per capita GDP growth, saving rate, inflation rate, depreciation rate of capital stock, employment rate, per capita labor income growth, and real internal rate of return, using a more robust econometric technique (Sun & Abraham, 2021). Although the focus is on storms, the paper extends to a comparative analysis of the macroeconomic effects of storms to those of other natural disasters, such as floods, earthquakes, droughts, and epidemics. In essence, the paper reveals nil immediate effects of storms on the studied macroeconomic outcomes. In contrast, the physical destruction generated by floods and earthquakes leads to a faster depreciation rate of capital stock by 0.18 and 0.30 percentage points, respectively. In addition, floods have an immediate positive effect on saving rates by about 2 percentage points, while epidemics have an immediate negative effect on the real internal rate of return of the same magnitude. Because the paper's objective is to describe these immediate effects, the mechanisms at play are not further investigated.

The closest study to this paper is the one in Loayza et al., 2012, which estimates the contemporaneous effects of disaster intensity on the average growth rate of the log of output per capita, using the generalized method of moments (GMM) with time fixed effects and the lagged dependent variable on quinquennial data of 94 countries for the period 1961-2005. They find a significant effect on economic growth, positive for floods and negative for droughts, and led by the impact of the disaster in the agricultural sector. Storms and earthquakes have no effect on economic growth, which the authors argue that it results from positive reconstruction effects of moderate disasters on industrial growth, dampened by potentially adverse effects on agricultural growth. The magnitude of all these effects grows as developing countries are considered. While their methodology has its merits, the use of five-year averages conflates both disaster and economic growth variables, thus making the impact estimates not strictly contemporaneous. By using recent methodologies in the difference-in-differences and event study design literature (Baker et al., 2022; De Chaisemartin & D'Haultfoeuille, 2022; Roth et al., 2022) that improve upon specifications using time and country fixed effects and estimate static treatment effects, our paper provides more appropriate contemporaneous impacts of natural disasters. Our paper is also unique in providing a macroeconomic

impact evaluation of natural disasters that includes epidemic events and other outcomes such as the per capita labor income growth and the real internal rate of return.

The remainder of the paper is organized as follows. Section 3.3 presents the data and the methodology. Section 3.4 provides the basic regression results. Section 3.5 points out to stronger findings through some robustness checks of the basic results and discusses them. Section 3.6 puts forth a conclusion with a caution about the limitations of this paper.

## 3.3 Data and methods

### 3.3.1 Data

Population and macroeconomic data from the Penn World Table (Feenstra et al., 2015)<sup>2</sup> is merged with natural disaster data<sup>3</sup> at the country-year level. Floods, storms, epidemics, earthquakes, and droughts account for more than 80% of disaster events around the world over the period 1950-2019 (EM-DAT). Annual growth rates of economic variables for the period 1951 through 2019 are computed. Only 55 countries<sup>4</sup> have complete data for all 69 years for per capita GDP growth, inflation rate, saving rate, and depreciation rate of capital stock.

Table 3.1 shows a large variation in per capita GDP growth in the sample around an average of about 2.6%. A large variation also applies to the other macroeconomic outcomes in the sample. On average, in this sample, floods occur about one third of the time, storms occur about one fourth of the time, epidemics and earthquakes occur one tenth of the time, and droughts occur one fifteenth of the time. Countries which experienced a disaster of interest (flood, storm, earthquake, drought, and epidemic) in 1951 are

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<sup>2</sup><http://www.gdpc.net/pwt>

<sup>3</sup><http://www.emdat.be/database>

<sup>4</sup>Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Canada, Colombia, Costa Rica, Cyprus, D.R. of the Congo, Denmark, Ecuador, Egypt, El Salvador, Ethiopia, Finland, France, Germany, Guatemala, Honduras, Iceland, India, Ireland, Israel, Italy, Japan, Kenya, Luxembourg, Mauritius, Mexico, Morocco, Netherlands, New Zealand, Nicaragua, Nigeria, Norway, Pakistan, Panama, Peru, Philippines, Portugal, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Thailand, Trinidad and Tobago, Turkey, Uganda, United Kingdom, United States, Uruguay, and Venezuela.

further excluded in the analysis,<sup>5</sup> leading to a balanced study sample of 3,519 country-year observations covering 51 countries for floods; 3,588 country-year observations covering 52 countries for storms and earthquakes; and 3,795 country-year observations covering all 55 countries for droughts and epidemics.

Table 3.1: Summary statistics of demographic, economic, and disaster characteristics

	Obs	Min	Mean	SD	Max
<b>Panel A: Demographic and economic characteristics</b>					
Population (in millions)	3795	0.146	46.45	122.1	1366.4
Expenditure-side real GDP at current PPPs (in billions 2011 USD)	3795	1.231	586.6	1664.6	20791.4
Annual growth rate of expenditure-side real GDP (percent)	3795	-81.14	4.193	6.197	72.20
Per-capita expenditure-side real GDP at current PPPs (2011 USD p.c.)	3795	251.4	15561.8	15780.9	112564.2
Annual growth rate of per-capita expenditure-side real GDP (percent)	3795	-80.85	2.555	6.037	67.81
Annual price inflation rate (percent)	3795	-60.24	3.708	12.04	355.7
Average depreciation rate of the capital stock	3795	0.0170	0.0408	0.0117	0.0930
Real private savings at current PPPs (in billions 2011 USD)	3795	0.0523	201.2	547.9	6292.2
Private savings rate	3795	0.00376	0.332	0.0907	0.929
Employment rate (percent)	3705	20.19	39.70	8.239	74.82
Annual growth rate of per-capita real labor income (percent)	3381	-80.85	2.510	6.162	72.93
Real internal rate of return	3384	0.01000	0.121	0.0826	0.624
<b>Panel B: Disaster characteristics</b>					
Flood occurred (1=yes, 0=no)	3795	0	0.320	0.467	1
Storm occurred (1=yes, 0=no)	3795	0	0.236	0.425	1
Epidemic occurred (1=yes, 0=no)	3795	0	0.0885	0.284	1
Earthquake occurred (1=yes, 0=no)	3795	0	0.101	0.302	1
Drought occurred (1=yes, 0=no)	3795	0	0.0669	0.250	1
Other disaster type occurred (1=yes, 0=no)	3795	0	0.193	0.395	1

*Notes:* This table describes the balanced panel sample composed of 55 countries, each with 69 annual records for per capita GDP growth rates, saving rates, inflation rates, and depreciation rates of the capital stock. The above four variables have fewer missing values than the employment rate, per capita labor income growth, and internal rate of return. Disaster types other than those five listed in the table include extreme temperature, landslide, wildfire, volcanic activity, mass movement (dry), insect infestation, etc.

<sup>5</sup>Despite not treated with the disaster every year, such countries are always-treated in the staggered sense because the time since the year of the first disaster in the sample will always be non-negative. We would not have good counterfactuals for the treated observations in such countries. In the original balanced panel covering 55 countries and 69 years, always-treated countries—in the staggered sense—are Italy, Japan, Morocco, and United States for floods; Japan, Mexico, and Philippines for storms; El Salvador, Nicaragua, and Turkey for earthquakes; and none for droughts and epidemics.

### 3.3.2 Methods

Consider a panel of  $N$  countries observed for  $T$  years. The economic outcome  $O_{i,t}$  is observed for each  $i \in \{1, \dots, N\}$  and  $t \in \{1, \dots, T\}$ . Let denote  $E_i$  the time when country  $i$  initially becomes hit by a storm and  $K_{i,t} = t - E_i$  the number of years relative to  $E_i$ . Denoting  $D_{i,t} = \mathbf{1}[K_{i,t} \geq 0]$  the treatment indicator with staggered timing, we start with the estimation of a two-way fixed effects (TWFE) regression in Eq.3.1, which captures the static treatment effect of storms on the economic outcome,  $\gamma$ , after removing country fixed effects  $\alpha_i$  and year fixed effects  $\beta_t$ :

$$O_{i,t} = \alpha_i + \beta_t + \gamma D_{i,t} + \varepsilon_{i,t}. \quad (3.1)$$

Following recent econometric research (Borusyak et al., 2021; De Chaisemartin & d’Haultfoeuille, 2020b; Goodman-Bacon, 2021), the coefficient  $\gamma$  in Eq.3.1 is a weighted average of several 2x2 difference-in-differences (DiD) estimates across time and groups of countries treated in the same period, which is not robust to heterogeneity in treatment effects across time and groups. The TWFE estimator does not rule out some “forbidden comparisons” of “late” treated as treatment groups to “early” treated as control groups, leading to changes in the treatment effect over time. Therefore, Goodman-Bacon, 2021’s approach is to decompose  $\gamma$  into mutually exclusive comparisons and examine if the treatment effect stays constant or varies across comparisons. Since Goodman-Bacon, 2021’s decomposition method makes a diagnosis of bias in  $\gamma$  without correcting the estimate, the paper relies on one of the heterogeneity-robust estimation methods suggested in the literature, Sun and Abraham, 2021’s DiD estimator, which uses the countries treated last and the never-treated countries as the controls.

Because multiple natural disasters could occur in a given country-year, we refine Eq.3.1 into Eq.3.2 to make  $\gamma$  more precise by controlling for confounding disasters that are not of interest:

$$O_{i,t} = \alpha_i + \beta_t + \gamma D_{i,t} + C'_{i,t} \Psi + \mu_{i,t}. \quad (3.2)$$

As  $D_{i,t}$  corresponds to the staggered treatment indicator for storms, the vector of confounders  $C'$  includes whether floods, earthquakes, droughts, epidemics, or other natural disasters occur in the given country-year. For example, floods occur about half of the time in years when storms strike; floods and other natural disasters occur as well when storms do not strike (EM-DAT). Estimations of Eq.3.2 are presented as preliminary robustness checks to those of Eq.3.1 in Section 3.4.

The TWFE specification has the caveat that it is not valid if the parallel pre-trends assumption does not hold true. To address this caveat, Eq.3.3 augments Eq.3.2 by including as controls the time periods leading up to the treatment (“leads” or “pre-trends”):

$$O_{i,t} = \alpha_i + \beta_t + \delta_{-10} \mathbf{1}[K_{i,t} \leq -10] + \sum_{k=-9}^{-2} \delta_k \mathbf{1}[K_{i,t} = k] + \gamma D_{i,t} + C'_{i,t} \Psi + \nu_{i,t}. \quad (3.3)$$

The coefficients  $\delta_k$  capture the incidence on the economic outcome  $k$  years before the storm event occurs. I restrict the number of pre-trends to be estimated to nine parameters as a practical “necessity” (Schmidheiny & Siegloch, 2019). All coefficients are relative to the baseline reference “lead” ( $\delta_{-1}$ ), which is omitted in Eq.3.3. Under the common trends assumption, the coefficients  $\delta_k$  should be jointly equal to zero. Thus, results from the estimation of Eq.3.3 in Section 3.5 constitute the preferred estimates.

### 3.4 Static effects of storms on economic outcomes

Naive TWFE results in Table 3.2a suggest a lack of evidence about the static effects of storms on economic growth—negative effect on per capita GDP growth, albeit insignificant—(column one). Similarly, the naive TWFE results indicate no static effect of storms on inflation rates (column three), growth rates of labor income (column six), and real internal rate of return (column seven), but a decreasing effect on saving rates (column two) and an increasing effect on capital stock depreciation rates (column four), both significant at the 1% level, and a decreasing effect on employment rates significant at the 10% level (column five). Note, however, in Figure 3.1b and Figure 3.1d, the timing groups—comparisons of “late” treated as treatment groups to “early” treated as control groups, and vice-versa—contribute to these significant TWFE estimates with a larger weight than that of the comparisons using the “never” treated as controls (87% versus 13% for saving and capital stock depreciation rates), and the treatment effect using the timing groups is about seven to nine times the treatment effect using the other comparisons. As for employment rates, the treatment effect also changes from the timing groups to the other comparisons, and the timing groups contribute to the average treatment effect with a weight of 89% (Figure 3.1e).

The corrected, heterogeneity-robust estimates in Table 3.2b indicate significant static effects of storms at the 10% level on saving rates (column two) and the 1% level on capital stock depreciation rates (column four), but invalidate the effects on employment rates (column five). When storm events occur, they immediately diminish the propensity to save by 0.9 percentage points and the capital stock gets depreciated at a faster rate by 0.15 percentage points.

Controlling for other disasters to avoid omitted variable bias gives a preliminary refinement of the story. As shown in Table 3.3b, the heterogeneity-robust version of Table 3.3a, the sign and magnitude of the static effect of storms on saving rates are maintained and the effect is more precise, with a significance at the 5% level (column two). As for the capital stock depreciation rates (column four), the sign and significance level of the effect are maintained, but the magnitude of the effect gets smaller (+ 0.09 percentage points).

Table 3.2: Static effects of storms on economic outcomes (non-robust to confounding disasters)

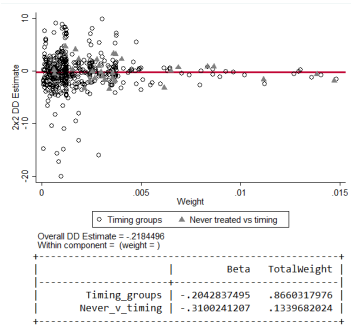
(a) Two-Way Fixed Effects estimations

VARIABLES	(1) Growth rate of GDP p.c.	(2) Saving rate	(3) Inflation rate	(4) Capital depreciation rate	(5) Employment rate	(6) Growth rate of labor income p.c.	(7) Real IRR
D_storm	-0.2184 (0.3594)	-0.0280*** (0.0038)	0.0276 (0.6745)	0.0012*** (0.0003)	-0.4244* (0.2312)	-0.6552 (0.4083)	0.0037 (0.0027)
Observations	3,588	3,588	3,588	3,588	3,312	3,036	3,036
R-squared	0.1197	0.5587	0.2039	0.7972	0.8043	0.1004	0.7702
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes

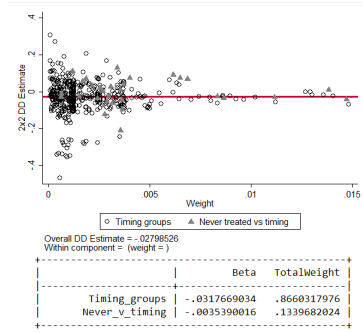
(b) Heterogeneity-robust Sun and Abraham, 2021's estimations

VARIABLES	(1) Growth rate of GDP p.c.	(2) Saving rate	(3) Inflation rate	(4) Capital depreciation rate	(5) Employment rate	(6) Growth rate of labor income p.c.	(7) Real IRR
D_storm	0.0865 (0.5084)	-0.0090* (0.0050)	0.0708 (0.9542)	0.0015*** (0.0004)	-0.0862 (0.3214)	-0.3187 (0.5910)	-0.0041 (0.0036)
Observations	3,588	3,588	3,588	3,588	3,312	3,036	3,036
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes

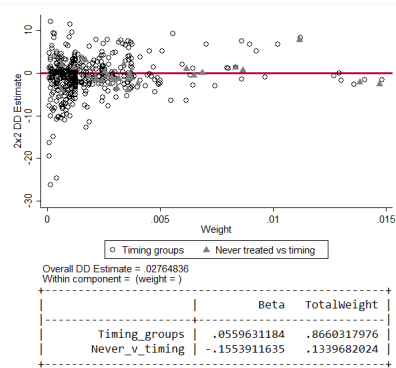
Notes: This table reports the static effect  $\gamma$  of storms using the Two-Way Fixed Effects specification in Eq.3.1 (Table 3.2a) and the heterogeneity-robust Sun and Abraham, 2021's estimator (Table 3.2b). The outcomes are: per capita GDP growth rate (percent), column (1); private saving rate, column (2); price inflation rate (percent), column (3); average depreciation rate of the capital stock, column (4); employment rate, column (5); per capita labor income growth rate (percent), column (6); and real internal rate of return, column (7). The non-missing balanced sample covers 52 countries, each with 69 years in columns (1) through (4); 48 countries, each with 69 years in column (5); 44 countries, each with 69 years in columns (6) and (7). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.



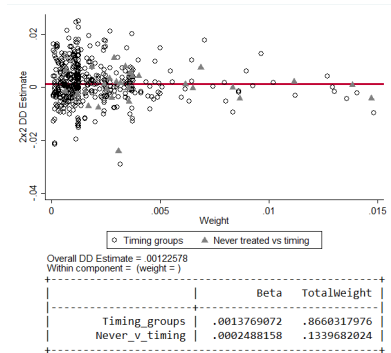
(a) Per capita GDP growth rate (percent)



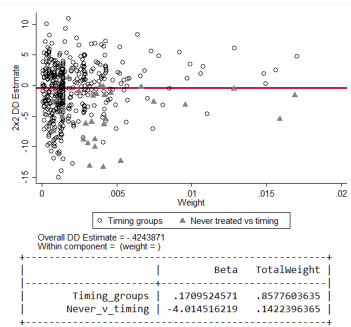
(b) Private saving rate



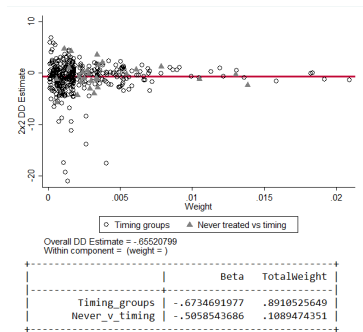
(c) Price inflation rate (percent)



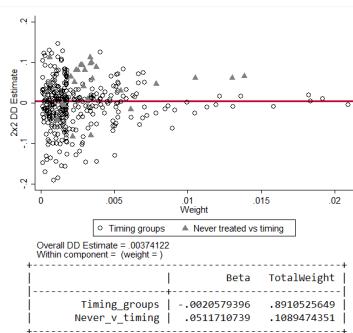
(d) Average depreciation rate of the capital stock



(e) Employment rate (percent)



(f) Per capita labor income growth rate (percent)



(g) Real internal rate of return

Figure 3.I: Goodman-Bacon, 2021's decomposition results for the static effects of storms on economic outcomes (non-robust to confounding disasters)

Table 3.3: Static effects of storms on economic outcomes (robust to confounding disasters)

(a) Two-Way Fixed Effects estimations

VARIABLES	(1) Growth rate of GDP p.c.	(2) Saving rate	(3) Inflation rate	(4) Capital depreciation rate	(5) Employment rate	(6) Growth rate of labor income p.c.	(7) Real IRR
D_storm	-0.2254 (0.3597)	-0.0277*** (0.0038)	0.0158 (0.6763)	0.0013*** (0.0003)	-0.4644** (0.2296)	-0.6409 (0.4088)	0.0035 (0.0027)
Flood occurred	0.8346*** (0.2694)	-0.0026 (0.0028)	-0.3265 (0.5065)	-0.0000 (0.0003)	-0.6198*** (0.1756)	0.9471*** (0.3079)	-0.0026 (0.0020)
Earthquake occurred	0.5277 (0.3959)	-0.0038 (0.0042)	0.0502 (0.7444)	0.0005 (0.0004)	-0.1292 (0.2576)	0.0307 (0.4611)	-0.0031 (0.0030)
Drought occurred	0.0613 (0.4245)	0.0112** (0.0045)	0.4145 (0.7982)	0.0005 (0.0004)	-0.0789 (0.2801)	-0.1295 (0.5050)	-0.0036 (0.0033)
Epidemic occurred	0.4671 (0.3943)	-0.0192*** (0.0042)	-0.7275 (0.7414)	-0.0010*** (0.0004)	-1.6201*** (0.2585)	0.1951 (0.4907)	0.0070** (0.0032)
Other disaster occurred	0.1417 (0.3029)	0.0034 (0.0032)	-0.4349 (0.5695)	0.0006** (0.0003)	-0.5400*** (0.1922)	0.5050 (0.3390)	-0.0029 (0.0022)
Observations	3,588	3,588	3,588	3,588	3,312	3,036	3,036
R-squared	0.1232	0.5626	0.2044	0.7981	0.8082	0.1042	0.7710
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(b) Heterogeneity-robust Sun and Abraham, 2021's estimations

VARIABLES	(1) Growth rate of GDP p.c.	(2) Saving rate	(3) Inflation rate	(4) Capital depreciation rate	(5) Employment rate	(6) Growth rate of labor income p.c.	(7) Real IRR
D_storm	-0.2419 (0.4173)	-0.0094** (0.0041)	-0.4517 (0.7879)	0.0009*** (0.0004)	0.4243 (0.2601)	-0.5541 (0.4613)	-0.0024 (0.0030)
Flood occurred	0.0945 (0.2981)	-0.0042 (0.0028)	0.1555 (0.5315)	0.0005* (0.0003)	-0.0702 (0.1863)	0.1263 (0.3795)	-0.0041* (0.0022)
Earthquake occurred	0.4781 (0.5106)	-0.0019 (0.0047)	0.2794 (0.9667)	0.0004 (0.0004)	0.1586 (0.3196)	0.9132 (0.6242)	0.0028 (0.0037)
Drought occurred	0.0925 (0.4280)	-0.0040 (0.0042)	-1.3744 (0.8941)	0.0002 (0.0004)	-0.2313 (0.2834)	0.0343 (0.5335)	-0.0051 (0.0033)
Epidemic occurred	0.4103 (0.4056)	-0.0123** (0.0049)	-3.8173*** (0.8213)	-0.0005 (0.0004)	-0.9070*** (0.2811)	-0.8356 (0.5528)	0.0000 (0.0032)
Other disaster occurred	-0.1788 (0.2238)	0.0021 (0.0021)	-0.4282 (0.4179)	0.0004** (0.0002)	-0.1247 (0.1335)	-0.3289 (0.2488)	-0.0016 (0.0016)
Observations	3,588	3,588	3,588	3,588	3,312	3,036	3,036
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the static effect  $\gamma$  of storms using the Two-Way Fixed Effects specification with confounders in Eq.3.2 (Table 3.3a) and the heterogeneity-robust Sun and Abraham, 2021's estimator (Table 3.3b). The outcomes are: per capita GDP growth rate (percent), column (1); private saving rate, column (2); price inflation rate (percent), column (3); average depreciation rate of the capital stock, column (4); employment rate, column (5); per capita labor income growth rate (percent), column (6); and real internal rate of return, column (7). The non-missing balanced sample covers 52 countries, each with 69 years in columns (1) through (4); 48 countries, each with 69 years in column (5); 44 countries, each with 69 years in columns (6) and (7). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

### 3.5 Robustness checks and discussion

A further refinement of the story is given in Table 3.5, with its naive TWFE counterpart in Table 3.4, by not only isolating the effect of storms from that of other disasters but also controlling for the prior trends in the macroeconomic outcomes. As indicated by the joint test of the pre-trends, treatment and control groups have common pre-trends in the more robust estimations in Table 3.5. A striking finding is the failure to reject that storms have no static effects on per capita GDP growth (column one), saving rate (column two), inflation rate (column three), depreciation rate of capital stock (column four), employment rate (column five), per capita labor income growth (column six), and real internal rate of return (column seven). The missing immediate storm effects on economic growth corroborate those of several other studies (Cunado & Ferreira, 2014; Fomby et al., 2013; Loayza et al., 2012). Regarding inflation rates, other studies also find no response to natural disasters (Albala-Bertrand, 1993; Noy, 2009; Ramcharan, 2007). On the rates of saving, capital stock depreciation, and employment, the more robust results suggest that the earlier results are driven by the pre-trends in outcomes and confounding disasters.

Are other disaster types storm-like? Turning to the comparison of storms with the other disasters, estimations that are robust to heterogeneity, confounders, and pre-trends, are presented for per capita GDP growth, saving rate, inflation rate, depreciation rate of capital stock, employment rate, per capita labor income growth, and real internal rate of return (Table B.1 through Table B.7). Summary figures point to no evidence that any of the studied disasters has a static effect on per capita GDP growth (Figure 3.2), inflation rate (Figure 3.4), employment rate (Figure 3.6), and per capita labor income growth (Figure 3.7). However, floods lead to a faster depreciation rate of capital stock by 0.18 percentage points (Figure 3.5b) and rising saving rates by 2.30 percentage points (Figure 3.3b), a result newly encountered in the empirical literature. Earthquakes also lead to a faster depreciation rate of capital stock by 0.30 pp (Figure 3.5f), and given their severity, 1.7 times faster than floods. Epidemics lead to a declining real internal rate of return of businesses by 1.87 pp (Figure 3.8j), suggesting an immediate drop in the performance of enterprises in times of epidemics.

Table 3.4: Static two-way fixed effects of storms on economic outcomes (robust to confounding disasters and pre-trends)

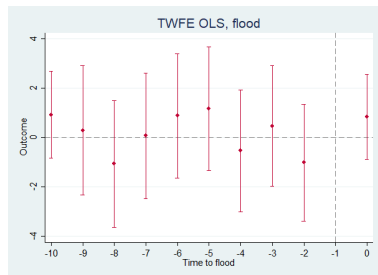
VARIABLES	(1) Growth rate of GDP p.c.	(2) Saving rate	(3) Inflation rate	(4) Capital depreciation rate	(5) Employment rate	(6) Growth rate of labor income p.c.	(7) Real IRR
lead10	0.3106 (0.8976)	0.0226** (0.0094)	0.4176 (1.6889)	-0.0007 (0.0008)	0.4686 (0.5796)	0.4572 (1.0121)	-0.0111* (0.0066)
lead9	-0.7201 (1.2415)	-0.0016 (0.0130)	-1.1580 (2.3359)	-0.0012 (0.0012)	-0.0111 (0.8062)	-0.1192 (1.4055)	-0.0003 (0.0092)
lead8	2.1949* (1.2302)	-0.0013 (0.0129)	1.1478 (2.3147)	-0.0012 (0.0011)	-0.1491 (0.8000)	2.3799* (1.3949)	0.0041 (0.0091)
lead7	0.5820 (1.2351)	-0.0004 (0.0130)	-0.3852 (2.3238)	-0.0011 (0.0012)	-0.2849 (0.8014)	0.9238 (1.3964)	0.0101 (0.0091)
lead6	0.6829 (1.2223)	-0.0027 (0.0128)	2.4315 (2.2998)	-0.0011 (0.0011)	-0.2641 (0.7937)	0.3499 (1.3819)	0.0147 (0.0090)
lead5	0.4827 (1.2258)	-0.0087 (0.0129)	0.7625 (2.3063)	-0.0007 (0.0011)	-0.3829 (0.7960)	1.0922 (1.3879)	0.0073 (0.0091)
lead4	1.3149 (1.2224)	-0.0067 (0.0128)	2.0456 (2.2999)	-0.0004 (0.0011)	-0.2721 (0.7930)	1.6595 (1.3804)	0.0052 (0.0090)
lead3	-0.6169 (1.2136)	-0.0044 (0.0128)	2.0003 (2.2835)	-0.0001 (0.0011)	-0.1114 (0.7875)	-0.5009 (1.3708)	0.0036 (0.0089)
lead2	1.3870 (1.2180)	0.0008 (0.0128)	-0.0579 (2.2918)	0.0001 (0.0011)	-0.2035 (0.7907)	1.5122 (1.3770)	0.0031 (0.0090)
D_storm	0.2570 (0.8761)	-0.0214** (0.0092)	0.6661 (1.6484)	0.0006 (0.0008)	-0.4080 (0.5670)	0.0275 (0.9876)	0.0030 (0.0064)
Flood occurred	0.8510*** (0.2700)	-0.0036 (0.0028)	-0.3009 (0.5079)	-0.0000 (0.0003)	-0.6462*** (0.1760)	0.9721*** (0.3091)	-0.0018 (0.0020)
Earthquake occurred	0.5122 (0.3963)	-0.0046 (0.0042)	0.0558 (0.7436)	0.0005 (0.0004)	-0.1493 (0.2580)	0.0043 (0.4618)	-0.0026 (0.0030)
Drought occurred	0.0702 (0.4251)	0.0098** (0.0045)	0.4448 (0.7998)	0.0005 (0.0004)	-0.1130 (0.2806)	-0.1171 (0.5057)	-0.0028 (0.0033)
Epidemic occurred	0.4570 (0.3945)	-0.0185*** (0.0041)	-0.7434 (0.7424)	-0.0010*** (0.0004)	-1.6018*** (0.2588)	0.2105 (0.4911)	0.0074** (0.0032)
Other disaster occurred	0.1473 (0.3032)	0.0027 (0.0032)	-0.4259 (0.5705)	0.0006** (0.0003)	-0.5633*** (0.1925)	0.5172 (0.3396)	-0.0024 (0.0022)
Observations	3,588	3,588	3,588	3,588	3,312	3,036	3,036
R-squared	0.1257	0.5671	0.2055	0.7982	0.8086	0.1066	0.7737
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	0.370	3.89e-05	0.866	0.951	0.669	0.559	9.46e-05

Notes: This table reports the static effect  $\gamma$  of storms using the Two-Way Fixed Effects specification with confounders and pre-trends in Eq.3.3. The outcomes are: per capita GDP growth rate (percent), column (1); private saving rate, column (2); price inflation rate (percent), column (3); average depreciation rate of the capital stock, column (4); employment rate, column (5); per capita labor income growth rate (percent), column (6); and real internal rate of return, column (7). The non-missing balanced sample covers 52 countries, each with 69 years in columns (1) through (4); 48 countries, each with 69 years in column (5); 44 countries, each with 69 years in columns (6) and (7). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

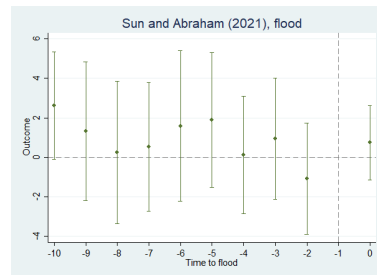
Table 3.5: Static effects of storms on economic outcomes (robust to heterogeneity in treatment effects, confounding disasters, and pre-trends)

VARIABLES	(1) Growth rate of GDP p.c.	(2) Saving rate	(3) Inflation rate	(4) Capital depreciation rate	(5) Employment rate	(6) Growth rate of labor income p.c.	(7) Real IRR
lead10	-1.1925 (1.2322)	-0.0048 (0.0117)	0.5944 (2.3700)	-0.0006 (0.0010)	-0.4346 (0.7324)	-0.6647 (1.3446)	-0.0089 (0.0080)
lead9	-0.8534 (1.5705)	-0.0052 (0.0139)	-1.5282 (2.9192)	-0.0016 (0.0012)	-0.3156 (0.8518)	-0.8083 (1.8266)	0.0007 (0.0120)
lead8	1.4090 (1.6125)	-0.0096 (0.0140)	1.4262 (3.1388)	-0.0015 (0.0012)	-0.2322 (0.8455)	1.6853 (1.8187)	0.0055 (0.0108)
lead7	0.1950 (1.4875)	-0.0056 (0.0136)	-0.5153 (2.7076)	-0.0014 (0.0012)	-0.3414 (0.8395)	0.0940 (1.6672)	0.0100 (0.0110)
lead6	0.4751 (1.5758)	-0.0062 (0.0131)	1.1107 (2.8643)	-0.0013 (0.0012)	-0.3592 (0.8022)	-0.1336 (1.6435)	0.0137 (0.0116)
lead5	0.4214 (1.5490)	-0.0105 (0.0135)	-0.0056 (2.8854)	-0.0012 (0.0011)	-0.2691 (0.8254)	-0.2691 (1.7708)	0.0066 (0.0107)
lead4	0.9560 (1.4762)	-0.0057 (0.0159)	2.1618 (2.7718)	-0.0007 (0.0011)	-0.2356 (0.7939)	1.5753 (1.6385)	0.0017 (0.0093)
lead3	-0.6780 (1.4701)	-0.0046 (0.0134)	1.3071 (2.8415)	-0.0004 (0.0011)	0.0967 (0.7903)	-0.8326 (1.6726)	0.0036 (0.0091)
lead2	1.4064 (1.5389)	0.0023 (0.0122)	-0.7866 (2.7764)	-0.0002 (0.0011)	0.0840 (0.7918)	1.5855 (1.7357)	0.0042 (0.0091)
D_storm	0.0259 (0.9826)	-0.0140 (0.0092)	-0.7331 (1.8909)	-0.0000 (0.0008)	0.2002 (0.5988)	-0.2754 (1.0821)	0.0039 (0.0063)
Flood occurred	0.0080 (0.3171)	-0.0037 (0.0030)	0.1370 (0.5723)	0.0005* (0.0003)	-0.0069 (0.2005)	0.0122 (0.4006)	-0.0046** (0.0023)
Earthquake occurred	0.4871 (0.5724)	-0.0013 (0.0053)	0.2440 (1.1080)	0.0004 (0.0005)	0.2525 (0.3627)	0.8788 (0.6864)	0.0038 (0.0040)
Drought occurred	0.0958 (0.4623)	-0.0043 (0.0044)	-1.4262 (0.9595)	0.0003 (0.0004)	-0.2475 (0.3105)	0.0654 (0.5622)	-0.0050 (0.0034)
Epidemic occurred	0.3080 (0.4400)	-0.0134** (0.0052)	-3.7942*** (0.8921)	-0.0001 (0.0004)	-0.7939*** (0.2930)	-1.1324** (0.5728)	0.0008 (0.0031)
Other disaster occurred	-0.2373 (0.2369)	0.0016 (0.0022)	-0.5039 (0.4469)	0.0004* (0.0002)	-0.1309 (0.1433)	-0.4103 (0.2655)	-0.0015 (0.0017)
Observations	3,588	3,588	3,588	3,588	3,312	3,036	3,036
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	0.933	0.990	0.996	0.545	0.999	0.932	0.874

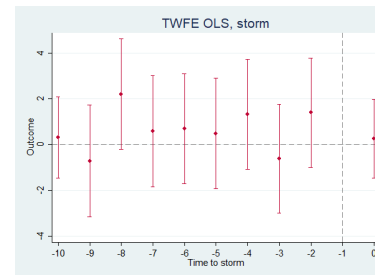
Notes: This table reports the heterogeneity-robust Sun and Abraham, 2021's estimator for the static effect  $\gamma$  of storms using the specification with confounders and pre-trends in Eq.3.3. The outcomes are: per capita GDP growth rate (percent), column (1); private saving rate, column (2); price inflation rate (percent), column (3); average depreciation rate of the capital stock, column (4); employment rate, column (5); per capita labor income growth rate (percent), column (6); and real internal rate of return, column (7). The non-missing balanced sample covers 52 countries, each with 69 years in columns (1) through (4); 48 countries, each with 69 years in column (5); 44 countries, each with 69 years in columns (6) and (7). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.



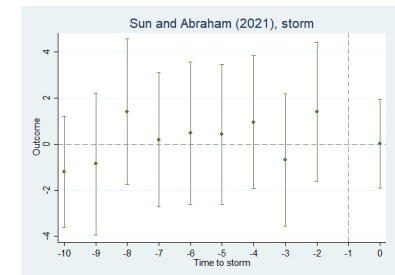
(a) Two-way fixed effects OLS estimator, floods



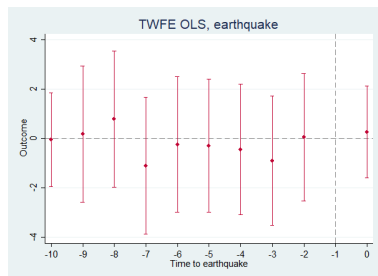
(b) Sun and Abraham, 2021's estimator, floods



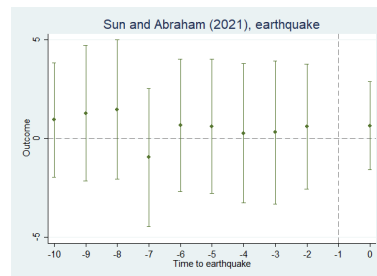
(c) Two-way fixed effects OLS estimator, storms



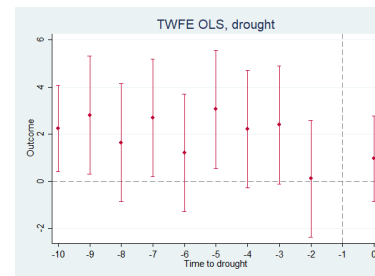
(d) Sun and Abraham, 2021's estimator, storms



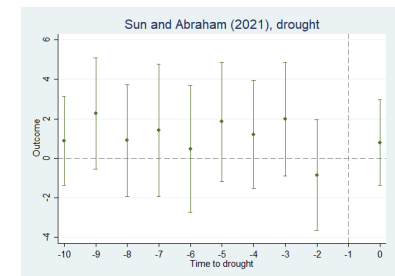
(e) Two-way fixed effects OLS estimator, earthquakes



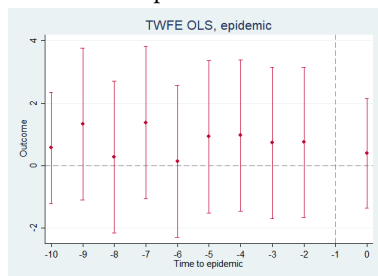
(f) Sun and Abraham, 2021's estimator, earthquakes



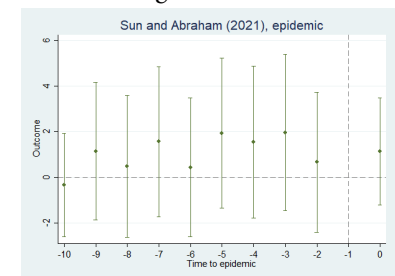
(g) Two-way fixed effects OLS estimator, droughts



(h) Sun and Abraham, 2021's estimator, droughts

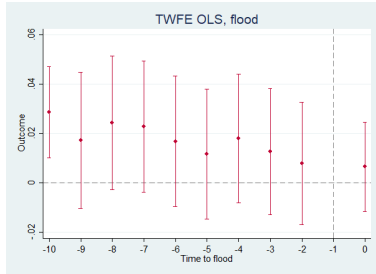


(i) Two-way fixed effects OLS estimator, epidemics

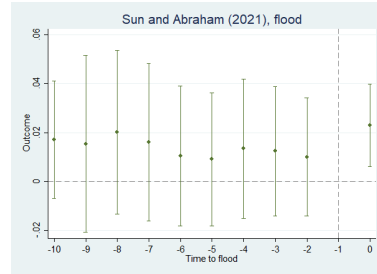


(j) Sun and Abraham, 2021's estimator, epidemics

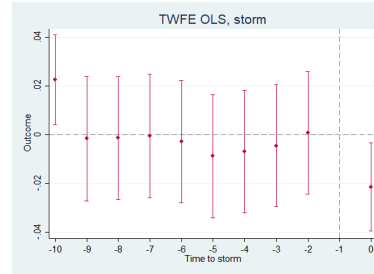
Figure 3.2: Average causal effect of natural disasters on per capita GDP growth rate (percent)



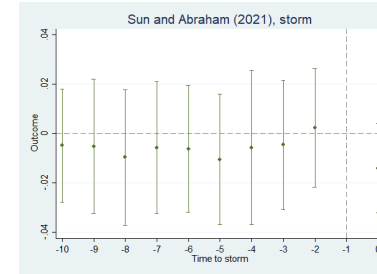
(a) Two-way fixed effects OLS estimator, floods



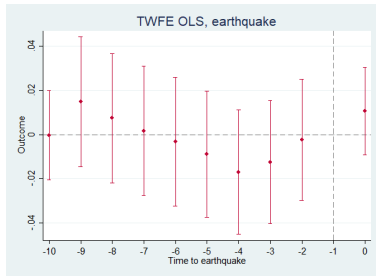
(b) Sun and Abraham, 2021's estimator, floods



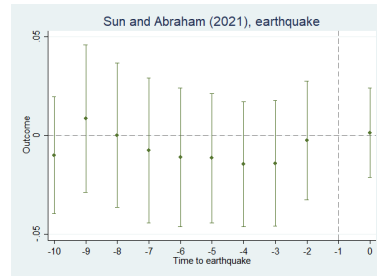
(c) Two-way fixed effects OLS estimator, storms



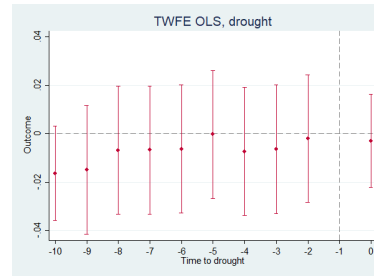
(d) Sun and Abraham, 2021's estimator, storms



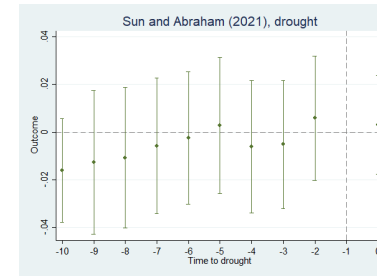
(e) Two-way fixed effects OLS estimator, earthquakes



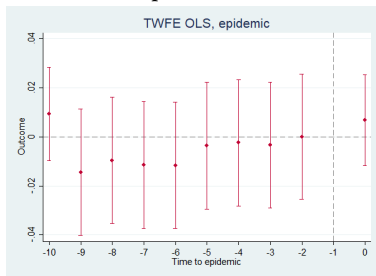
(f) Sun and Abraham, 2021's estimator, earthquakes



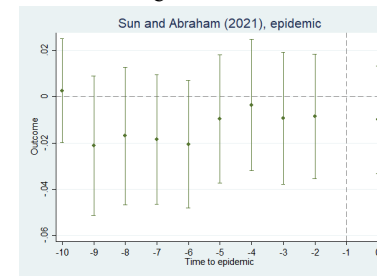
(g) Two-way fixed effects OLS estimator, droughts



(h) Sun and Abraham, 2021's estimator, droughts

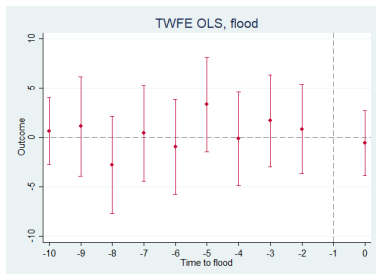


(i) Two-way fixed effects OLS estimator, epidemics

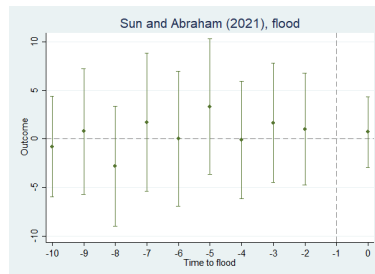


(j) Sun and Abraham, 2021's estimator, epidemics

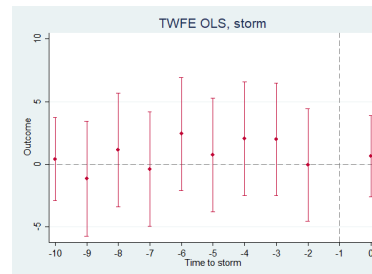
Figure 3.3: Average causal effect of natural disasters on private saving rate



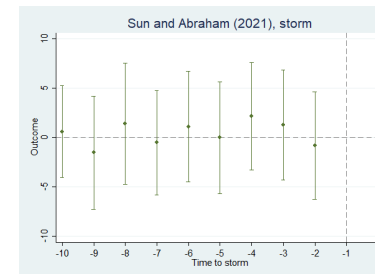
(a) Two-way fixed effects OLS estimator, floods



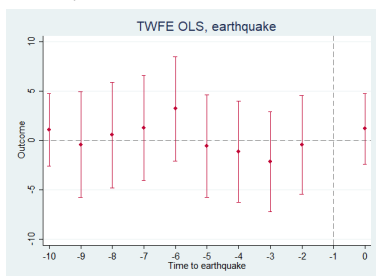
(b) Sun and Abraham, 2021's estimator, floods



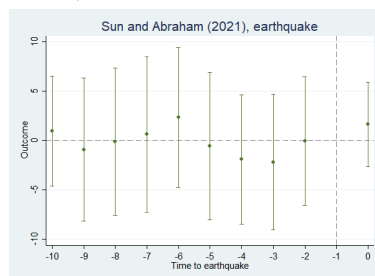
(c) Two-way fixed effects OLS estimator, storms



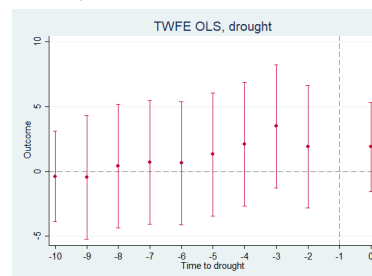
(d) Sun and Abraham, 2021's estimator, storms



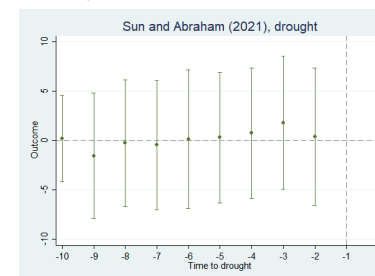
(e) Two-way fixed effects OLS estimator, earthquakes



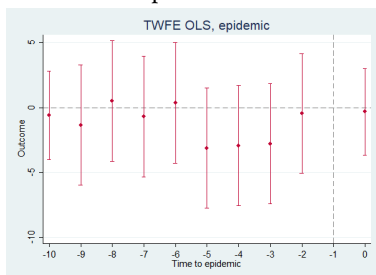
(f) Sun and Abraham, 2021's estimator, earthquakes



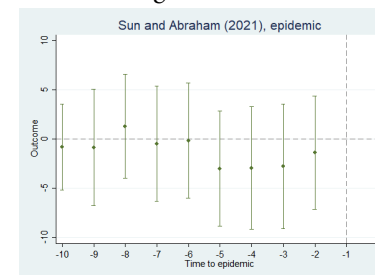
(g) Two-way fixed effects OLS estimator, droughts



(h) Sun and Abraham, 2021's estimator, droughts

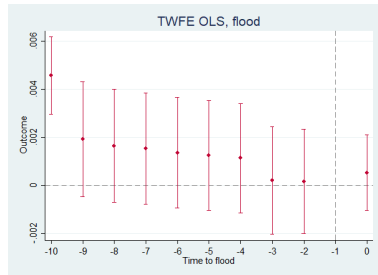


(i) Two-way fixed effects OLS estimator, epidemics

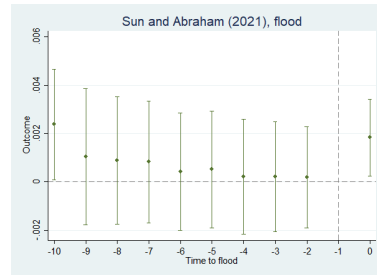


(j) Sun and Abraham, 2021's estimator, epidemics

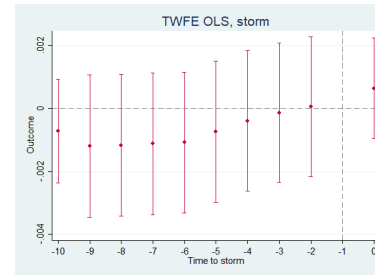
Figure 3.4: Average causal effect of natural disasters on annual price inflation rate (percent)



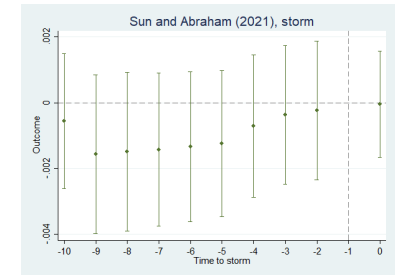
(a) Two-way fixed effects OLS estimator, floods



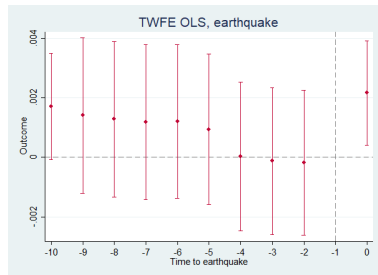
(b) Sun and Abraham, 2021's estimator, floods



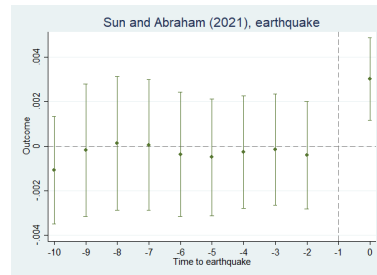
(c) Two-way fixed effects OLS estimator, storms



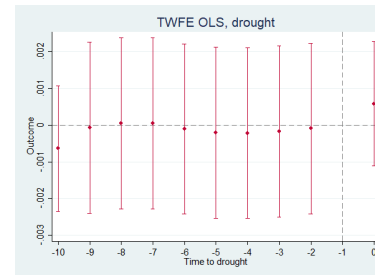
(d) Sun and Abraham, 2021's estimator, storms



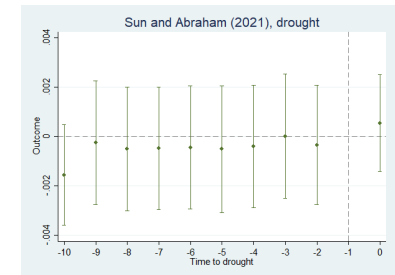
(e) Two-way fixed effects OLS estimator, earthquakes



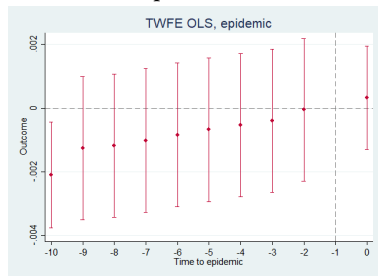
(f) Sun and Abraham, 2021's estimator, earthquakes



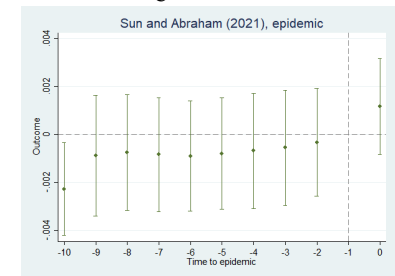
(g) Two-way fixed effects OLS estimator, droughts



(h) Sun and Abraham, 2021's estimator, droughts

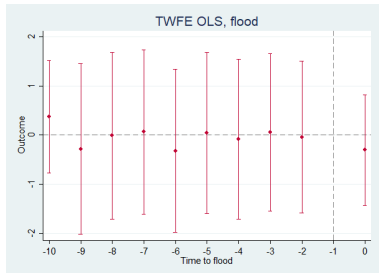


(i) Two-way fixed effects OLS estimator, epidemics

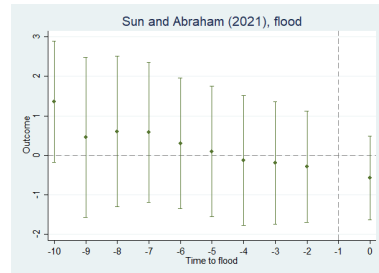


(j) Sun and Abraham, 2021's estimator, epidemics

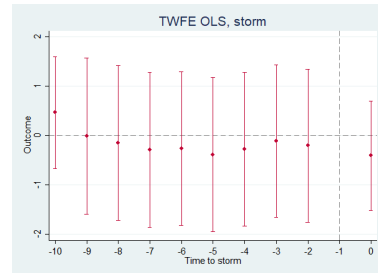
Figure 3.5: Average causal effect of natural disasters on the average depreciation rate of the capital stock



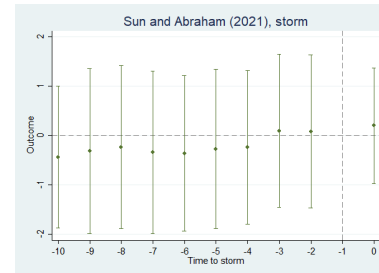
(a) Two-way fixed effects OLS estimator, floods



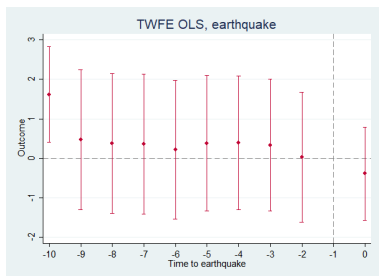
(b) Sun and Abraham, 2021's estimator, floods



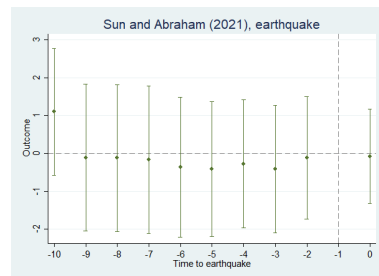
(c) Two-way fixed effects OLS estimator, storms



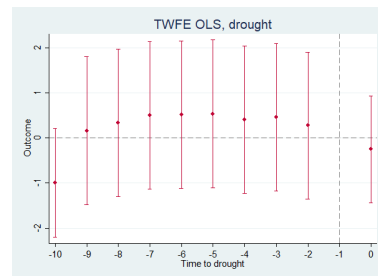
(d) Sun and Abraham, 2021's estimator, storms



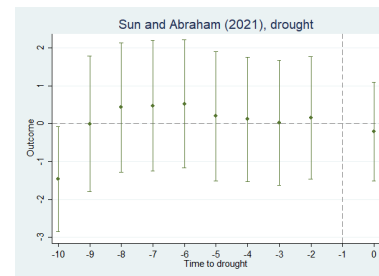
(e) Two-way fixed effects OLS estimator, earthquakes



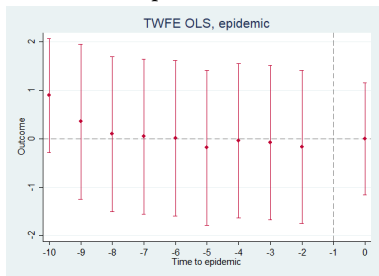
(f) Sun and Abraham, 2021's estimator, earthquakes



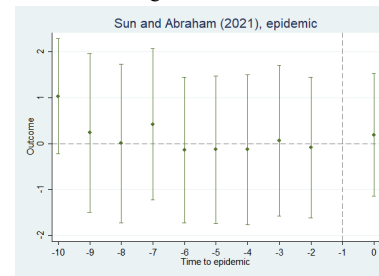
(g) Two-way fixed effects OLS estimator, droughts



(h) Sun and Abraham, 2021's estimator, droughts

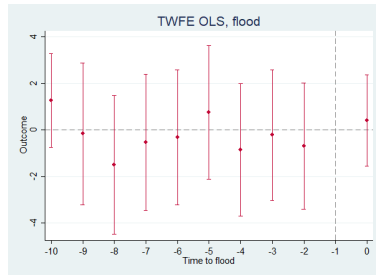


(i) Two-way fixed effects OLS estimator, epidemics

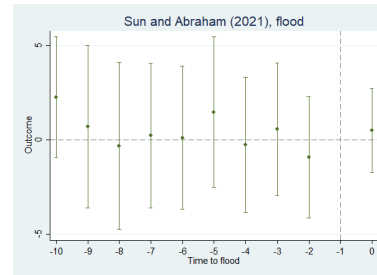


(j) Sun and Abraham, 2021's estimator, epidemics

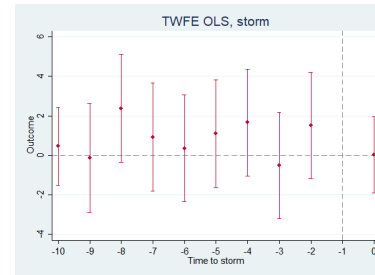
Figure 3.6: Average causal effect of natural disasters on employment rate (percent)



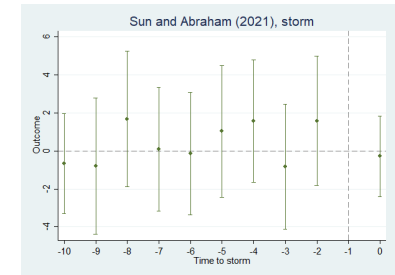
(a) Two-way fixed effects OLS estimator, floods



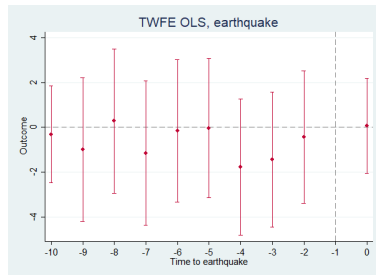
(b) Sun and Abraham, 2021's estimator, floods



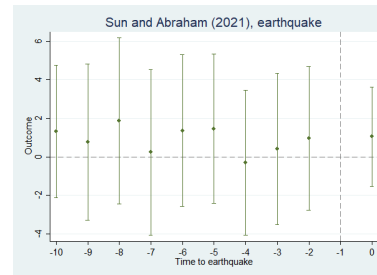
(c) Two-way fixed effects OLS estimator, storms



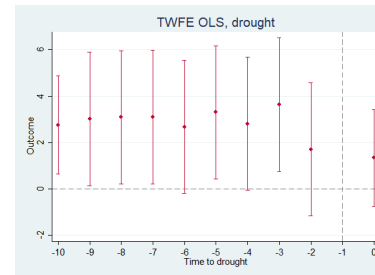
(d) Sun and Abraham, 2021's estimator, storms



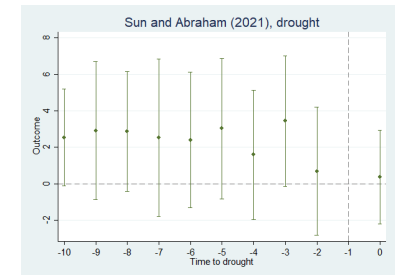
(e) Two-way fixed effects OLS estimator, earthquakes



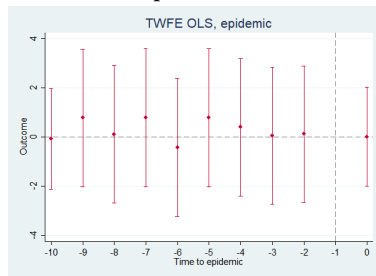
(f) Sun and Abraham, 2021's estimator, earthquakes



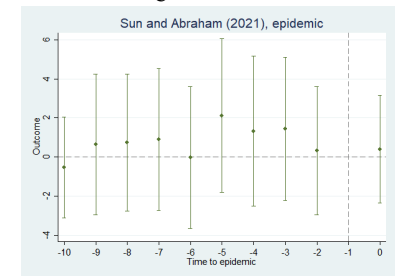
(g) Two-way fixed effects OLS estimator, droughts



(h) Sun and Abraham, 2021's estimator, droughts

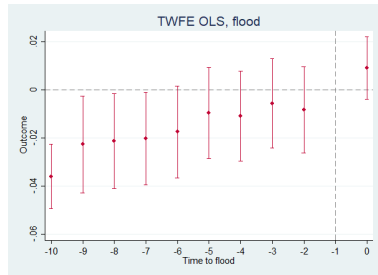


(i) Two-way fixed effects OLS estimator, epidemics

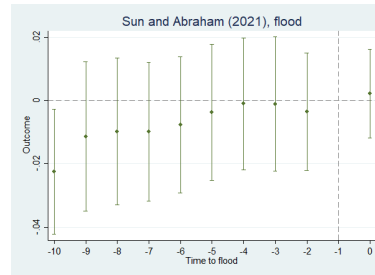


(j) Sun and Abraham, 2021's estimator, epidemics

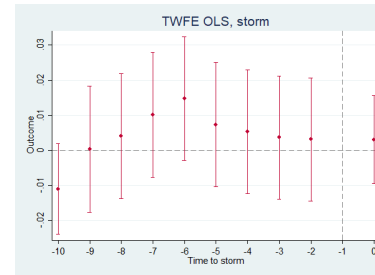
Figure 3.7: Average causal effect of natural disasters on the annual growth rate of per capita real labor income (percent)



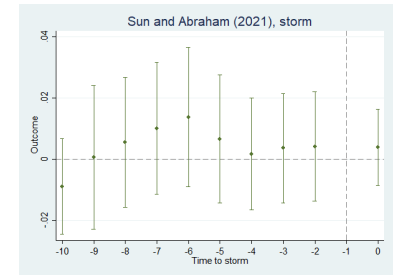
(a) Two-way fixed effects OLS estimator, floods



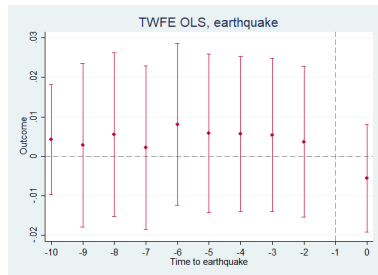
(b) Sun and Abraham, 2021's estimator, floods



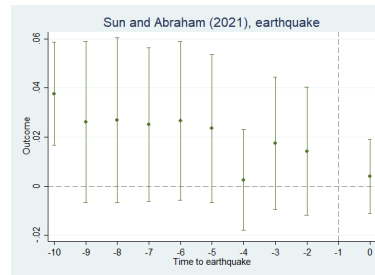
(c) Two-way fixed effects OLS estimator, storms



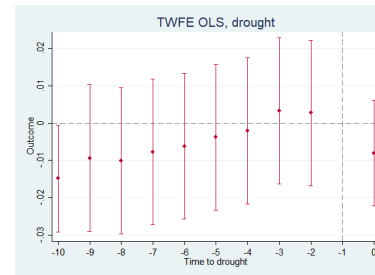
(d) Sun and Abraham, 2021's estimator, storms



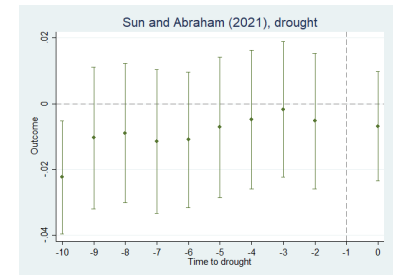
(e) Two-way fixed effects OLS estimator, earthquakes



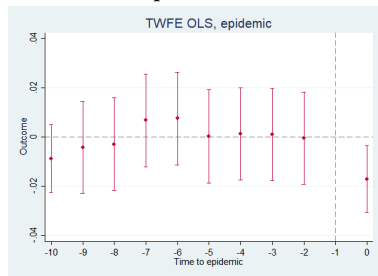
(f) Sun and Abraham, 2021's estimator, earthquakes



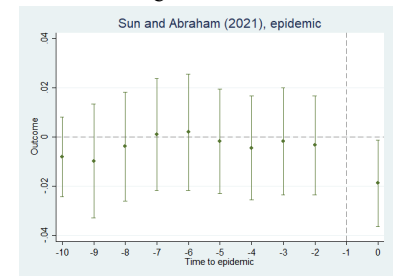
(g) Two-way fixed effects OLS estimator, droughts



(h) Sun and Abraham, 2021's estimator, droughts



(i) Two-way fixed effects OLS estimator, epidemics



(j) Sun and Abraham, 2021's estimator, epidemics

Figure 3.8: Average causal effect of natural disasters on the real internal rate of return

### 3.6 Conclusion and caveats

This paper highlights descriptive evidence about the static impact of natural disasters on macroeconomic outcomes. Findings that are robust to confounders, pre-trends in outcomes, and heterogeneity in treatment effects, suggest no immediate impact on economic growth, whatsoever the studied disaster (storms, floods, earthquakes, droughts, and epidemics). The absence of immediate impact of storms and droughts is also underscored on the following macroeconomic outcomes: saving rate, inflation rate, depreciation rate of capital stock, employment rate, per capita labor income growth, and real internal rate of return. In contrast, the depreciation of capital stock is worsened by 0.30 percentage points in the wake of earthquakes, and the magnitude of this effect is almost double of that of floods. The flood-induced propensity to save rises by about 2 percentage points, and the real internal rate of return of economic activity goes down approximately by the same magnitude when epidemics strike a country in a given year.

While the paper proceeds methodically to improve the robustness of contemporaneous disaster effects in the literature, there are four main limitations to recognize for this study. First, the paper addresses neither the dynamic post-disaster effects—to differentiate itself from prior studies—nor the impact mechanisms—because it puts an emphasis on providing some descriptive evidence base for static effects. Second, the estimations conducted in this paper do not control for macroeconomic variables that are determinants of growth because these variables can be endogenous or viewed as additional outcomes rather than controls, given the descriptive extent of the paper. Third, the study limits the number of outcomes that are assessed to a few ones. It is possible that significant immediate effects could be caught should other macroeconomic outcomes be studied using the same methodology. Last, to make the paper shorter and simpler, the study does not present heterogeneous effects by country's economic development status, sector of the economy, and the intensity of the disaster of interest, which could bring additional insights to the descriptive evidence.

CHAPTER 4  
THIRD ESSAY

SAVING FOR A RAINY DAY: THE DYNAMIC IMPACT  
OF STORMS ON SAVING RATES<sup>1</sup>

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<sup>1</sup>Mensah, E. R. and M. J. Filipski, "Impact of natural disasters on the economy," to be submitted to *Nature Climate Change*.

## 4.1 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 type 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.

*JEL* Codes: D81, Q54, E21, C23.

## 4.2 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.<sup>2</sup> 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 & 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

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<sup>2</sup>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%.

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 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; Baiardi et al., 2014, 2020; Eeckhoudt & 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 & Mobarak, 2011; Noy & 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 & 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 & Schündeln, 2005).<sup>3</sup> 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 & d’Haultfoeuille, 2020a; Sun & 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

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<sup>3</sup>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 & Shah, 2015; Cassar et al., 2017; Eckel et al., 2009; Hanaoka et al., 2018; Page et al., 2014; Reynaud et al., 2013).

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 type (“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 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) rising inflation which could make consumption costlier to afford, and thus, diminish savings levels; (3) GDP growth which could decrease saving rates, independently of falling 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 & 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 & Sant’Anna, 2021; Goodman-Bacon, 2021; Sun & 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 & d’Haultfoeuille, 2020a) and not relying on the estimators for staggered treatment timing.

The rest of the paper is organized as follows. Section 4.3 describes the data. Section 4.4 exposes the estimation methods. Section 4.5 presents the treatment effects on saving rates across all countries. Section 4.6 provides a discussion of the possible mechanisms behind the results, with further checks in Section 4.7. Section 4.8 concludes.

### 4.3 Data

We merge population and macroeconomic 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.<sup>4</sup> 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 variables (for GDP, inflation, and capital stock) and exclude countries which experienced storms in 1951,<sup>5</sup> leading to a study sample of 9,856 country-year observations covering 176 countries over the period 1951 through 2019.

Table 4.1 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 4.1).

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<sup>4</sup>Data of PWT 9.1, as available on [www.gdpc.net/pwt](http://www.gdpc.net/pwt). EM-DAT database (<http://www.emdat.be/database>).

<sup>5</sup>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.

Table 4.1: Summary statistics of demographic, economic, and disaster characteristics

	Obs	Min	Mean	SD	Max
<b>Panel A: Demographic and economic characteristics</b>					
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
<b>Panel B: Disaster characteristics</b>					
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.

## Saving rates in the last thirty years, 1990-2019

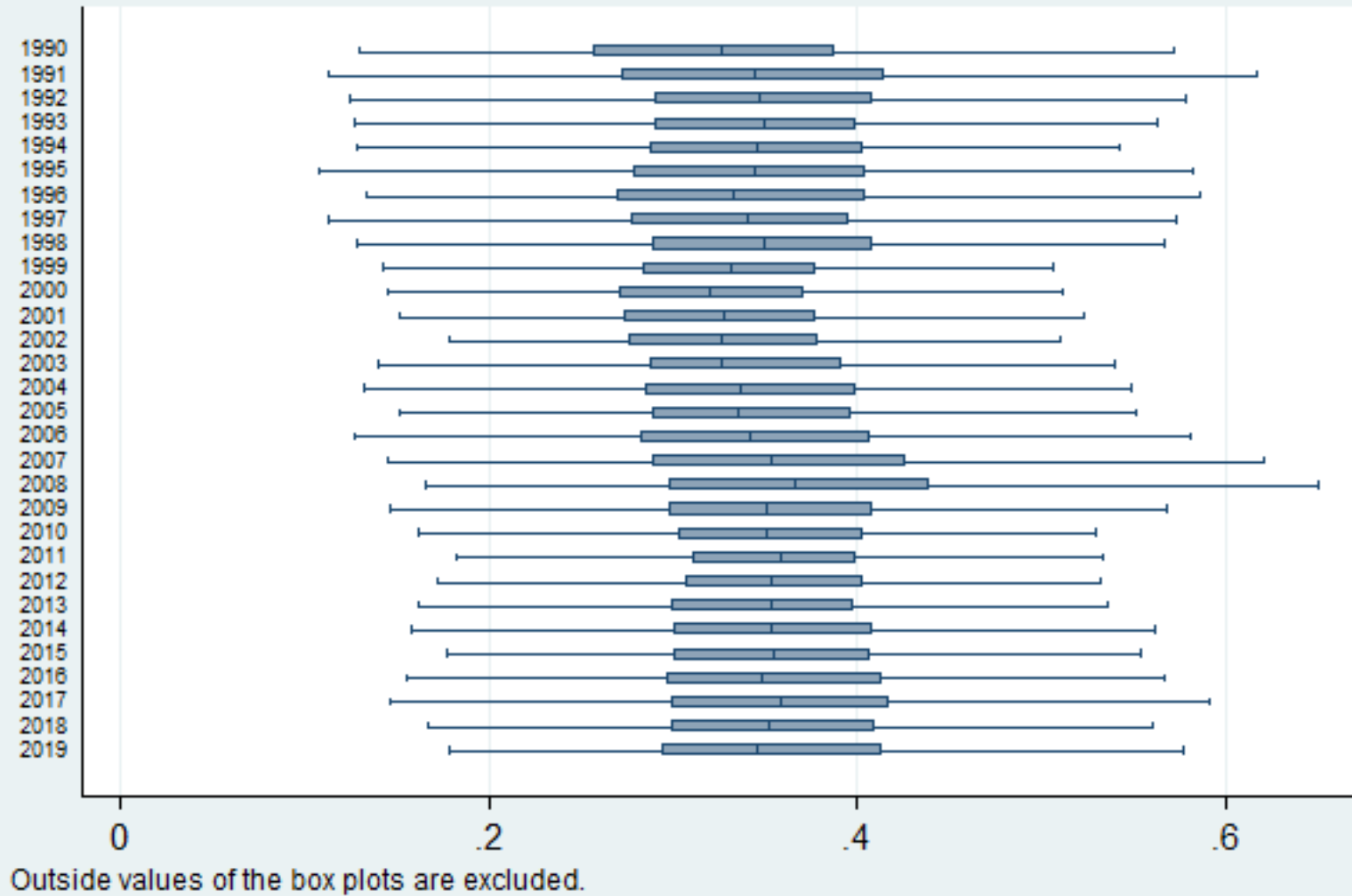


Figure 4.1: Saving rates in the last thirty years (1990-2019)

## Saving rates by storm occurrence groups in the last thirty years, 1990-2019

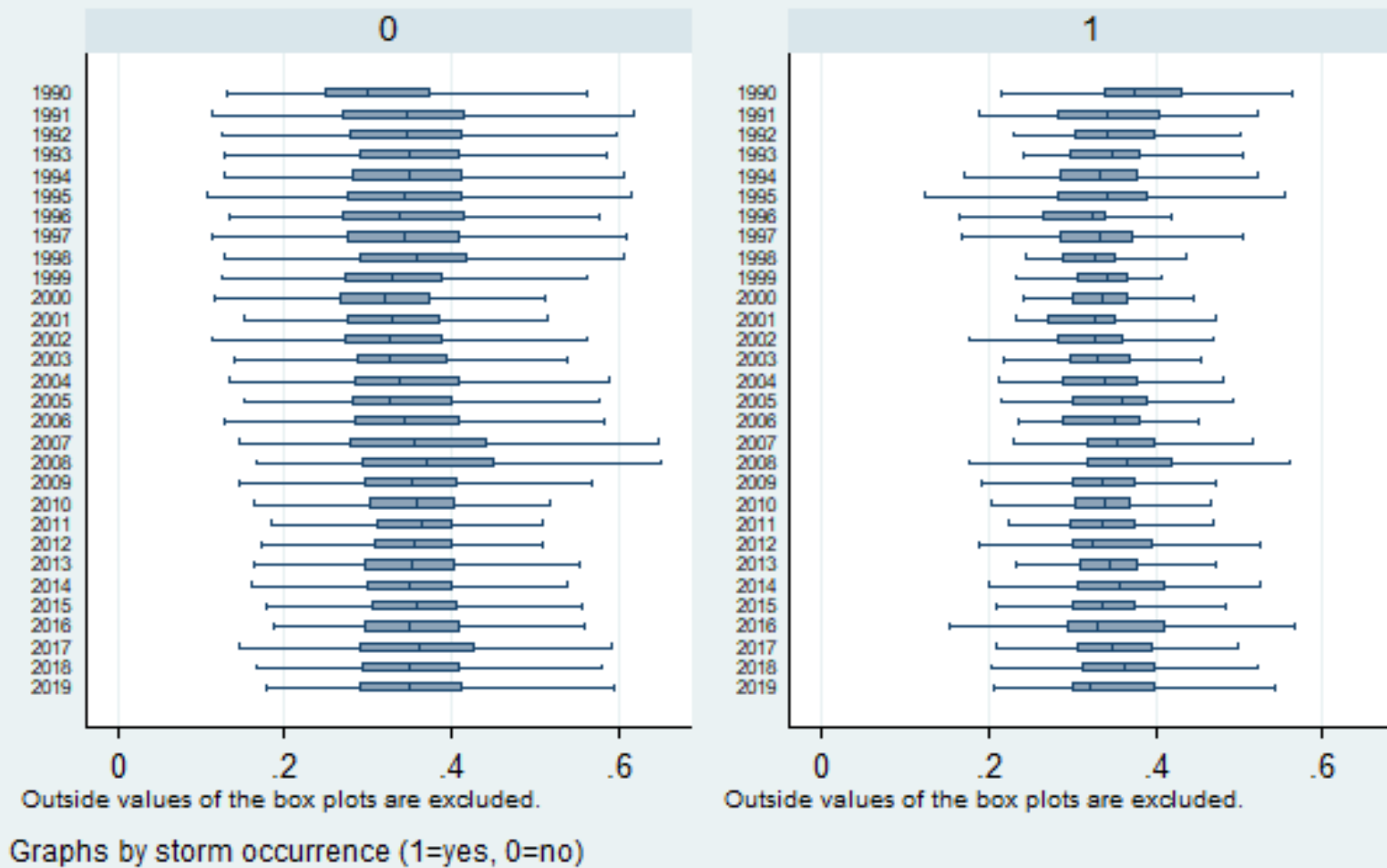


Figure 4.2: Saving rates in the last thirty years (1990-2019), by storm occurrence groups

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 C.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 4.2 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 C.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 C.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 C.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 (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.

## 4.4 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 & Sant'Anna, 2021; Goodman-Bacon,

2021; Sun & 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  $i \in \{1, \dots, N\}$  and  $t \in \{1, \dots, T\}$ . 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:

$$O_{i,t} = \alpha_i + \beta_t + \sum_{k=-H}^{-2} \delta_k \mathbf{I}[K_{i,t} = k] + \sum_{k=0}^L \gamma_k \mathbf{I}[K_{i,t} = k] + \varepsilon_{i,t}, \quad H, L \leq T. \quad (4.1)$$

The specification in Eq.4.1 illustrates a regression of country-year saving rates  $O_{i,t}$  using country fixed effects  $\alpha_i$  and year fixed effects  $\beta_t$ . The coefficients  $\delta_k$  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  $\gamma_k$  corresponding to the time periods following the treatment (“lags”) indicate the incidence on saving rates  $k$  years after the disaster occurs. The coefficient  $\gamma_0$  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” ( $\delta_{-1}$ ), which is omitted in Eq.4.1.

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 4.1). Hence, we estimate Eq.4.2 which is Eq.4.1 using the time window  $[-5, 5]$  and binning leads

and lags at the new endpoints  $H$  and  $L$ .<sup>6</sup>

$$O_{i,t} = \alpha_i + \beta_t + \delta_{-H} \mathbf{I}[K_{i,t} \leq -H] + \sum_{k=-H+1}^{-2} \delta_k \mathbf{I}[K_{i,t} = k] + \sum_{k=0}^{L-1} \gamma_k \mathbf{I}[K_{i,t} = k] + \gamma_L \mathbf{I}[K_{i,t} \geq L] + \varepsilon_{i,t}. \quad (4.2)$$

Under standard assumptions of parallel trends,<sup>7</sup> no anticipatory effects,<sup>8</sup> and homogeneity of treatment effects,<sup>9</sup> 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  $\delta_k$  are not valid, and so is the parallel trends test of their joint insignificance (Sun & Abraham, 2021).

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<sup>6</sup>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.

<sup>7</sup>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  $\delta_k$  in the time window  $[-H, -2]$  are jointly insignificant in Eq.4.2. 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 & 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.

<sup>8</sup>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.

<sup>9</sup>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 & d’Haultfoeuille, 2020a; Goodman-Bacon, 2021; Sun & Abraham, 2021), the coefficients  $\delta_k$  and  $\gamma_k$  in Eq.4.2 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 “late” treated to “early” treated countries (“forbidden comparisons”) creates contamination in the treatment effects (Goodman-Bacon, 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 & D’Haultfoeuille, 2022; Roth et al., 2022). Due to subtle differences among those estimators, 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 & Sant’Anna, 2021; De Chaisemartin & d’Haultfoeuille, 2020b; Gardner, 2021; Sun & 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  $\gamma_k$  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  $\gamma_k$  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  $\gamma_k$  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  $\delta_k$  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  $\delta_k$  represents a robust test of the parallel trends assumption, these placebo estimates provide an evaluation *ex-ante* of future treatment (De Chaisemartin & 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 type 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.

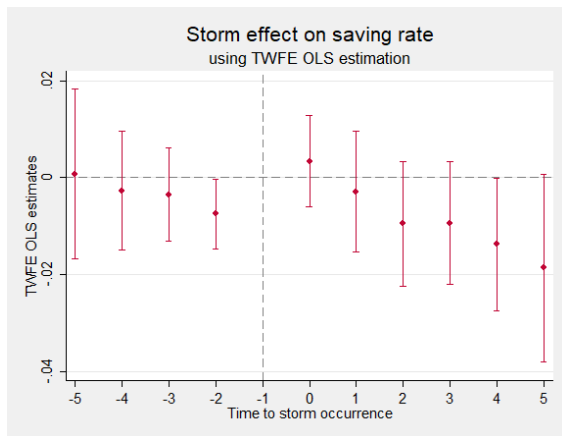
## 4.5 Results

### 4.5.1 Naive results: TWFE on saving rates and heterogeneity-robust effects for staggered treatment timing

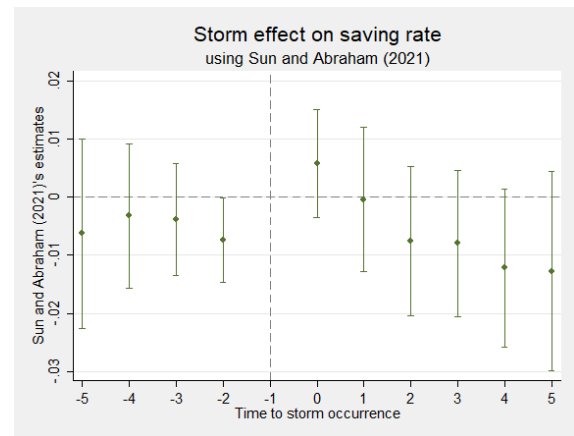
Figure 4.3a presents the storm (all inclusive) event study plot for TWFE OLS, with its counterpart plot correcting for heterogeneity in treatment effects using Sun and Abraham, 2021’s estimator in Figure 4.3b. The estimated coefficients are shown in columns 1-2 of Table 4.2. There are two salient patterns 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 turns on and off (De Chaisemartin & d’Haultfoeuille, 2020a).

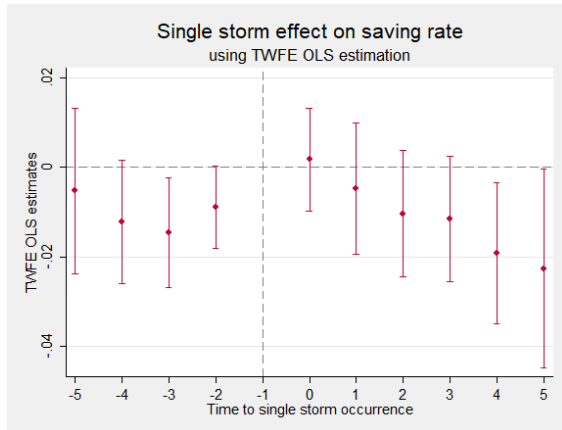
The country-year comparisons of single storms to non-storms using the naive TWFE OLS estimator (column 3 of Table 4.2 and Figure 4.3c) and Sun and Abraham, 2021’s estimator (column 4 of Table 4.2 and Figure 4.3d) indicate a significant reduction in saving rates by 1.7 to 1.9 percentage points in the 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 4.2).



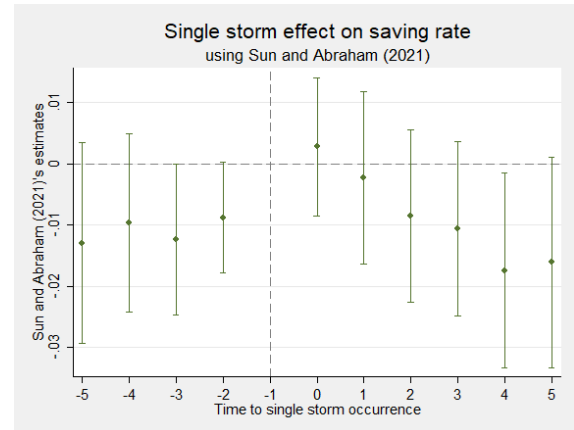
(a) Two-way fixed effects OLS estimator, all storms



(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 4.3: Staggered treatment estimators for the average causal effect of storms on saving rates  
*Notes:* In Figure 4.3a and Figure 4.3b, the treatment is any storm. In Figure 4.3c and Figure 4.3d, the treatment is restricted to storms that occur as the sole natural disaster type in the year (single storms).

Table 4.2: 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 (0.009)	-0.006 (0.008)	-0.005 (0.009)	-0.013 (0.008)
storm_lead4	-0.003 (0.006)	-0.003 (0.006)	-0.012* (0.007)	-0.010 (0.007)
storm_lead3	-0.003 (0.005)	-0.004 (0.005)	-0.015** (0.006)	-0.012** (0.006)
storm_lead2	-0.007** (0.004)	-0.007** (0.004)	-0.009* (0.005)	-0.009* (0.005)
storm_lago	0.003 (0.005)	0.006 (0.005)	0.002 (0.006)	0.003 (0.006)
storm_lag1	-0.003 (0.006)	-0.000 (0.006)	-0.005 (0.007)	-0.002 (0.007)
storm_lag2	-0.010 (0.007)	-0.008 (0.007)	-0.010 (0.007)	-0.009 (0.007)
storm_lag3	-0.009 (0.006)	-0.008 (0.006)	-0.012 (0.007)	-0.011 (0.007)
storm_lag4	-0.014* (0.007)	-0.012* (0.007)	-0.019** (0.008)	-0.017** (0.008)
storm_lag5	-0.019* (0.010)	-0.013 (0.009)	-0.023** (0.011)	-0.016* (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.4.2 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%.

#### 4.5.2 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 & d'Haultfoeuille, 2020a), we find that saving rates are affected by storms (all inclusive)—not instanta-

neously, but after a four-year delay (Figure 4.4). In the fifth post-storm year, the saving rate decreases by 1.87 percentage points (Figure 4.5, Panel A). Parallel pre-trends is satisfied, with placebo estimates of pre-trends jointly insignificant (Figure 4.4).

After accounting only for storms that occur as the sole natural disaster type 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 4.5, 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 4.4). Notwithstanding, parallel pre-trends is not rejected when looking at multiple factors that may explain the observed saving behavior in Section 4.6.

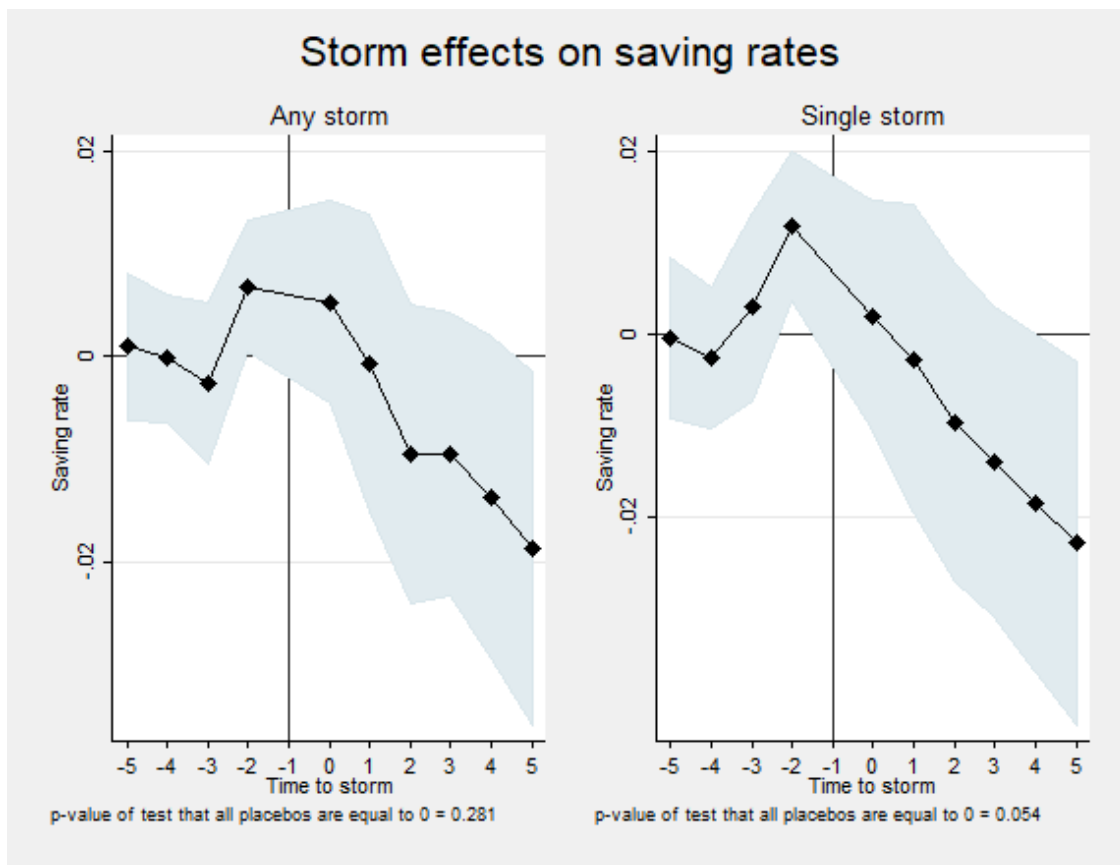


Figure 4.4: De Chaisemartin and d'Haultfoeuille, 2020a's estimator for the average causal effect of storms on saving rates

	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 4.5: 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.

## 4.6 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 & Mobarak, 2008; Schumacher & Strobl, 2011; Skidmore, 2001), ratio of disaster damage to GDP (Toya & Skidmore, 2007), and intensity measures of disasters based on the death toll (Cunado & 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 4.6.1 and Subsection 4.7.1). We use two measures of the relative intensity of storms (total damage per capita and ratio of disaster damage to GDP).<sup>10</sup> 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 4.6.1, part b); (ii) decreasing effect of storms on employment rate—risk of unemployment leading to the decline in saving rates (see Subsection 4.6.1, part b); (iii) decreasing effect of storms on per capita labor income growth—risk of income loss channel to declining saving rates (see Subsection 4.6.1, 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 4.6.1, part b).

Beside these direct channels, 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).<sup>11</sup> 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 saving rates adjust to natural disasters. Remittances including official development assistance lessen the negative macroeconomic impacts of natural disasters (Hochrainer, 2009; Raschky & Schwindt, 2016; D. Yang, 2008). We investigate this channel by estimating the treatment effects on per capita GDP growth (see Subsection 4.6.2), which might follow the same trends as those on GDP growth (see Subsection 4.7.2).

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;

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<sup>10</sup>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 C.1).

<sup>11</sup>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 & Weck-Hannemann, 2007).

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 & 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 4.6.3). Given that developed countries offer a more enabling environment for access to insurance and credit (Kunreuther, 1996; Toya & 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 preferences—for time (Callen, 2015; Cassar et al., 2017) and risk (Cameron & Shah, 2015; Hanaoka et al., 2018; Shupp et al., 2017)—in disaster-stricken locations, which in turn alter consumption and saving patterns. However, we lack country-year data on risk and time preferences for a representative agent. Accordingly, we do not explore this channel.

## 4.6.1 Direct channels

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

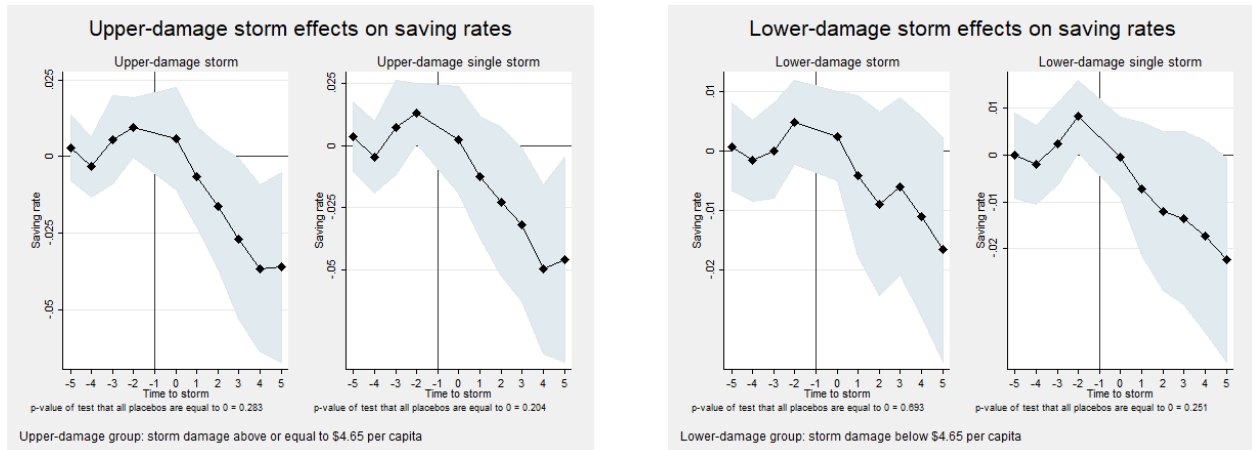
To further check whether storm effects follow the magnitude of total damage per capita, we create two groups of storms using the third quartile of total damage per capita for storms as cutoff<sup>12</sup>, which is \$4.65 per capita. Average damage is of 42 USD cents per capita in the “lower-damage” group versus \$934 per capita in the “upper-damage” group. Figure 4.6a 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 C.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 C.1, Panel B). In contrast, Figure 4.6b 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 C.1, Panel D). Hence, the higher the storm damage per capita, the higher use of the share of income that is saved on coping with the storm.

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 “upper-ratio” storms (total damage equal or above 0.04% of GDP) or “lower-ratio” storms (total damage less than 0.04% of GDP) storms. Mean proportion of damage to GDP is 4% for upper-ratio storms versus 0.004% for lower-ratio storms. Figure 4.7a shows decreasing saving rates due to upper-ratio storms (all inclusive) and single storms. Upper-ratio 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 C.2, Panel A). The effects of upper-ratio 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 C.2, Panel

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<sup>12</sup>Median storm damage per capita is very low (1.62 USD cents per capita), hence not used as the cutoff.

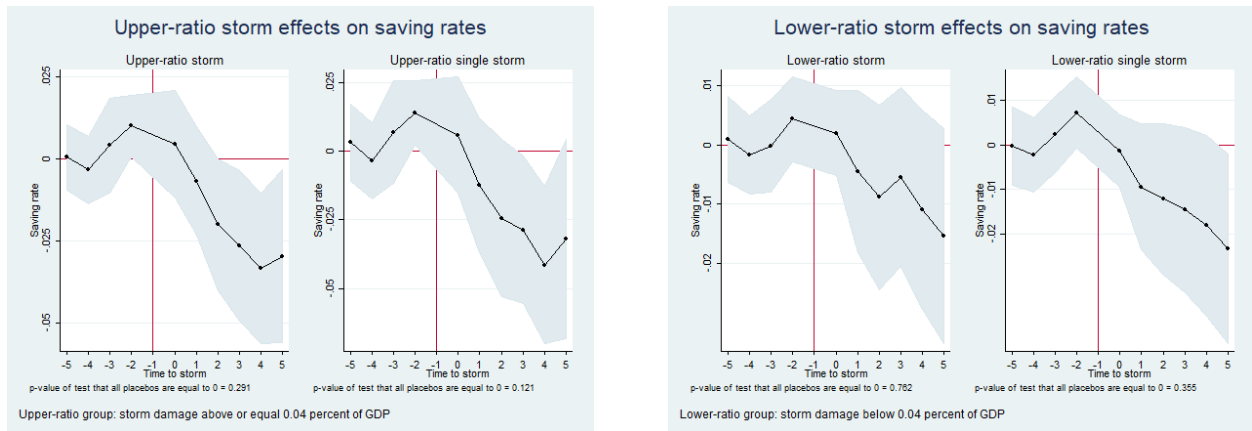
B), as opposed to those of lower-ratio single storms by -2.34 percentage points in the fifth post-storm year (Figure C.2, Panel D). Refining storm intensity groups by using other cutoffs as a robustness check in Subsection 4.7.1 maintains the finding of a negative effect of severely damaging storms on saving rates.



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

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

Figure 4.6: 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 upper-ratio of damage to GDP storms on saving rates

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

Figure 4.7: Average causal effect of storms on saving rates by groups of ratio of damage to GDP (third quartile as cutoff)

## **b. Other direct channels**

### *(i) 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 4.8a). Thus, we rule out the risk of asset loss as a channel leading to a fall in saving rates.

### *(ii) 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 4.8b, employment rates are not worsened by storms, a finding also reported in several impact studies of natural disasters (Cavallo & 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.

### *(iii) 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 4.8c 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 4.4). Hence, we fail to reject the income loss channel to declining post-storm saving rates.

(iv) Inflation rate

We find no evidence of rising inflation rates instantaneously or in the years following storms (all inclusive) and single storms (Figure 4.8d). 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).

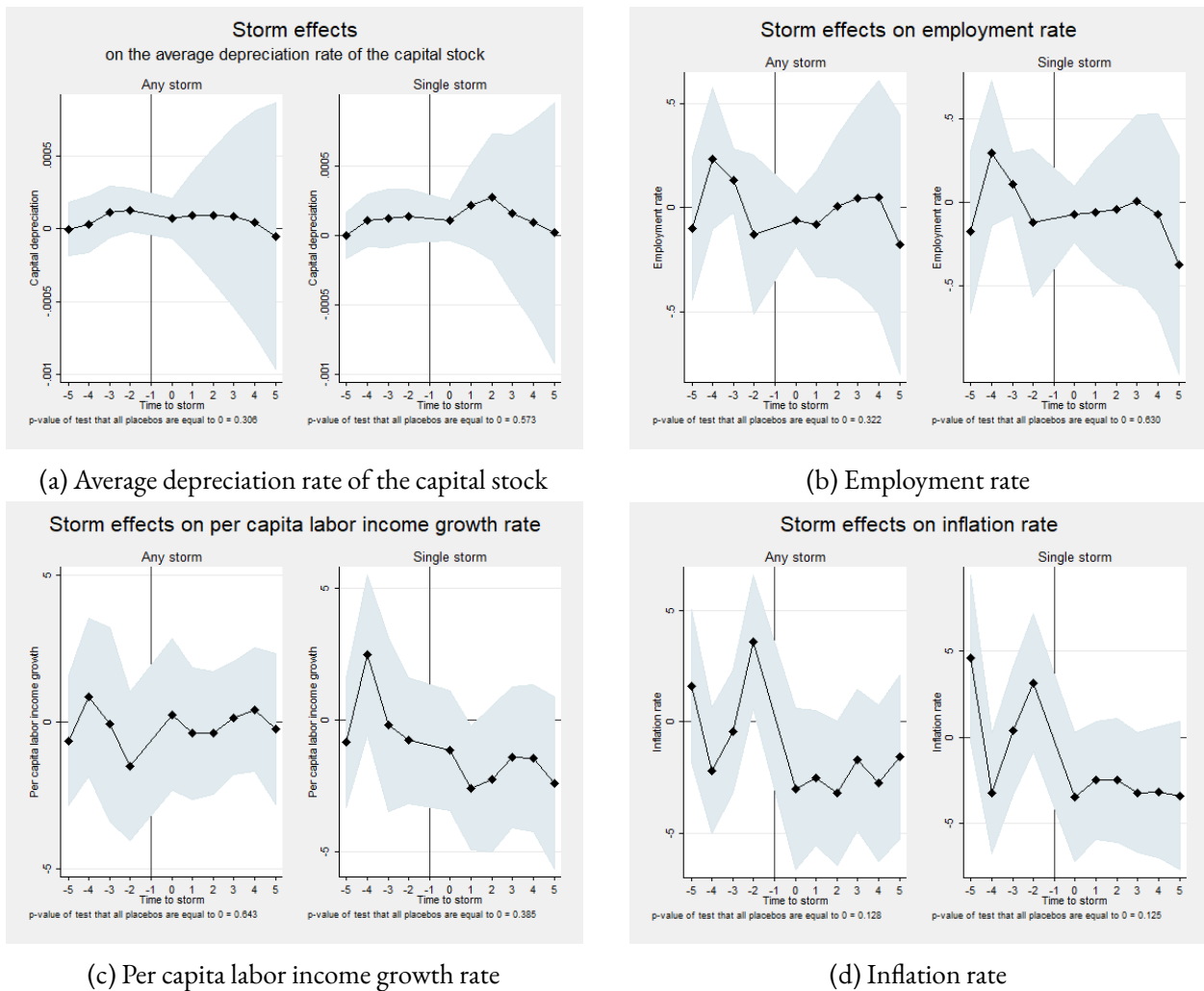


Figure 4.8: De Chaisemartin and d’Haultfoeuille, 2020a’s estimator for the average causal effect of storms on other direct disaster-induced outcomes

## 4.6.2 Per capita GDP growth

The insignificant effects of single storms on per capita GDP growth rates (Figure 4.9) 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 & Noy, 2009; Kellenberg & Mobarak, 2011; Noy & 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 & Ferreira, 2014; Fomby et al., 2013). We examine further the treatment effects of storms on GDP growth rates in Subsection 4.7.2 before drawing a conclusion on the disaster aid channel.

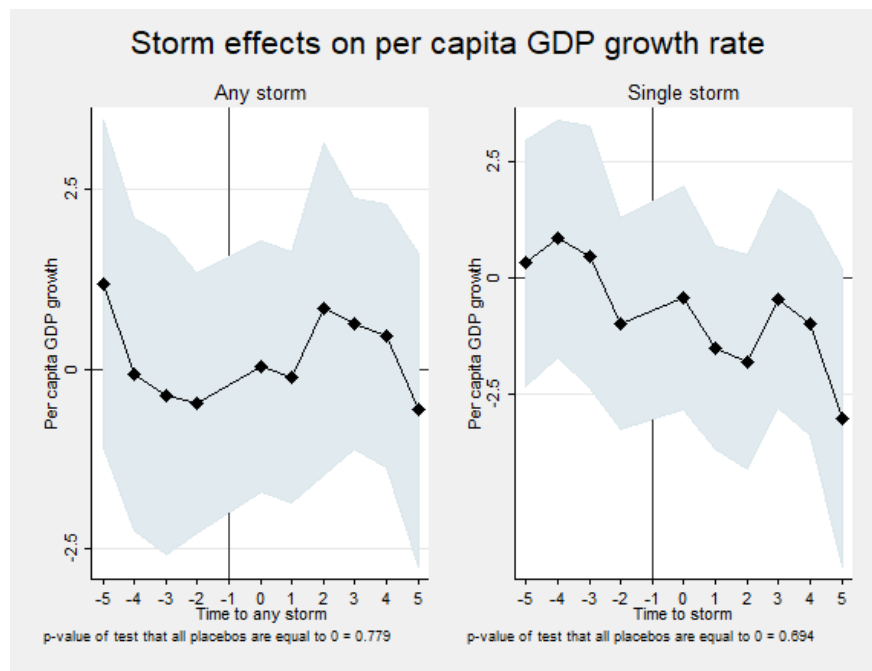


Figure 4.9: De Chaisemartin and d’Haultfoeuille, 2020a’s estimator for the average causal effect of storms on per capita GDP growth rates

### 4.6.3 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 4.4). While Figure 4.10 highlights flat post-storm saving rates in developed countries (statistically insignificant decreases), Figure 4.11 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 C.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 4.11) makes us not to rule out the potential mechanism of economic development institutions behind the impact of storm events on saving rates.

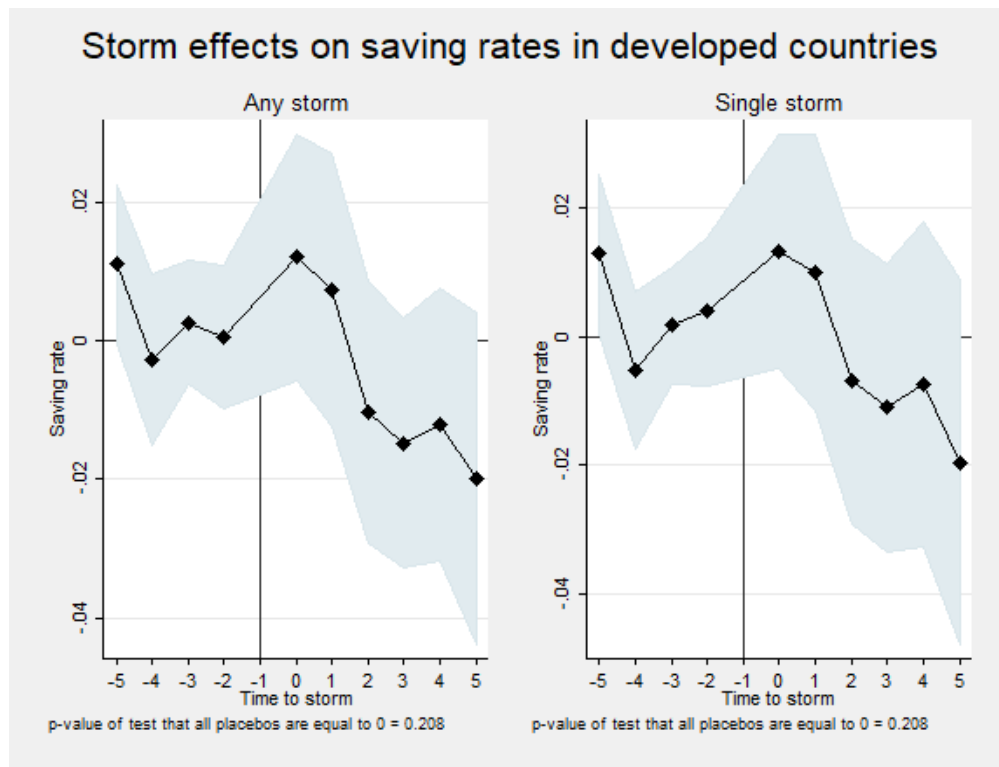


Figure 4.10: De Chaisemartin and d'Haultfoeuille, 2020a's estimator for the average causal effect of storms on saving rates in developed countries

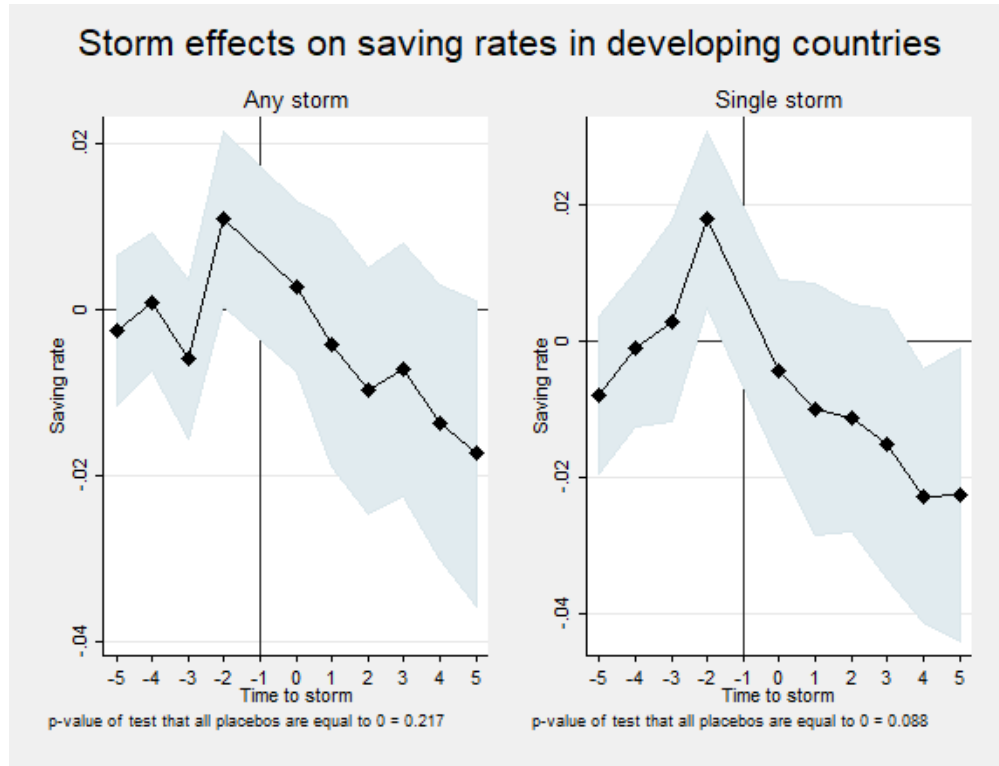


Figure 4.11: De Chaisemartin and d’Haultfoeuille, 2020a’s estimator for the average causal effect of storms on saving rates in developing countries

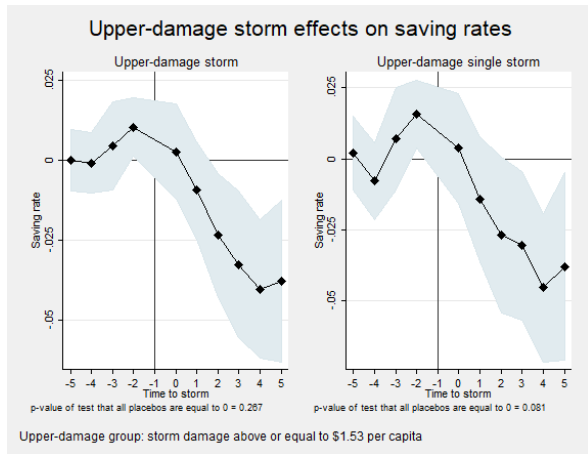
## 4.7 Robustness checks

### 4.7.1 Changing the cutoff for damage groups

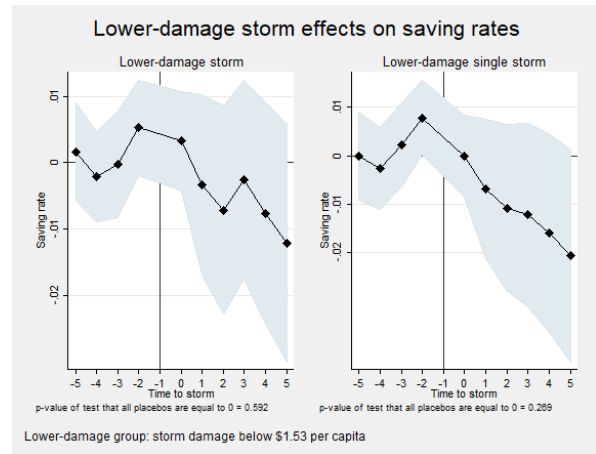
To verify whether the heterogeneity in storm effects on saving rates by storm damage groups is robust to the cutoff used in creating the damage groups, we now use the second tercile of total damage per capita and ratio of damage to GDP instead of the third quartile in precedent results (Subsection 4.6.1, 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 4.12a).

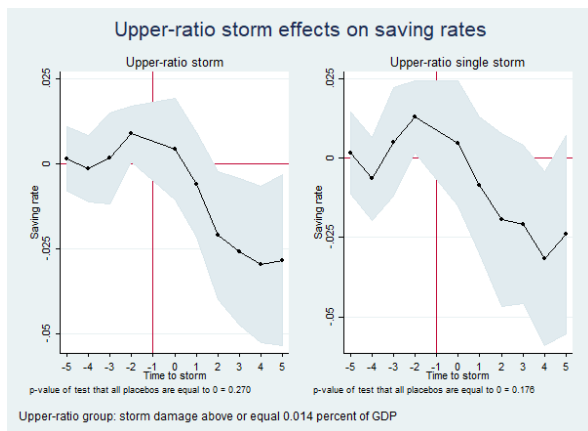


(a) De Chaisemartin and d’Haultfoeuille, 2020a’s estimator for the average causal effects of upper-damage per capita storms on saving rates

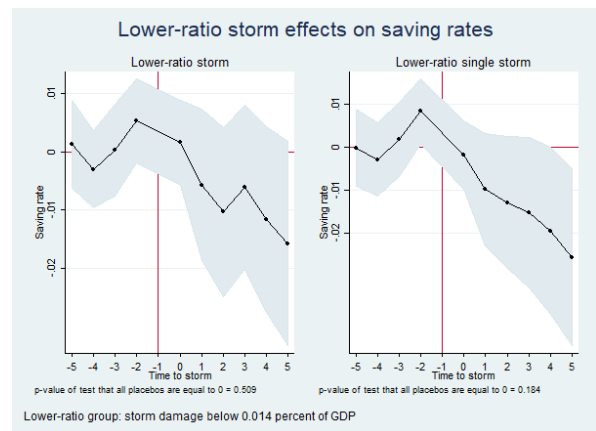


(b) De Chaisemartin and d’Haultfoeuille, 2020a’s estimator for the average causal effects of lower-damage per capita storms on saving rates

Figure 4.12: 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 upper-ratio of damage to GDP storms on saving rates



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

Figure 4.13: 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 more intensely damaging storms from less intensely damaging ones, we still find that the storm effects on saving rates decrease with the ratio of damage to GDP. The treatment effects for upper-ratio 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 4.13a). While saving rates for upper-ratio single storms reduce by 3.17 percentage points in the fourth post-storm year as compared to the year preceding the storms (Figure 4.13a), the treatment effects of lower-ratio 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 4.13b).

#### **4.7.2 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 4.14. The results look similar to those in Figure 4.9, although there is evidence of a slower reduction in GDP growth by 3.34 percentage points in the fifth year following single storms (Figure 4.14). 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 4.7.3 lead to finally rule out the disaster aid channel.

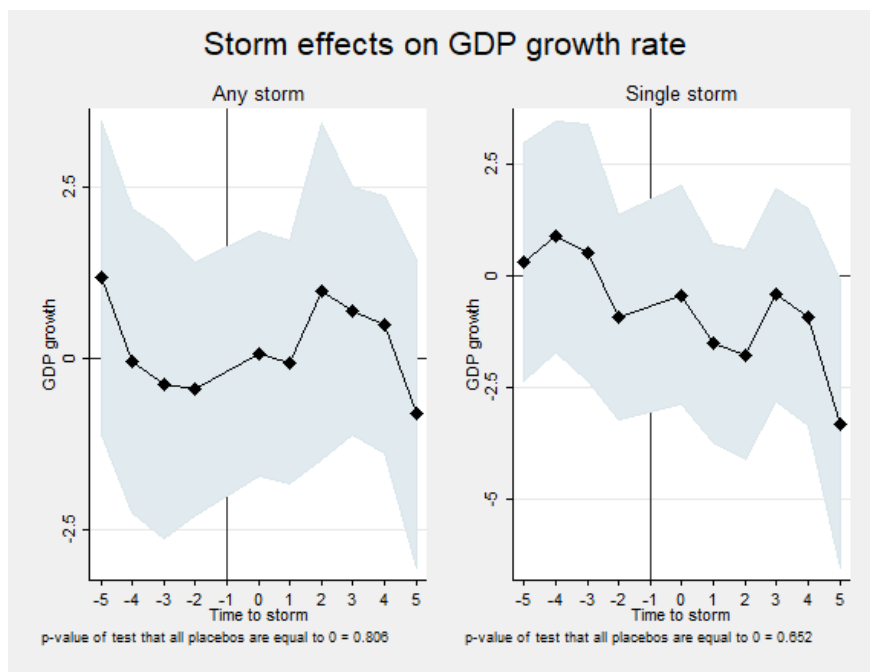


Figure 4.14: De Chaisemartin and d’Haultfoeuille, 2020a’s estimator for the average causal effect of storms on GDP growth rates

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

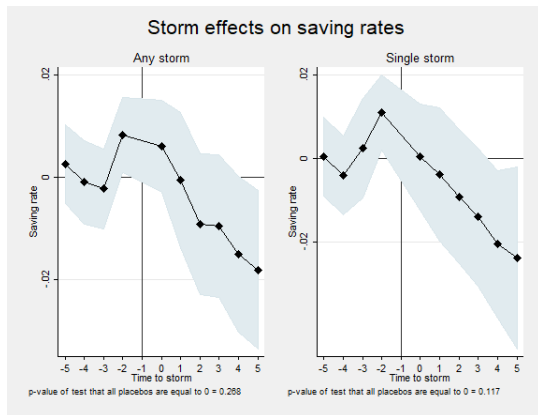
Because the study panel sample covers 176 countries with different lengths of years (between 14 and 69), we assess the robustness of the findings using a more balanced panel sample that includes 151 countries with non-missing data for at least 46 years (two thirds of the years covered in the estimations above).<sup>13</sup>

Focusing on storm events (all inclusive), the robust findings using a more balanced sample indicate these events are not saving-free, a result generated from facing a higher storm damage per capita. We find that GDP growth rates are not influenced by storms (Figure 4.15d), thus suggesting that any changes in saving rates derive from changes in savings levels. The evidence about the reduction of saving rates

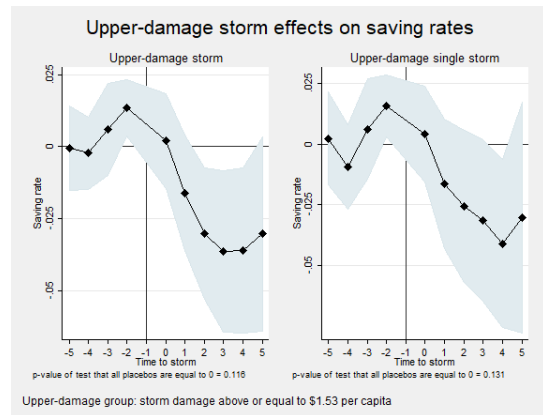
<sup>13</sup>We do not use the fully balanced panel sample of 55 countries with non-missing data for years 1951 through 2019 due to low statistical power issues in dynamic estimations with switching treatment. Because De Chaisemartin and d’Haultfoeuille, 2020a’s estimator compares switchers to non-switchers, 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.

following any storm, irrespective of the storm intensity, is weak: a decrease by 1.81 percentage points occurring after a four-year delay (Figure 4.15a). 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 4.15b). For storms (all inclusive), there is no evidence suggesting as impact mechanisms the risk of income loss (Figure 4.15c) or presence of weak economic development institutions (Figure 4.15e).

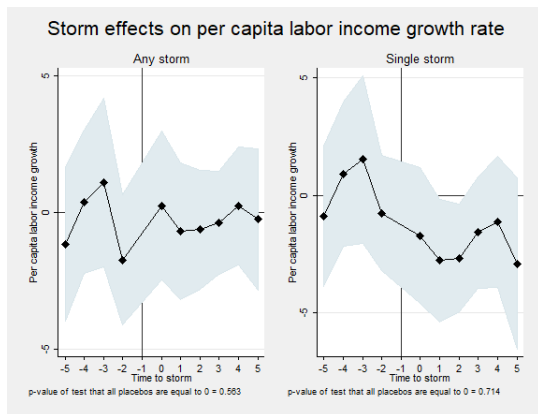
Regarding single storm events, robust findings using a more balanced sample point towards the risk of income loss, high storm damage per capita, and the presence of weak economic development institutions, as the mechanisms driving falling saving rates. Strikingly, in the aftermath of storms as the sole 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 4.15c). 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 4.15a)—a drop pertaining to savings levels (Figure 4.15d). 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 4.15b). Single storms also lead to falling saving rates by 2.45 percentage points in the fourth post-storm year in developing countries (Figure 4.15e), while there is no such evidence in developed countries (Figure 4.15f).



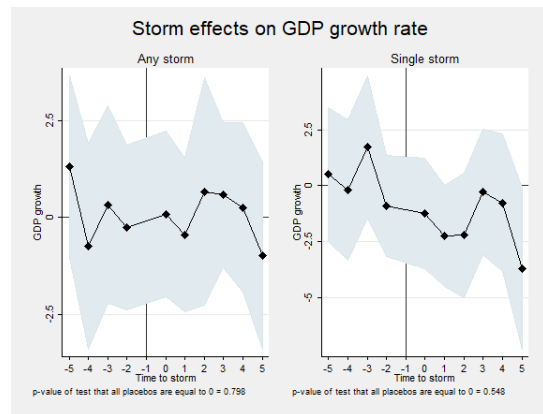
(a) De Chaisemartin and d'Haultfoeuille, 2020a's estimator for the average causal effect of storms on saving rates



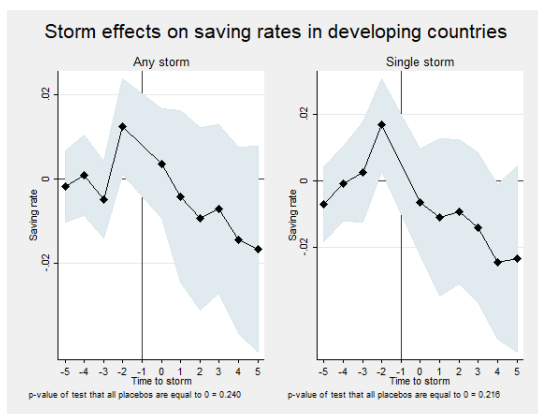
(b) De Chaisemartin and d'Haultfoeuille, 2020a's estimator for the average causal effects of upper-damage per capita storms on saving rates



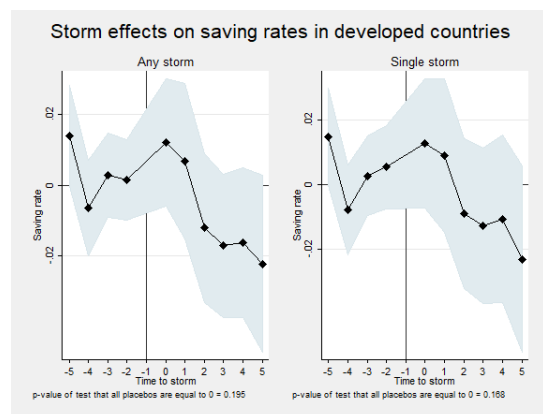
(c) De Chaisemartin and d'Haultfoeuille, 2020a's estimator for the average causal effect of storms on per capita labor income growth rate



(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 4.15: Summary effects of storms using a more balanced panel sample

## 4.8 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 disaster type 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.

# CHAPTER 5

## CONCLUSION

Natural disasters are often expected to have large economic repercussions. A short stance suggested by this dissertation is “the economic repercussions of storms are small.” A three-part, little longer stance provided by this dissertation is as follows: (1) in general, at the country level, storms do not immediately affect economic growth, inflation rates, employment rates, and other macroeconomic outcomes, and so do not other natural disasters such as floods, earthquakes, droughts, and epidemics; (2) nevertheless, retail local prices within a country can change a little due to certain storms, even though the price changes are mostly efficient; (3) storms also influence savings rates, not immediately in the wake of storms, but after two years, and only the intensely-damaging storms induce this decrease in the propensity to save.

In a policy perspective, I articulate below detailed answers to the four specific research questions that this dissertation addressed. On the first question “Do market prices change in times of catastrophe?,” the analyses suggest “yes” as the answer. Prices of the four products studied—toilet paper, batteries, milk, and bottled water—in coastal Texas, US, change a little due to Hurricane Harvey. These modest price changes would have complied with a standard proportional price increase cap of 25%. On the second question “Are the changes in prices efficient (in that they reflect changes in supply and demand)?,” the investigation concludes that the price changes due to Hurricane Harvey in the short run are demand-driven and fall mostly within the ranges of efficiency permitted by variations in the price elasticity of demand. Altogether, these two answers lead to discard the claim about “price gouging” and suggest that, if price gouging laws

were to be decreed, it would be advised not to set a subjective price cap but clarify the allowable price increases based on empirical supply and demand dynamics.

Regarding the third question “Do natural disasters have an immediate impact on the economy when they strike a country?,” the descriptive analysis offers “mostly no!” as the answer. In light of the estimations for seven macroeconomic outcomes, the answer is strictly negative for storms and droughts. On the standard measure of the economy, economic growth, the answer is strictly negative for all five studied natural disasters. Only three studied macroeconomic outcomes have an immediate response to certain disasters: floods and earthquakes (in a larger magnitude) increase the depreciation rate of the capital stock; floods increase saving rates; and epidemics decrease the internal rate of return of firms. Although the analysis does not pinpoint the mechanisms through which these immediate impacts occur or why they do not occur in some cases, it strengthens the indication that the macroeconomic impacts of natural disasters are mostly not contemporaneous, thereby underscoring the importance of planning ahead in disaster management policies.

About the last question “Do natural disasters affect saving rates?,” the study takes the case of storms and offers a simple answer that aligns with the third question. The answer is the following: “yes, but the elapsed time is three years, and it concerns the severe storms in terms of damage.” A closer look at this delayed, storm-induced propensity to dis-save reveals that its driving forces are the slower per capita labor income growth and the weak economic development institutions. The analyses discard several driving forces, including disaster aid, asset depreciation, unemployment, and inflation.

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# APPENDIX A

## IMMINENT NATURAL DISASTER AND PRICE GOUGING? EVIDENCE FROM HURRICANE HARVEY IN THE U.S.

### A.1 Proof of the result from the optimal pricing model

#### Proof

Define the function  $\psi[\cdot]$  as follows:

$$\psi [P_{rsjyw}^*, c_j, b_{sj}^*(t); t] = P_{rsjyw}^* - c_j + \frac{1}{b_{sj}^*(t)} \quad , \quad b_{sj}^*(t) \neq 0.$$

As established in Eq.2.2,  $\psi [P_{rsjyw}^*, c_j, b_{sj}^*(t); t] = 0$ . For any  $P_{rsjyw}^* > 0$ , any  $c_j > 0$ , and  $b_{sj}^*(t) \neq 0$ ,  $\psi [P_{rsjyw}^*, c_j, b_{sj}^*(t); t]$  is continuously differentiable, with  $\frac{\partial \psi}{\partial P_{rsjyw}^*} = 1 \neq 0$ .

Take  $c_j$  constant in all periods  $t$  studied. By the implicit function theorem, there exists a unique function  $P_{rsjyw}^*(t)$  defined for  $b_{sj}^*(t) \neq 0$ , which is continuously differentiable such that

$$\frac{dP_{rsjyw}^*}{dt} = -\frac{\frac{\partial \psi}{\partial t}}{\frac{\partial \psi}{\partial P_{rsjyw}^*}} = -\frac{\partial \psi}{\partial t}. \quad (\text{Eq.A.1})$$

Using the chain rule and  $\frac{\partial \psi}{\partial b_{sj}^*(t)} = \frac{-1}{(b_{sj}^*(t))^2}$  yields the result in Eq.2.3:

$$\frac{dP_{rsjyw}^*}{dt} = -\frac{\partial \psi}{\partial b_{sj}^*(t)} \frac{\partial b_{sj}^*(t)}{\partial t} = \frac{1}{(b_{sj}^*(t))^2} \frac{\partial b_{sj}^*(t)}{\partial t}. \quad \square \quad (\text{Eq.A.2})$$

## A.2 Tables

Table A.1: Summary statistics of individual assistance and needs applications

	Coastal counties Mean(SD)	Non-coastal counties Mean(SD)	All counties Mean(SD)
<b>Panel A: Year 2017</b>			
Number of applications to Housing Programs (IHP/HA)	4729.8 (32374.0)	66.24 (1880.8)	1415.9 (17615.1)
Applications to Housing Programs (IHP/HA) (%)	0.757 (4.616)	0.0263 (0.925)	0.239 (2.627)
Number of applications reporting a need for shelter	1480.7 (10182.0)	20.10 (599.3)	442.8 (5540.4)
Applications reporting a need for shelter (%)	0.240 (1.542)	0.00884 (0.366)	0.0760 (0.892)
Number of applications reporting a need for special accommodations	211.6 (1517.6)	2.667 (75.04)	63.13 (824.3)
Applications reporting a need for special accommodations (%)	0.0314 (0.194)	0.00107 (0.0384)	0.00989 (0.110)
Number of applications reporting a need for emergency items or food	3619.8 (25329.6)	46.15 (1323.5)	1080.4 (13767.5)
Applications reporting a need for emergency items or food (%)	0.569 (3.519)	0.0192 (0.715)	0.179 (2.005)
Observations	15858	38937	54795
<b>Panel B: Harvey period (17 Aug 2017 - 1 Sep 2017)</b>			
Number of applications to Housing Programs (IHP/HA)	79877.9 (108207.1)	1326.0 (8317.2)	26901.7 (72191.0)
Applications to Housing Programs (IHP/HA) (%)	12.8 (14.4)	0.5 (4.1)	4.5 (10.6)
Number of applications reporting a need for shelter	25005.8 (34113.4)	402.3 (2653.3)	8413.0 (22723.0)
Applications reporting a need for shelter (%)	4.1 (5.0)	0.2 (1.6)	1.4 (3.6)
Number of applications reporting a need for special accommodations	3573.5 (5187.1)	53.4 (331.8)	1199.5 (3398.5)
Applications reporting a need for special accommodations (%)	0.5 (0.6)	0.02 (0.2)	0.2 (0.4)
Number of applications reporting a need for emergency items or food	61132.3 (85595.2)	923.9 (5854.1)	20527.1 (56596.0)
Applications reporting a need for emergency items or food (%)	9.6 (11.1)	0.4 (3.2)	3.4 (8.1)
Warned for hurricane Harvey (1=yes, 0=no)	0.7 (0.5)	0.2 (0.4)	0.3 (0.5)
Observations	939	1945	2884

*Notes:* This table presents the summary statistics of individual assistance applications using the store sample in the state of Texas. Standard deviations are in parentheses below the means. Observation level is store zipcode<sub>3</sub> x UPC x year x week. Observations cover all 52 weeks in year 2017 in Panel A and weeks of Harvey season in Panel B. Store zipcode<sub>3</sub> is the 3-digit zipcode that localizes the store. Nielsen protects the confidentiality of individual stores by providing only the first 3 digits of the full zip code. Store zipcode<sub>3</sub> is also provided only for a subset of purchase trips. Since Nielsen does not also give an exact store location (it is inferred from the closest store to the zip code of the consumer home), we replace any missing store zipcode<sub>3</sub> by the consumer home zipcode<sub>3</sub>. UPC refers to Universal Product Code. Applications to individual assistance programs (%) and applications reporting needs (%) are normalized by the total population (number of households) in the zipcode<sub>3</sub> in year 2017 and multiplied by 100. Data source for hurricane warning is NOAA/NCEI, supplemented by NOAA way back machine data and Google search with date range customized to Harvey season. Data source for applications to individual assistance programs and applications reporting needs is FEMA.

Table A.2: Summary statistics of product price and quantity

	Toilet paper Mean(SD)	Batteries Mean(SD)	Milk Mean(SD)	Bottled water Mean(SD)
<b>Panel A: Household sample</b>				
Price per unit	0.632 (0.445)	1.475 (2.738)	0.0571 (0.0271)	0.0225 (0.0404)
Log of Price per unit	-0.560 (0.439)	-0.0973 (0.929)	-2.979 (0.499)	-4.384 (0.939)
Price per unit - winsorized 1st and 99th	0.626 (0.283)	1.244 (1.024)	0.0571 (0.0271)	0.0225 (0.0404)
Price per unit - winsorized 2nd and 98th	0.626 (0.279)	1.161 (0.857)	0.0571 (0.0271)	0.0225 (0.0404)
Price index	0.993 (0.110)	0.998 (0.157)	0.987 (0.115)	0.996 (0.129)
Log of Price index	-0.0130 (0.110)	-0.0131 (0.147)	-0.0205 (0.122)	-0.0111 (0.123)
Price index - winsorized 1st and 99th	0.992 (0.0943)	0.995 (0.122)	0.987 (0.105)	0.994 (0.104)
Price index - winsorized 2nd and 98th	0.992 (0.0869)	0.994 (0.107)	0.987 (0.0972)	0.993 (0.0943)
Quantity purchased (units)	16.36 (13.04)	11.19 (13.89)	64.77 (57.57)	416.1 (417.5)
Log of Quantity purchased (units)	2.539 (0.721)	1.930 (0.976)	3.890 (0.755)	5.460 (1.228)
Observations	89513	38061	19811	122069
<b>Panel B: Store sample</b>				
Price per unit	0.6 (0.5)	1.5 (2.8)	0.06 (0.03)	0.02 (0.04)
Log of Price per unit	-0.6 (0.4)	-0.09 (0.9)	-2.9 (0.5)	-4.3 (1.0)
Price per unit - winsorized 1st and 99th	0.6 (0.3)	1.3 (1.0)	0.06 (0.03)	0.02 (0.04)
Price per unit - winsorized 2nd and 98th	0.6 (0.3)	1.2 (0.9)	0.06 (0.03)	0.02 (0.04)
Price index	1.0 (0.1)	1.0 (0.2)	1.0 (0.1)	1.0 (0.1)
Log of Price index	-0.01 (0.1)	-0.01 (0.1)	-0.02 (0.1)	-0.010 (0.1)
Price index - winsorized 1st and 99th	1.0 (0.09)	1.0 (0.1)	1.0 (0.1)	1.0 (0.1)
Price index - winsorized 2nd and 98th	1.0 (0.08)	1.0 (0.1)	1.0 (0.10)	1.0 (0.09)
Total Quantity purchased (units)	18.7 (16.7)	11.9 (15.0)	77.0 (83.1)	549.3 (686.0)
Log of Total Quantity purchased (units)	2.6 (0.8)	2.0 (1.0)	4.0 (0.8)	5.6 (1.3)
Observations	83229	37248	17564	106590

*Notes:* This table presents the summary statistics of product price and quantity across all counties in the household sample (Panel A) and the store sample (Panel B). Units are count for toilet paper and batteries, but ounces for milk and bottled water. Price index winsorized at the  $x$ th and  $(100-x)$ th percentiles means: values smaller than the  $x$ th percentile are replaced by the  $x$ th percentile, and values higher than the  $(100-x)$ th percentile are replaced by the  $(100-x)$ th percentile. Standard deviations are in parentheses below the means. Observation level is household  $\times$  UPC  $\times$  year  $\times$  day in Panel A. Observation level is store zipcode3  $\times$  UPC  $\times$  year  $\times$  week in Panel B. Observations cover all 52 weeks in years 2014 to 2017. Store zipcode3 is the 3-digit zip code that localizes the store. Nielsen protects the confidentiality of individual stores by providing only the first 3 digits of the full zip code. Store zipcode3 is also provided only for a subset of purchase trips. Since Nielsen does not also give an exact store location (it is inferred from the closest store to the zip code of the consumer home), we replace any missing store zipcode3 by the consumer home zipcode3. UPC refers to Universal Product Code.

Table A.3: Demand estimations in Harvey period

*Panel A. All counties*

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Price index ( $\vartheta$ )	-0.132*** (0.032)	-0.153*** (0.049)	-0.359*** (0.067)	-0.440*** (0.045)	-0.176*** (0.038)	-0.277*** (0.061)	-0.487*** (0.082)	-0.665*** (0.061)
Hurricane Harvey period (17 Aug 2017 - 1 Sep 2017) ( $\chi$ )	-0.346 (0.324)	-0.324 (0.361)	0.496 (0.570)	-0.703* (0.382)	-0.369 (0.323)	-0.398 (0.364)	0.405 (0.573)	-0.744** (0.377)
Price index x Harvey period ( $\iota$ )	-0.341 (0.319)	0.350 (0.360)	-0.578 (0.579)	0.807** (0.376)	0.363 (0.318)	0.425 (0.364)	-0.484 (0.582)	0.848** (0.370)
Observations	83,229	37,248	17,564	106,590	83,229	37,248	17,564	106,590
R-squared	0.005	0.011	0.010	0.006	0.005	0.011	0.011	0.006
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Year and Week fixed effects and Sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Change in intercept in Harvey period=	-0.346	-0.324	0.496	-0.703	-0.369	-0.398	0.405	-0.744
Testing change in intercept=0, p-value=	0.285	0.369	0.385	0.066	0.254	0.274	0.479	0.048
Elasticity coefficient in Harvey period=	0.208	0.197	-0.925	0.366	0.186	0.147	-0.960	0.183
Testing elasticity coefficient=0, p-value=	0.510	0.580	0.103	0.325	0.555	0.681	0.092	0.618
Testing elasticity coefficient=-1, p-value=	0.000	0.001	0.896	0.000	0.000	0.001	0.944	0.001

*Panel B. Coastal counties*

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Price index ( $\vartheta$ )	-0.124** (0.061)	-0.268*** (0.090)	-0.405*** (0.121)	-0.662*** (0.081)	-0.227*** (0.067)	-0.443*** (0.107)	-0.603*** (0.141)	-0.985*** (0.108)
Hurricane Harvey period (17 Aug 2017 - 1 Sep 2017) ( $\chi$ )	-0.772 (0.676)	-0.826 (0.569)	1.385 (1.136)	-0.253 (0.668)	-0.847 (0.674)	-0.945 (0.578)	1.212 (1.123)	-0.287 (0.641)
Price index x Harvey period ( $\iota$ )	0.712 (0.666)	0.795 (0.557)	-1.476 (1.082)	0.322 (0.647)	0.786 (0.664)	0.914 (0.567)	-1.299 (1.070)	0.355 (0.619)
Observations	24,226	11,069	5,080	31,559	24,226	11,069	5,080	31,559
R-squared	0.009	0.017	0.036	0.008	0.009	0.017	0.038	0.009
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Year and Week fixed effects and Sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Change in intercept in Harvey period=	-0.772	-0.826	1.385	-0.253	-0.847	-0.945	1.212	-0.287
Testing change in intercept=0, p-value=	0.253	0.146	0.223	0.705	0.209	0.102	0.280	0.654
Elasticity coefficient in Harvey period=	0.585	0.529	-1.848	-0.338	0.555	0.471	-1.869	-0.625
Testing elasticity coefficient=0, p-value=	0.375	0.337	0.080	0.597	0.397	0.399	0.073	0.306
Testing elasticity coefficient=-1, p-value=	0.016	0.006	0.422	0.300	0.018	0.008	0.405	0.539

*Notes:* This table reports the results from demand estimations in Harvey period using a semi-log functional form and canonical difference-in-differences. The estimation sample includes all years (2014-2017) in all counties in the state of Texas in Panel A, and coastal counties in the state of Texas in Panel B. As defined by FEMA, coastal counties (a) have a shoreline, or (b) contain velocity zones (V-zones) or coastal high hazard areas. Dependent variable is log store-UPC-level total quantity purchased. In Hurricane Harvey period, the change in demand intercept is  $\chi$ , and the elasticity coefficient is  $\eta = (\vartheta + \iota) * \bar{P}$  evaluated at the estimation sample mean. The product is toilet paper (TP) in columns 1 and 5, batteries (BT) in columns 2 and 6, milk (MK) in columns 3 and 7, and bottled water (BW) in columns 4 and 8. In columns 5 through 8, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Year and week fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.4: Demand changes due to warning for Harvey, during landfall, and after strike - All counties of the state of Texas

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Price index ( $\vartheta^{P^{r e}}$ )	-0.116** (0.050)	-0.129* (0.070)	-0.577*** (0.110)	-0.432*** (0.063)	-0.144*** (0.055)	-0.307*** (0.085)	-0.724*** (0.128)	-0.605*** (0.086)
Post-catastrophe year ( $\alpha$ )	-0.019 (0.064)	0.102 (0.095)	-0.397*** (0.133)	0.068 (0.092)	0.018 (0.075)	0.000 (0.122)	-0.414** (0.161)	0.124 (0.123)
Price index x Post ( $\beta^{Post}$ )	-0.047 (0.064)	-0.058 (0.094)	0.342** (0.133)	0.022 (0.091)	-0.083 (0.075)	0.044 (0.122)	0.359** (0.162)	-0.032 (0.123)
Price index x Warning for actual Harvey ( $\beta^{\varpi}$ )	0.001 (0.076)	-0.055 (0.095)	-0.211 (0.136)	0.234** (0.093)	-0.002 (0.075)	-0.053 (0.095)	-0.207 (0.136)	0.235** (0.093)
Price index x During actual Harvey landfall ( $\beta^h$ )	-0.020 (0.084)	0.062 (0.103)	0.172 (0.168)	0.506*** (0.110)	-0.023 (0.084)	0.069 (0.103)	0.170 (0.167)	0.514*** (0.111)
Price index x After actual Harvey strike (1-3 weeks) ( $\beta^S$ )	-0.157 (0.177)	0.019 (0.348)	-0.249 (0.244)	0.696*** (0.117)	-0.157 (0.177)	0.021 (0.345)	-0.245 (0.243)	0.697*** (0.118)
Applications to Housing Programs (IHP/HA) (%)	0.020 (0.042)	0.011 (0.057)	0.089 (0.082)	-0.092 (0.057)	0.020 (0.042)	0.012 (0.057)	0.086 (0.082)	-0.090 (0.057)
Applications reporting a need for shelter (%)	-0.070 (0.055)	0.011 (0.065)	-0.210 (0.219)	0.101 (0.062)	-0.071 (0.055)	0.011 (0.065)	-0.202 (0.218)	0.100 (0.062)
Applications reporting a need for special accommodations (%)	0.525 (0.736)	-0.969 (0.758)	-0.864 (1.416)	-0.512 (0.801)	0.532 (0.735)	-0.976 (0.760)	-0.872 (1.404)	-0.479 (0.801)
Applications reporting a need for emergency items or food (%)	-0.029 (0.067)	0.038 (0.088)	0.008 (0.084)	0.104 (0.092)	-0.029 (0.067)	0.038 (0.088)	0.009 (0.084)	0.100 (0.092)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	0.075* (0.044)	-0.006 (0.089)	-0.028 (0.086)	0.265*** (0.073)	0.075* (0.044)	-0.006 (0.089)	-0.029 (0.087)	0.267*** (0.073)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.005 (0.034)	-0.025 (0.071)	0.023 (0.064)	0.157*** (0.060)	-0.004 (0.034)	-0.024 (0.071)	0.024 (0.065)	0.159*** (0.060)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	-0.017 (0.043)	0.073 (0.072)	-0.048 (0.079)	0.085 (0.058)	-0.017 (0.043)	0.072 (0.072)	-0.052 (0.079)	0.087 (0.058)
Hurricane Maria period (16 - 30 Sep 2017)	0.027 (0.035)	0.020 (0.066)	-0.059 (0.063)	0.063 (0.057)	0.028 (0.035)	0.020 (0.066)	-0.058 (0.063)	0.064 (0.057)
Observations	82,935	37,128	17,461	105,881	82,935	37,128	17,461	105,881
R-squared	0.012	0.003	0.068	0.015	0.012	0.003	0.069	0.016
Counties	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Elasticity in pre-catastrophe=	-0.115	-0.129	-0.570	-0.431	-0.143	-0.305	-0.715	-0.602
Testing elasticity in pre-catastrophe=0, p-value=	0.020	0.066	0.000	0.000	0.008	0.000	0.000	0.000
Change in elasticity parameter at warning ( $\beta^{Post} + \beta^{\varpi}$ )=	-0.046	-0.112	0.131	0.257	-0.085	-0.009	0.152	0.204
Testing ( $\beta^{Post} + \beta^{\varpi}$ ) = 0, p-value=	0.645	0.398	0.491	0.046	0.422	0.955	0.472	0.184
Efficient price change at warning ( $\lambda^{\varpi}$ )=	0.000	0.000	0.000	1.375	0.000	0.000	0.000	0.000
Change in elasticity parameter at landfall ( $\beta^{Post} + \beta^h$ )=	-0.067	0.005	0.514	0.528	-0.106	0.113	0.529	0.482
Testing ( $\beta^{Post} + \beta^h$ ) = 0, p-value=	0.531	0.972	0.017	0.000	0.345	0.477	0.023	0.004
Efficient price change at landfall ( $\lambda^h$ )=	0.000	0.000	1.545	2.831	0.000	0.000	1.011	1.316
Change in elasticity parameter after strike ( $\beta^{Post} + \beta^S$ )=	-0.203	-0.039	0.092	0.718	-0.240	0.065	0.114	0.666
Testing ( $\beta^{Post} + \beta^S$ ) = 0, p-value=	0.282	0.915	0.740	0.000	0.212	0.859	0.697	0.000
Efficient change after strike ( $\lambda^S$ )=	0.000	0.000	0.000	3.847	0.000	0.000	0.000	1.817

Notes: This table reports the results from demand estimations using a flexible, semi-log functional form (Eq.2.5). The estimation sample covers all four years (2014-2017) and includes all counties in the state of Texas. Dependent variable is log store-UPC-level total quantity purchased. Using Eq.2.5, the elasticity coefficient in pre-catastrophe years is  $\eta = (\vartheta^{P^{r e}}) * \bar{P}$  evaluated at the estimation sample mean. Efficient price changes are set to zero if the change in elasticity parameter is not significant at the 5% level. The product is toilet paper (TP) in columns 1 and 5, batteries (BT) in columns 2 and 6, milk (MK) in columns 3 and 7, and bottled water (BW) in columns 4 and 8. In columns 5 through 8, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.5: Assessment of price changes due to warning for Harvey, during landfall, and after strike - All counties of the state of Texas

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Post-catastrophe year ( $\ell$ )	0.009*** (0.001)	0.001 (0.002)	0.002 (0.002)	0.010*** (0.001)	0.009*** (0.001)	-0.000 (0.001)	0.002 (0.002)	0.009*** (0.001)
Warning for actual Harvey ( $\varpi$ )	0.000 (0.009)	0.000 (0.012)	0.005 (0.023)	0.021** (0.010)	0.001 (0.009)	0.004 (0.010)	0.011 (0.021)	0.018** (0.007)
During actual Harvey landfall ( $h$ )	0.027** (0.012)	0.036** (0.014)	-0.020 (0.026)	-0.006 (0.011)	0.024** (0.011)	0.034*** (0.013)	-0.020 (0.025)	-0.001 (0.008)
After actual Harvey strike (1-3 weeks) ( $\xi$ )	-0.008 (0.015)	-0.005 (0.042)	0.027 (0.029)	-0.010 (0.010)	-0.004 (0.014)	0.001 (0.042)	0.029 (0.027)	-0.009 (0.009)
Applications to Housing Programs (IHP/HA) (%)	-0.004 (0.004)	-0.003 (0.008)	-0.020 (0.016)	0.006 (0.005)	-0.004 (0.004)	-0.001 (0.005)	-0.021 (0.016)	0.006 (0.004)
Applications reporting a need for shelter (%)	-0.005 (0.005)	0.003 (0.008)	0.060*** (0.022)	-0.005 (0.003)	-0.004 (0.005)	0.002 (0.007)	0.059*** (0.021)	-0.004 (0.003)
Applications reporting a need for special accommodations (%)	0.052 (0.083)	-0.061 (0.097)	-0.064 (0.248)	0.171*** (0.061)	0.045 (0.078)	-0.041 (0.091)	-0.060 (0.244)	0.146*** (0.045)
Applications reporting a need for emergency items or food (%)	0.004 (0.006)	0.005 (0.011)	0.010 (0.019)	-0.014** (0.006)	0.004 (0.006)	0.002 (0.008)	0.011 (0.019)	-0.014*** (0.005)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.010* (0.006)	0.000 (0.012)	-0.027* (0.015)	0.011* (0.005)	-0.008 (0.006)	-0.002 (0.009)	-0.021 (0.013)	0.010** (0.004)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.010* (0.006)	0.002 (0.014)	0.008 (0.010)	0.010* (0.005)	-0.006 (0.004)	0.003 (0.008)	0.008 (0.010)	0.009** (0.004)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.002 (0.006)	0.000 (0.009)	-0.023 (0.019)	0.000 (0.005)	0.001 (0.005)	-0.001 (0.007)	-0.026* (0.014)	0.003 (0.004)
Hurricane Maria period (16 - 30 Sep 2017)	0.000 (0.004)	-0.022* (0.012)	0.032** (0.015)	-0.000 (0.004)	0.003 (0.003)	-0.015* (0.008)	0.023** (0.012)	0.002 (0.003)
Constant	0.992*** (0.001)	1.002*** (0.002)	0.985*** (0.001)	0.992*** (0.001)	0.990*** (0.001)	0.998*** (0.001)	0.985*** (0.001)	0.988*** (0.000)
Observations	82,935	37,128	17,461	105,881	82,935	37,128	17,461	105,881
R-squared	0.005	0.008	0.020	0.004	0.007	0.009	0.025	0.007
Counties	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Efficient price change at warning ( $\lambda^\varpi$ )=	0.000	0.000	0.000	1.375	0.000	0.000	0.000	0.000
Observed price change at warning ( $\varpi + \ell$ )=	0.009	0.001	0.007	0.031	0.010	0.004	0.013	0.027
Testing observed price change at warning ( $\varpi + \ell$ ) = $\lambda^\varpi$ , p-value=	0.315	0.910	0.756	0.000	0.246	0.694	0.534	0.000
Efficient price change at landfall ( $\lambda^h$ )=	0.000	0.000	1.545	2.831	0.000	0.000	1.011	1.316
Observed price change at landfall ( $h + \ell$ )=	0.036	0.038	-0.018	0.003	0.033	0.034	-0.018	0.008
Testing observed price change at landfall ( $h + \ell$ ) = $\lambda^h$ , p-value=	0.003	0.009	0.000	0.000	0.002	0.007	0.000	0.000
Efficient price change after strike ( $\lambda^\xi$ )=	0.000	0.000	0.000	3.847	0.000	0.000	0.000	1.817
Observed price change after strike ( $\xi + \ell$ )=	0.001	-0.003	0.029	-0.001	0.005	0.001	0.031	0.000
Testing observed price change after strike ( $\xi + \ell$ ) = $\lambda^\xi$ , p-value=	0.946	0.937	0.323	0.000	0.704	0.983	0.242	0.000

Notes: This table reports the results from price estimations using Eq.2.6. The estimation sample covers all four years (2014-2017) and includes all counties in the state of Texas. Dependent variable is store-UPC-level price index. In columns 5 through 8, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. The product is toilet paper (TP) in columns 1 and 5, batteries (BT) in columns 2 and 6, milk (MK) in columns 3 and 7, and bottled water (BW) in columns 4 and 8. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.6: First-stage regression results from IV demand estimations for toilet paper - All counties in the state of Texas

VARIABLES	(1) TP (Eq.7)	(2) TP (Eq.8)	(3) TP (Eq.9)	(4) TP (Eq.10)	(5) TP (Eq.11)	(6) TP (Eq.7)	(7) TP (Eq.8)	(8) TP (Eq.9)	(9) TP (Eq.10)	(10) TP (Eq.11)
Price index instrument ( $\delta^1$ )	0.4288*** (0.0300)	-0.0015*** (0.0006)	-0.0000 (0.0000)	-0.0000* (0.0000)	-0.0000 (0.0000)	0.4047*** (0.0198)	-0.0011** (0.0004)	-0.0000 (0.0000)	-0.0000* (0.0000)	-0.0000 (0.0000)
Price index instrument x Post ( $\delta^3$ )	0.0207 (0.0396)	0.4535*** (0.0258)	-0.0010 (0.0008)	-0.0002 (0.0008)	0.0002 (0.0002)	0.0024 (0.0267)	0.4102*** (0.0179)	-0.0010 (0.0008)	-0.0003 (0.0008)	0.0002 (0.0002)
Price index instrument x Warning for actual Harvey ( $\delta^4$ )	0.0028 (0.0084)	0.0016 (0.0083)	1.0046*** (0.0071)	-0.0001 (0.0020)	-0.0004 (0.0008)	0.0044 (0.0081)	0.0032 (0.0081)	1.0048*** (0.0069)	-0.0000 (0.0020)	-0.0005 (0.0008)
Price index instrument x During actual landfall ( $\delta^5$ )	0.0131 (0.0095)	0.0133 (0.0095)	0.0058 (0.0058)	1.0056*** (0.0080)	0.0001 (0.0002)	0.0135 (0.0095)	0.0139 (0.0095)	0.0057 (0.0058)	1.0051*** (0.0080)	0.0001 (0.0002)
Price index instrument x After actual strike (1-3 weeks) ( $\delta^6$ )	-0.0025 (0.0129)	-0.0020 (0.0129)	-0.0010 (0.0028)	0.0000 (0.0001)	1.0049*** (0.0105)	0.0015 (0.0122)	0.0019 (0.0122)	-0.0010 (0.0028)	0.0000 (0.0001)	1.0064*** (0.0097)
Post-catastrophe year ( $\delta^2$ )	-0.0146 (0.0392)	0.5477*** (0.0257)	0.0010 (0.0008)	0.0002 (0.0008)	-0.0002 (0.0002)	0.0040 (0.0264)	0.5895*** (0.0178)	0.0011 (0.0008)	0.0003 (0.0008)	-0.0002 (0.0002)
Applications to Housing Programs (IHP/HA) (%)	0.0003 (0.0032)	-0.0009 (0.0032)	-0.0010 (0.0025)	-0.0009 (0.0009)	0.0000 (0.0001)	-0.0002 (0.0033)	-0.0013 (0.0033)	-0.0011 (0.0025)	-0.0009 (0.0009)	0.0000 (0.0001)
Applications reporting a need for shelter (%)	-0.0005 (0.0044)	-0.0009 (0.0045)	0.0001 (0.0029)	-0.0016 (0.0014)	-0.0000 (0.0001)	-0.0005 (0.0044)	-0.0007 (0.0045)	0.0001 (0.0029)	-0.0016 (0.0014)	-0.0001 (0.0001)
Applications reporting a need for special accommodations (%)	-0.0069 (0.0737)	0.0061 (0.0741)	-0.0052 (0.0522)	0.0395 (0.0281)	0.0003 (0.0006)	0.0042 (0.0733)	0.0133 (0.0736)	-0.0053 (0.0522)	0.0398 (0.0281)	0.0003 (0.0006)
Applications reporting a need for emergency items or food (%)	-0.0005 (0.0049)	0.0004 (0.0050)	0.0010 (0.0056)	-0.0004 (0.0012)	-0.0000 (0.0001)	-0.0006 (0.0049)	0.0004 (0.0050)	0.0011 (0.0036)	-0.0004 (0.0012)	-0.0000 (0.0001)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.0081 (0.0057)	-0.0079 (0.0057)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	-0.0063 (0.0055)	-0.0062 (0.0055)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.0080 (0.0058)	-0.0081 (0.0058)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	-0.0037 (0.0041)	-0.0038 (0.0041)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.0017 (0.0055)	0.0020 (0.0055)	-0.0002 (0.0009)	0.0000 (0.0001)	0.0002 (0.0003)	-0.0000 (0.0047)	0.0002 (0.0047)	-0.0003 (0.0009)	0.0000 (0.0001)	0.0002 (0.0003)
Hurricane Maria period (16 - 30 Sep 2017)	0.0022 (0.0039)	0.0023 (0.0039)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	0.0048 (0.0032)	0.0049 (0.0032)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Observations	79,369	79,369	79,369	79,369	79,369	79,369	79,369	79,369	79,369	79,369
Method	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Counties	All	All	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F test of excluded instruments: F-stat=	102.118	62.983	4856.648	5030.770	3065.836	187.473	106.768	5056.320	5050.771	3247.471
F test of excluded instruments: p-value=	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: This table reports the first-stage regression results from IV demand estimations (Eq.2.7-Eq.2.11) across all counties in the state of Texas. The product is toilet paper (TP) in all columns. In columns 6 through 10, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.7: First-stage regression results from IV demand estimations for batteries - All counties in the state of Texas

VARIABLES	(1) BT (Eq.7)	(2) BT (Eq.8)	(3) BT (Eq.9)	(4) BT (Eq.10)	(5) BT (Eq.11)	(6) BT (Eq.7)	(7) BT (Eq.8)	(8) BT (Eq.9)	(9) BT (Eq.10)	(10) BT (Eq.11)
Price index instrument ( $\delta^1$ )	0.7067*** (0.0447)	-0.0037*** (0.0009)	-0.0002* (0.0001)	-0.0001 (0.0000)	0.0000 (0.0000)	0.4993*** (0.0236)	-0.0023*** (0.0006)	-0.0001* (0.0001)	-0.0001 (0.0000)	0.0000 (0.0000)
Price index instrument x Post ( $\delta^3$ )	-0.0362 (0.0626)	0.6777*** (0.0440)	-0.0054** (0.0025)	-0.0010 (0.0011)	0.0002 (0.0001)	0.0167 (0.0362)	0.5209*** (0.0276)	-0.0057** (0.0024)	-0.0011 (0.0011)	0.0002 (0.0001)
Price index instrument x Warning for actual Harvey ( $\delta^4$ )	-0.0003 (0.0104)	-0.0009 (0.0105)	1.0027*** (0.0102)	-0.0052* (0.0030)	-0.0007 (0.0008)	0.0061 (0.0084)	0.0057 (0.0085)	1.0034*** (0.0092)	-0.0048* (0.0026)	-0.0007 (0.0008)
Price index instrument x During actual landfall ( $\delta^5$ )	0.0306** (0.0138)	0.0288** (0.0139)	0.0117 (0.0103)	1.0256*** (0.0122)	0.0003 (0.0003)	0.0291** (0.0120)	0.0277** (0.0120)	0.0103 (0.0097)	1.0233*** (0.0102)	0.0003 (0.0003)
Price index instrument x After actual strike (1-3 weeks) ( $\delta^6$ )	0.0042 (0.0341)	0.0039 (0.0351)	-0.0067 (0.0086)	0.0002 (0.0009)	1.0153*** (0.0296)	0.0089 (0.0379)	0.0087 (0.0386)	-0.0061 (0.0085)	0.0002 (0.0008)	1.0153*** (0.0296)
Post-catastrophe year ( $\delta^2$ )	0.0366 (0.0622)	0.3257*** (0.0437)	0.0053** (0.0025)	0.0010 (0.0011)	-0.0002 (0.0001)	-0.0177 (0.0360)	-0.4761*** (0.0274)	0.0057** (0.0024)	0.0012 (0.0011)	-0.0002 (0.0001)
Applications to Housing Programs (IHP/HA) (%)	-0.0036 (0.0036)	-0.0049 (0.0035)	-0.0065* (0.0035)	-0.0044* (0.0023)	0.0001 (0.0001)	-0.0043 (0.0034)	-0.0047 (0.0034)	-0.0061* (0.0034)	-0.0042* (0.0022)	0.0001 (0.0001)
Applications reporting a need for shelter (%)	-0.0032 (0.0047)	-0.0033 (0.0048)	0.0052 (0.0036)	-0.0024 (0.0032)	-0.0001 (0.0001)	-0.0018 (0.0044)	-0.0020 (0.0045)	0.0055 (0.0034)	-0.0022 (0.0031)	-0.0001 (0.0001)
Applications reporting a need for special accommodations (%)	-0.0658 (0.0741)	-0.0225 (0.0741)	-0.0323 (0.0691)	0.0240 (0.0349)	0.0003 (0.0005)	-0.0425 (0.0710)	-0.0116 (0.0710)	-0.0324 (0.0663)	0.0200 (0.0328)	0.0003 (0.0005)
Applications reporting a need for emergency items or food (%)	0.0090* (0.0051)	0.0085 (0.0051)	0.0079* (0.0046)	0.0057** (0.0028)	-0.0001 (0.0001)	0.0081* (0.0048)	0.0071 (0.0048)	0.0074* (0.0044)	0.0055** (0.0027)	-0.0001 (0.0001)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.0178** (0.0080)	-0.0183** (0.0079)	0.0001 (0.0001)	-0.0000 (0.0000)	-0.0000 (0.0000)	-0.0135* (0.0071)	-0.0140** (0.0071)	0.0001 (0.0001)	-0.0000 (0.0000)	-0.0000 (0.0000)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	0.0066 (0.0146)	0.0062 (0.0146)	0.0000 (0.0001)	-0.0000 (0.0000)	0.0000 (0.0000)	0.0040 (0.0080)	0.0036 (0.0080)	-0.0000 (0.0001)	-0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.0037 (0.0087)	0.0052 (0.0087)	0.0029 (0.0029)	0.0015* (0.0008)	0.0003 (0.0005)	0.0006 (0.0061)	0.0018 (0.0061)	0.0018 (0.0027)	0.0014* (0.0007)	0.0003 (0.0005)
Hurricane Maria period (16 - 30 Sep 2017)	-0.0273** (0.0111)	-0.0267** (0.0112)	-0.0000 (0.0001)	-0.0000 (0.0000)	0.0000 (0.0000)	-0.0171** (0.0080)	-0.0167** (0.0080)	-0.0000 (0.0001)	-0.0000 (0.0000)	0.0000 (0.0000)
Observations	34,339	34,339	34,339	34,339	34,339	34,339	34,339	34,339	34,339	34,339
Method	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Counties	All	All	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F test of excluded instruments: F-stat=	97.501	56.174	3026.921	4355.320	464.349	161.579	79.419	3548.208	5114.253	464.349
F test of excluded instruments: p-value=	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: This table reports the first-stage regression results from IV demand estimations (Eq.2.7-Eq.2.11) across all counties in the state of Texas. The product is batteries (BT) in all columns. In columns 6 through 10, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.8: First-stage regression results from IV demand estimations for milk - All counties in the state of Texas

VARIABLES	(1) MK (Eq.7)	(2) MK (Eq.8)	(3) MK (Eq.9)	(4) MK (Eq.10)	(5) MK (Eq.11)	(6) MK (Eq.7)	(7) MK (Eq.8)	(8) MK (Eq.9)	(9) MK (Eq.10)	(10) MK (Eq.11)
Price index instrument ( $\delta^1$ )	0.3387*** (0.0363)	0.0107*** (0.0033)	-0.0001 (0.0002)	0.0000 (0.0001)	0.0000 (0.0001)	0.2914*** (0.0297)	0.0083*** (0.0027)	-0.0001 (0.0002)	0.0000 (0.0001)	0.0000 (0.0001)
Price index instrument x Post ( $\delta^2$ )	-0.1745*** (0.0610)	0.1538*** (0.0491)	-0.0287 (0.0228)	0.0005 (0.0009)	-0.0009 (0.0007)	-0.1223*** (0.0455)	0.1612*** (0.0345)	-0.0275 (0.0216)	0.0004 (0.0008)	-0.0009 (0.0007)
Price index instrument x Warning for actual Harvey ( $\delta^4$ )	-0.0164 (0.0327)	-0.0164 (0.0325)	0.9165*** (0.0606)	-0.0049 (0.0037)	0.0050 (0.0060)	-0.0097 (0.0296)	-0.0103 (0.0294)	0.9206*** (0.0578)	-0.0042 (0.0035)	0.0050 (0.0060)
Price index instrument x During actual landfall ( $\delta^5$ )	-0.0194 (0.0297)	-0.0205 (0.0301)	0.0093 (0.0265)	0.9681*** (0.0288)	-0.0013 (0.0018)	-0.0197 (0.0291)	-0.0203 (0.0293)	0.0111 (0.0253)	0.9709*** (0.0284)	-0.0013 (0.0018)
Price index instrument x After actual strike (1-3 weeks) ( $\delta^6$ )	0.0293 (0.0369)	0.0294 (0.0377)	0.0519 (0.0446)	0.0009 (0.0011)	1.0038*** (0.0338)	0.0316 (0.0341)	0.0318 (0.0350)	0.0505 (0.0435)	0.0007 (0.0010)	1.0038*** (0.0338)
Post-catastrophe year ( $\delta^2$ )	0.1785*** (0.0606)	0.8358*** (0.0487)	0.0285 (0.026)	-0.0006 (0.0009)	0.0009 (0.0006)	0.1264*** (0.0452)	0.8288*** (0.0341)	0.0273 (0.0215)	-0.0004 (0.0008)	0.0009 (0.0006)
Applications to Housing Programs (IHP/HA) (%)	-0.0259 (0.0170)	-0.0295* (0.0169)	-0.0179 (0.0164)	-0.0175* (0.0100)	-0.0004 (0.0006)	-0.0264 (0.0167)	-0.0300* (0.0166)	-0.0179 (0.0162)	-0.0171* (0.0098)	-0.0004 (0.0006)
Applications reporting a need for shelter (%)	0.0591** (0.0240)	0.0601*** (0.0228)	0.0223 (0.0364)	0.0068 (0.0057)	0.0009 (0.0013)	0.0579** (0.0228)	0.0592*** (0.0219)	0.0219 (0.0358)	0.0058 (0.0052)	0.0009 (0.0013)
Applications reporting a need for special accommodations (%)	-0.0327 (0.2500)	-0.0064 (0.2433)	0.2799 (0.2565)	-0.0078 (0.1454)	0.2655* (0.0101)	-0.0242 (0.2435)	-0.0040 (0.2383)	0.2752 (0.2524)	0.2653* (0.1437)	-0.0078 (0.0101)
Applications reporting a need for emergency items or food (%)	0.0170 (0.0205)	0.0198 (0.0199)	0.0072 (0.0176)	0.0088 (0.0069)	0.0004 (0.0006)	0.0175 (0.0198)	0.0204 (0.0194)	0.0074 (0.0174)	0.0087 (0.0069)	0.0004 (0.0006)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.0345** (0.0145)	-0.0344** (0.0145)	-0.0003 (0.0004)	0.0001* (0.0000)	0.0000 (0.0000)	-0.0282** (0.0126)	-0.0281** (0.0125)	-0.0003 (0.0004)	0.0001* (0.0000)	0.0000 (0.0000)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	0.0063 (0.0086)	0.0062 (0.0086)	0.0001 (0.0002)	0.0001** (0.0000)	0.0001 (0.0000)	0.0060 (0.0085)	0.0059 (0.0085)	0.0001 (0.0002)	0.0001** (0.0000)	0.0001 (0.0000)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	-0.0223 (0.0210)	-0.0227 (0.0211)	-0.0054 (0.0067)	0.0008 (0.0006)	0.0004 (0.0012)	-0.0259* (0.0153)	-0.0261* (0.0153)	-0.0052 (0.0064)	0.0007 (0.0005)	0.0004 (0.0012)
Hurricane Maria period (16 - 30 Sep 2017)	0.0327** (0.0157)	0.0326** (0.0158)	-0.0003 (0.0003)	0.0001** (0.0000)	0.0000 (0.0000)	0.0241** (0.0121)	0.0239** (0.0122)	-0.0003 (0.0003)	0.0001** (0.0000)	0.0000 (0.0000)
Observations	16,043	16,043	16,043	16,043	16,043	16,043	16,043	16,043	16,043	16,043
Method	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Counties	All	All	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F test of excluded instruments: F-stat=	20.227	5.486	348.157	326.059	289.677	24.854	7.803	374.715	434.889	289.677
F test of excluded instruments: p-value=	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: This table reports the first-stage regression results from IV demand estimations (Eq.2.7-Eq.2.11) across all counties in the state of Texas. The product is milk (MK) in all columns. In columns 6 through 10, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.9: First-stage regression results from IV demand estimations for bottled water - All counties in the state of Texas

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	BW (Eq.7)	BW (Eq.8)	BW (Eq.9)	BW (Eq.10)	BW (Eq.11)	BW (Eq.7)	BW (Eq.8)	BW (Eq.9)	BW (Eq.10)	BW (Eq.11)
Price index instrument ( $\delta^1$ )	0.3774*** (0.0240)	-0.0004 (0.0004)	0.0000 (0.0000)	-0.0000 (0.0000)	0.0000 (0.0000)	0.2765*** (0.0127)	-0.0007** (0.0003)	0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Price index instrument x Post ( $\delta^3$ )	-0.0579* (0.0340)	0.3216*** (0.0240)	-0.0030** (0.0013)	-0.0050** (0.0025)	-0.0015 (0.0013)	-0.0265 (0.0210)	0.2525*** (0.0168)	-0.0040*** (0.0011)	-0.0048* (0.0025)	-0.0017* (0.0010)
Price index instrument x Warning for actual Harvey ( $\delta^4$ )	0.0231** (0.0102)	0.0206** (0.0102)	1.0206*** (0.0092)	0.0066** (0.0032)	-0.0022 (0.0017)	0.0188*** (0.0072)	0.0163** (0.0072)	1.0140*** (0.0064)	0.0065** (0.0030)	-0.0021 (0.0017)
Price index instrument x During actual landfall ( $\delta^5$ )	-0.0147 (0.0114)	-0.0143 (0.0114)	-0.0088 (0.0081)	0.9998*** (0.0127)	0.0007 (0.0005)	-0.0075 (0.0086)	-0.0071 (0.0086)	-0.0044 (0.0061)	0.9982*** (0.0113)	0.0007 (0.0005)
Price index instrument x After actual strike (1-3 weeks) ( $\delta^6$ )	-0.0086 (0.0110)	-0.0086 (0.0110)	-0.0071 (0.0049)	-0.0006 (0.0004)	0.9931*** (0.0107)	-0.0077 (0.0088)	-0.0077 (0.0088)	-0.0068 (0.0046)	-0.0006 (0.0004)	0.9916*** (0.0094)
Post-catastrophe year ( $\delta^2$ )	0.0655* (0.0336)	0.6793*** (0.0239)	0.0030** (0.0013)	0.0050** (0.0025)	0.0015 (0.0013)	0.0346* (0.0209)	0.7448*** (0.0167)	0.0041*** (0.0011)	0.0047* (0.0025)	0.0016* (0.0010)
Applications to Housing Programs (IHP/HA) (%)	0.0070 (0.0046)	0.0071 (0.0046)	0.0069 (0.0045)	0.0017 (0.0016)	0.0002 (0.0002)	0.0070* (0.0039)	0.0070* (0.0039)	0.0068* (0.0037)	0.0017 (0.0014)	0.0002 (0.0002)
Applications reporting a need for shelter (%)	-0.0054 (0.0034)	-0.0059* (0.0034)	-0.0059* (0.0031)	-0.0009 (0.0014)	-0.0002 (0.0002)	-0.0041 (0.0031)	-0.0047 (0.0031)	-0.0050* (0.0028)	-0.0007 (0.0013)	-0.0002 (0.0002)
Applications reporting a need for special accommodations (%)	0.1725*** (0.0543)	0.1731*** (0.0543)	0.1656*** (0.0518)	0.0055 (0.0201)	0.0002 (0.0005)	0.1388*** (0.0438)	0.1398*** (0.0439)	0.1295*** (0.0403)	0.0015 (0.0175)	0.0002 (0.0005)
Applications reporting a need for emergency items or food (%)	-0.0160*** (0.0054)	-0.0159*** (0.0054)	-0.0153*** (0.0052)	-0.0023 (0.0018)	-0.0001 (0.0001)	-0.0148*** (0.0048)	-0.0147*** (0.0048)	-0.0136*** (0.0046)	-0.0021 (0.0016)	-0.0001 (0.0001)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	0.0091** (0.0044)	0.0089** (0.0044)	-0.0001** (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	0.0103*** (0.0040)	0.0101** (0.0040)	-0.0000* (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	0.0081 (0.0049)	0.0078 (0.0049)	-0.0001* (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	0.0077** (0.0037)	0.0075** (0.0037)	-0.0000* (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	-0.0025 (0.0054)	-0.0023 (0.0054)	-0.0034* (0.0018)	-0.0012** (0.0006)	0.0014 (0.0010)	0.0008 (0.0040)	0.0010 (0.0040)	-0.0019 (0.0013)	-0.0012** (0.0005)	0.0015* (0.0008)
Hurricane Maria period (16 - 30 Sep 2017)	0.0015 (0.0034)	0.0015 (0.0034)	0.0000 (0.0000)	0.0000 (0.0000)	-0.0000 (0.0000)	0.0029 (0.0030)	0.0029 (0.0030)	-0.0000 (0.0000)	0.0000 (0.0000)	-0.0000* (0.0000)
Observations	97,763	97,763	97,763	97,763	97,763	97,763	97,763	97,763	97,763	97,763
Method	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
Counties	All	All	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F test of excluded instruments: F-stat=	85.646	37.765	5733.425	2969.051	3792.394	140.187	48.856	7096.319	4052.700	3970.868
F test of excluded instruments: p-value=	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: This table reports the first-stage regression results from IV demand estimations (Eq.2.7-Eq.2.11) across all counties in the state of Texas. The product is bottled water (BW) in all columns. In columns 6 through 10, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels:

\*\*\* 1%, \*\* 5%, \* 10%.

Table A.10: Demand changes due to warning for Harvey, during landfall, and after strike, using IV estimations - All counties in the state of Texas

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Price index ( $\vartheta^{pre}$ )	-0.666** (0.275)	-0.618*** (0.157)	0.459 (0.576)	-0.069 (0.347)	-0.706** (0.292)	-0.874*** (0.217)	0.531 (0.667)	-0.099 (0.473)
Price index x Post ( $\beta^{post}$ )	1.381*** (0.327)	-0.383* (0.227)	-0.530 (1.176)	-2.431*** (0.579)	1.497*** (0.350)	-0.427 (0.307)	-0.598 (1.195)	-3.090*** (0.754)
Price index x Warning for actual Harvey ( $\beta^{\varpi}$ )	-0.013 (0.080)	-0.094 (0.099)	-0.189 (0.133)	0.292*** (0.100)	-0.015 (0.080)	-0.085 (0.099)	-0.188 (0.132)	0.294*** (0.099)
Price index x During actual landfall ( $\beta^h$ )	-0.086 (0.085)	0.048 (0.109)	0.046 (0.182)	0.531*** (0.110)	-0.088 (0.085)	0.055 (0.110)	0.046 (0.181)	0.544*** (0.110)
Price index x After actual Harvey strike (1-3 weeks) ( $\beta^{\xi}$ )	-0.163 (0.177)	0.056 (0.342)	-0.418** (0.202)	0.596*** (0.126)	-0.166 (0.177)	0.063 (0.330)	-0.419** (0.202)	0.593*** (0.128)
Post-catastrophe year ( $\alpha$ )	-1.442*** (0.325)	0.425* (0.227)	0.466 (1.161)	2.532*** (0.577)	-1.555*** (0.347)	0.465 (0.306)	0.534 (1.179)	3.181*** (0.749)
Applications to Housing Programs (IHP/HA) (%)	0.029 (0.042)	0.001 (0.062)	0.113 (0.094)	-0.080 (0.056)	0.029 (0.042)	-0.000 (0.062)	0.112 (0.094)	-0.075 (0.056)
Applications reporting a need for shelter (%)	-0.055 (0.049)	-0.029 (0.060)	-0.237 (0.227)	0.105* (0.063)	-0.055 (0.049)	-0.028 (0.060)	-0.237 (0.226)	0.105 (0.064)
Applications reporting a need for special accommodations (%)	-0.052 (0.738)	-1.239* (0.689)	-0.643 (1.434)	-0.405 (0.816)	-0.055 (0.738)	-1.232* (0.697)	-0.646 (1.434)	-0.379 (0.816)
Applications reporting a need for emergency items or food (%)	-0.016 (0.065)	0.087 (0.081)	-0.025 (0.096)	0.081 (0.090)	-0.016 (0.065)	0.088 (0.081)	-0.024 (0.096)	0.074 (0.091)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	0.082* (0.044)	0.020 (0.088)	-0.050 (0.098)	0.300*** (0.073)	0.081* (0.044)	0.020 (0.089)	-0.049 (0.096)	0.310*** (0.073)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	0.009 (0.035)	-0.018 (0.074)	0.044 (0.066)	0.183*** (0.061)	0.006 (0.073)	-0.019 (0.065)	0.044 (0.065)	0.187*** (0.060)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	-0.018 (0.044)	0.072 (0.075)	-0.085 (0.084)	0.048 (0.060)	-0.017 (0.044)	0.069 (0.076)	-0.085 (0.085)	0.057 (0.059)
Hurricane Maria period (16 - 30 Sep 2017)	0.030 (0.036)	-0.001 (0.070)	-0.096 (0.072)	0.047 (0.057)	0.028 (0.036)	0.004 (0.070)	-0.097 (0.068)	0.052 (0.057)
Observations	79,369	34,339	16,043	97,763	79,369	34,339	16,043	97,763
R-squared	-0.008	-0.008	-0.003	-0.015	-0.006	-0.005	-0.004	-0.012
Method	IV	IV	IV	IV	IV	IV	IV	IV
Counties	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Endogeneity test of endogenous regressors: Chi-square=	31.655	34.278	5.119	26.788	33.721	33.989	5.709	25.846
Endogeneity test of endogenous regressors: p-value=	0.000	0.000	0.402	0.000	0.000	0.000	0.336	0.000
Elasticity in pre-catastrophe=	-0.661	-0.616	0.453	-0.069	-0.701	-0.869	0.525	-0.098
Testing elasticity in pre-catastrophe=0, p-value=	0.015	0.000	0.426	0.842	0.016	0.000	0.426	0.835
Change in elasticity parameter at warning ( $\beta^{post} + \beta^{\varpi}$ )=	1.367	-0.476			1.482	-0.512		
Testing ( $\beta^{post} + \beta^{\varpi}$ ) = 0, p-value=	0.000	0.054			0.000	0.112		
Efficient price change at warning ( $\lambda^{\varpi}$ )=	3.087	0.000			2.970	0.000		
Change in elasticity parameter at landfall ( $\beta^{post} + \beta^h$ )=	1.294	-0.335			1.409	-0.372		
Testing ( $\beta^{post} + \beta^h$ ) = 0, p-value=	0.000	0.179			0.000	0.251		
Efficient price change at landfall ( $\lambda^h$ )=	2.922	0.000			2.823	0.000		
Change in elasticity parameter after strike ( $\beta^{post} + \beta^{\xi}$ )=	1.218	-0.327			1.331	-0.363		
Testing ( $\beta^{post} + \beta^{\xi}$ ) = 0, p-value=	0.001	0.428			0.001	0.422		
Efficient change after strike ( $\lambda^{\xi}$ )=	2.749	0.000			2.667	0.000		

Notes: This table reports the results from IV demand estimations using a flexible, semi-log functional form (Eq.2.5). The estimation sample covers all four years (2014-2017) and includes all counties in the state of Texas. Dependent variable is log store-UPC-level total quantity purchased. Using Eq.2.5, the elasticity coefficient in pre-catastrophe years is  $\eta = (\vartheta^{pre}) * \bar{P}$  evaluated at the estimation sample mean. Efficient price changes are set to zero if the change in elasticity parameter is not significant at the 5% level. In columns 3 and 4, efficient price changes are not computed because  $\vartheta^{pre}$  is not significant at the 5% level. The product is toilet paper (TP) in columns 1 and 5, batteries (BT) in columns 2 and 6, milk (MK) in columns 3 and 7, and bottled water (BW) in columns 4 and 8. In columns 5 through 8, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.II: Assessment of price changes due to warning for Harvey, during landfall, and after strike, using IV estimates of demand changes - All counties of the state of Texas

VARIABLES	(1) TP	(2) BT	(3) TP	(4) BT
Post-catastrophe year ( $\ell$ )	0.008*** (0.001)	0.001 (0.002)	0.008*** (0.001)	-0.001 (0.001)
Warning for actual Harvey ( $\varpi$ )	0.000 (0.009)	-0.006 (0.011)	0.002 (0.009)	0.000 (0.009)
During actual Harvey landfall ( $h$ )	0.015 (0.010)	0.031** (0.015)	0.015 (0.010)	0.029** (0.013)
After actual Harvey strike (1-3 weeks) ( $\S$ )	-0.005 (0.015)	-0.009 (0.046)	-0.001 (0.014)	-0.002 (0.047)
Applications to Housing Programs (IHP/HA) (%)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)	-0.003 (0.004)
Applications reporting a need for shelter (%)	-0.002 (0.005)	-0.004 (0.005)	-0.002 (0.005)	-0.003 (0.005)
Applications reporting a need for special accommodations (%)	0.038 (0.073)	-0.062 (0.084)	0.046 (0.073)	-0.037 (0.080)
Applications reporting a need for emergency items or food (%)	0.001 (0.005)	0.008 (0.006)	0.000 (0.005)	0.007 (0.006)
Hurricane Hermine period (28 Aug 2016 - 3 Sep 2016)	-0.009 (0.006)	-0.010 (0.009)	-0.007 (0.006)	-0.008 (0.008)
Hurricane Matthew period (28 Sep 2016 - 9 Oct 2016)	-0.010 (0.006)	0.004 (0.015)	-0.005 (0.004)	0.002 (0.008)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.001 (0.006)	0.004 (0.010)	-0.001 (0.005)	0.001 (0.007)
Hurricane Maria period (16 - 30 Sep 2017)	0.003 (0.004)	-0.030** (0.012)	0.005 (0.003)	-0.019** (0.008)
Constant	0.991*** (0.001)	1.001*** (0.002)	0.989*** (0.001)	0.997*** (0.001)
Observations	79,369	34,339	79,369	34,339
R-squared	0.005	0.009	0.006	0.010
Counties	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes
Efficient price change at warning ( $\lambda^{\varpi}$ )=	3.087	0.000	2.970	0.000
Observed price change at warning ( $\varpi + \ell$ )=	0.008	-0.005	0.010	0.000
Testing observed price change at warning ( $\varpi + \ell$ ) = $\lambda^{\varpi}$ , p-value=	0.000	0.661	0.000	0.981
Efficient price change at landfall ( $\lambda^h$ )=	2.922	0.000	2.823	0.000
Observed price change at landfall ( $h + \ell$ )=	0.023	0.033	0.023	0.029
Testing observed price change at landfall ( $h + \ell$ ) = $\lambda^h$ , p-value=	0.000	0.027	0.000	0.025
Efficient change after strike ( $\lambda^{\S}$ )=	2.749	0.000	2.667	0.000
Observed price change after strike ( $\S + \ell$ )=	0.003	-0.007	0.007	-0.003
Testing observed price change after strike ( $\S + \ell$ ) = $\lambda^{\S}$ , p-value=	0.000	0.872	0.000	0.953

Notes: This table reports the results from price estimations using Eq.2.6. The estimation sample covers all four years (2014-2017) and includes all counties in the state of Texas. Dependent variable is store-UPC-level price index. In columns 3 through 4, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. The product is toilet paper (TP) in columns 1 and 3, batteries (BT) in columns 2 and 4. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.12: Demand changes due to warning for Harvey, during landfall, and after strike - All counties of the state of Texas, Base year 2014

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Price index ( $\vartheta^{pre}$ )	-0.081 (0.066)	-0.222** (0.087)	-0.426*** (0.162)	-0.504*** (0.091)	-0.147* (0.078)	-0.296** (0.125)	-0.498*** (0.187)	-0.731*** (0.123)
Post-catastrophe year ( $\alpha$ )	0.008 (0.090)	0.139 (0.129)	-0.362* (0.185)	-0.179 (0.136)	0.033 (0.109)	0.065 (0.181)	-0.325 (0.227)	-0.214 (0.180)
Price index x Post ( $\beta^{post}$ )	-0.104 (0.090)	-0.078 (0.128)	0.307* (0.185)	0.341** (0.134)	-0.129 (0.109)	-0.005 (0.181)	0.269 (0.228)	0.378** (0.179)
Price index x Warning for actual Harvey ( $\beta^w$ )	0.006 (0.076)	-0.053 (0.095)	-0.190 (0.133)	0.224** (0.093)	0.003 (0.075)	-0.053 (0.095)	-0.187 (0.133)	0.229** (0.093)
Price index x During actual Harvey landfall ( $\beta^h$ )	-0.011 (0.084)	0.062 (0.103)	0.161 (0.167)	0.480*** (0.110)	-0.014 (0.084)	0.064 (0.103)	0.159 (0.166)	0.486*** (0.111)
Price index x After actual Harvey strike (1-3 weeks) ( $\beta^s$ )	-0.157 (0.178)	0.015 (0.345)	-0.246 (0.234)	0.691*** (0.116)	-0.137 (0.178)	0.017 (0.345)	-0.243 (0.233)	0.693*** (0.117)
Applications to Housing Programs (IHP/HA) (%)	0.020 (0.042)	0.018 (0.058)	0.073 (0.083)	-0.079 (0.058)	0.020 (0.042)	0.019 (0.058)	0.070 (0.083)	-0.077 (0.058)
Applications reporting a need for shelter (%)	-0.072 (0.055)	0.010 (0.066)	-0.183 (0.216)	0.106* (0.062)	-0.073 (0.055)	0.010 (0.066)	-0.176 (0.216)	0.105* (0.062)
Applications reporting a need for special accommodations (%)	0.574 (0.735)	-1.054 (0.769)	-0.971 (1.413)	-0.455 (0.806)	0.581 (0.734)	-1.052 (0.768)	-0.982 (1.403)	-0.426 (0.805)
Applications reporting a need for emergency items or food (%)	-0.031 (0.067)	0.034 (0.089)	0.026 (0.085)	0.080 (0.092)	-0.031 (0.066)	0.033 (0.089)	0.028 (0.084)	0.078 (0.092)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.001 (0.043)	0.064 (0.072)	-0.034 (0.079)	0.050 (0.059)	0.001 (0.043)	0.064 (0.072)	-0.037 (0.079)	0.050 (0.059)
Hurricane Maria period (16 - 30 Sep 2017)	0.047 (0.036)	0.010 (0.067)	-0.049 (0.065)	0.027 (0.057)	0.048 (0.036)	0.012 (0.067)	-0.048 (0.065)	0.028 (0.057)
Observations	38,592	17,516	8,025	49,521	38,592	17,516	8,025	49,521
R-squared	0.014	0.005	0.078	0.018	0.014	0.005	0.078	0.018
Counties	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Elasticity in pre-catastrophe=	-0.081	-0.221	-0.421	-0.503	-0.146	-0.295	-0.493	-0.727
Testing elasticity in pre-catastrophe=0, p-value=	0.221	0.011	0.008	0.000	0.059	0.018	0.008	0.000
Change in elasticity parameter at warning ( $\beta^{post} + \beta^w$ )=		-0.131	0.117	0.565	-0.126	-0.058	0.082	0.607
Testing ( $\beta^{post} + \beta^w$ ) = 0, p-value=		0.405	0.607	0.000	0.342	0.776	0.755	0.002
Efficient price change at warning ( $\lambda^w$ )=		0.000	0.000	2.224	0.000	0.000	0.000	1.137
Change in elasticity parameter at landfall ( $\beta^{post} + \beta^h$ )=		-0.017	0.468	0.821	-0.143	0.060	0.428	0.865
Testing ( $\beta^{post} + \beta^h$ ) = 0, p-value=		0.918	0.061	0.000	0.300	0.774	0.130	0.000
Efficient price change at landfall ( $\lambda^h$ )=		0.000	0.000	3.231	0.000	0.000	0.000	1.620
Change in elasticity parameter after strike ( $\beta^{post} + \beta^s$ )=		-0.063	0.060	1.032	-0.286	0.012	0.026	1.071
Testing ( $\beta^{post} + \beta^s$ ) = 0, p-value=		0.864	0.840	0.000	0.170	0.976	0.936	0.000
Efficient change after strike ( $\lambda^s$ )=		0.000	0.000	4.063	0.000	0.000	0.000	2.007

Notes: This table reports the results from demand estimations using a flexible, semi-log functional form (Eq.2.5). The estimation sample covers years 2014 and 2017 and includes all counties in the state of Texas. Dependent variable is log store-UPC-level total quantity purchased. Using Eq.2.5, the elasticity coefficient in pre-catastrophe years is  $\eta = (\vartheta^{pre}) * \bar{P}$  evaluated at the estimation sample mean. Efficient price changes are set to zero if the change in elasticity parameter is not significant at the 5% level. In column 1, efficient price changes are not computed because  $\vartheta^{pre}$  is not significant at the 5% level. The product is toilet paper (TP) in columns 1 and 5, batteries (BT) in columns 2 and 6, milk (MK) in columns 3 and 7, and bottled water (BW) in columns 4 and 8. In columns 5 through 8, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.13: Demand changes due to warning for Harvey, during landfall, and after strike, using IV estimations - All counties in the state of Texas, Base year 2014

VARIABLES	(1) TP	(2) BT	(3) MK	(4) BW	(5) TP	(6) BT	(7) MK	(8) BW
Price index ( $\vartheta^{Pre}$ )	-0.639 (0.448)	-0.782** (0.320)	-0.969 (0.674)	0.437 (0.556)	-0.659 (0.460)	-0.990** (0.403)	-1.024 (0.723)	0.585 (0.736)
Price index x Post ( $\beta^{Post}$ )	1.573*** (0.519)	-0.163 (0.385)	3.650 (4.252)	-2.338** (0.988)	1.692*** (0.540)	-0.317 (0.515)	3.147 (2.674)	-2.932** (1.245)
Price index x Warning for actual Harvey ( $\beta^{\varpi}$ )	-0.008 (0.080)	-0.100 (0.100)	-0.136 (0.147)	0.273*** (0.099)	-0.010 (0.080)	-0.093 (0.100)	-0.159 (0.131)	0.272*** (0.098)
Price index x During actual landfall ( $\beta^h$ )	-0.079 (0.085)	0.037 (0.109)	0.096 (0.229)	0.508*** (0.110)	-0.081 (0.086)	0.045 (0.110)	0.085 (0.211)	0.517*** (0.110)
Price index x After actual Harvey strike (1-3 weeks) ( $\beta^s$ )	-0.162 (0.178)	0.048 (0.348)	-0.469* (0.250)	0.594*** (0.124)	-0.166 (0.178)	0.054 (0.333)	-0.462** (0.231)	0.593*** (0.125)
Post-catastrophe year ( $\alpha$ )	-1.669*** (0.515)	0.235 (0.385)	-3.675 (4.209)	2.505** (0.986)	-1.785*** (0.534)	0.385 (0.513)	-3.176 (2.644)	3.090** (1.238)
Applications to Housing Programs (IHP/HA) (%)	0.030 (0.042)	0.011 (0.063)	0.201 (0.167)	-0.074 (0.057)	0.031 (0.042)	0.010 (0.063)	0.185 (0.130)	-0.071 (0.057)
Applications reporting a need for shelter (%)	-0.057 (0.049)	-0.032 (0.060)	-0.402 (0.361)	0.106* (0.063)	-0.057 (0.049)	-0.032 (0.061)	-0.360 (0.281)	0.105* (0.063)
Applications reporting a need for special accommodations (%)	0.062 (0.740)	-1.342* (0.695)	-0.415 (1.814)	-0.375 (0.825)	0.057 (0.740)	-1.330* (0.704)	-0.480 (1.700)	-0.370 (0.823)
Applications reporting a need for emergency items or food (%)	-0.021 (0.065)	0.080 (0.082)	-0.096 (0.150)	0.069 (0.092)	-0.021 (0.066)	0.081 (0.082)	-0.085 (0.126)	0.066 (0.092)
Hurricane Irma period (30 Aug 2017 - 12 Sep 2017)	0.004 (0.045)	0.048 (0.076)	-0.004 (0.130)	0.007 (0.060)	0.006 (0.045)	0.045 (0.076)	-0.012 (0.106)	0.011 (0.060)
Hurricane Maria period (16 - 30 Sep 2017)	0.053 (0.037)	-0.022 (0.071)	-0.157 (0.134)	0.005 (0.057)	0.050 (0.037)	-0.019 (0.071)	-0.125 (0.084)	0.007 (0.057)
Observations	36,850	16,143	7,341	45,580	36,850	16,143	7,341	45,580
R-squared	-0.010	-0.004	-0.150	-0.008	-0.006	-0.005	-0.051	-0.005
Method	IV	IV	IV	IV	IV	IV	IV	IV
Counties	All	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	No	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Endogeneity test of endogenous regressors: Chi-square=	23.286	14.649	2.217	9.940	25.373	16.617	2.444	10.407
Endogeneity test of endogenous regressors: p-value=	0.000	0.012	0.818	0.077	0.000	0.005	0.785	0.065
Elasticity in pre-catastrophe=	-0.634	-0.779	-0.959	0.436	-0.653	-0.984	-1.014	0.582
Testing elasticity in pre-catastrophe=0, p-value=	0.154	0.014	0.151	0.431	0.152	0.014	0.156	0.427
Change in elasticity parameter at warning ( $\beta^{Post} + \beta^{\varpi}$ )=		-0.263				-0.409		
Testing ( $\beta^{Post} + \beta^{\varpi}$ ) = 0, p-value=		0.509				0.434		
Efficient price change at warning ( $\lambda^{\varpi}$ )=		0.000				0.000		
Change in elasticity parameter at landfall ( $\beta^{Post} + \beta^h$ )=		-0.126				-0.272		
Testing ( $\beta^{Post} + \beta^h$ ) = 0, p-value=		0.751				0.603		
Efficient price change at landfall ( $\lambda^h$ )=		0.000				0.000		
Change in elasticity parameter after strike ( $\beta^{Post} + \beta^s$ )=		-0.115				-0.263		
Testing ( $\beta^{Post} + \beta^s$ ) = 0, p-value=		0.825				0.670		
Efficient change after strike ( $\lambda^s$ )=		0.000				0.000		

Notes: This table reports the results from IV demand estimations using a flexible, semi-log functional form (Eq.2.5). The estimation sample covers years 2014 and 2017 and includes all counties in the state of Texas. Dependent variable is log store-UPC-level total quantity purchased. Using Eq.2.5, the elasticity coefficient in pre-catastrophe years is  $\eta = (\vartheta^{Pre}) * \bar{P}$  evaluated at the estimation sample mean. Efficient price changes are set to zero if the change in elasticity parameter is not significant at the 5% level. In columns 1, 3-5, and 7-8, efficient price changes are not computed because  $\vartheta^{Pre}$  is not significant at the 5% level. The product is toilet paper (TP) in columns 1 and 5, batteries (BT) in columns 2 and 6, milk (MK) in columns 3 and 7, and bottled water (BW) in columns 4 and 8. In columns 5 through 8, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Applications to individual assistance programs and applications reporting needs are normalized by the total population (number of households) in the zipcode3 for the corresponding year and multiplied by 100. Timing of actual Harvey landfall in Texas is August 25-26th, 2017. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table A.14: Comparison of price changes due to placebo Harvey versus actual Harvey - All counties in the state of Texas, Base year 2014

VARIABLES	(1) BT	(2) MK	(3) BW	(4) TP	(5) BT	(6) MK	(7) BW
Year 2015 ( $\mathbb{1}$ )	0.004 (0.003)	-0.004 (0.003)	-0.002 (0.001)	0.006*** (0.001)	-0.000 (0.002)	-0.003 (0.002)	-0.002* (0.001)
During placebo Harvey landfall ( $\mathbb{1}$ )	-0.005 (0.022)	0.116 (0.110)	0.002 (0.007)	0.012 (0.008)	-0.004 (0.018)	0.031 (0.036)	0.005 (0.007)
After placebo strike (1-3 weeks) ( $\varkappa$ )	-0.083 (0.082)	-0.044 (0.035)	-0.006 (0.009)	-0.005 (0.018)	-0.045 (0.053)	-0.045 (0.035)	-0.002 (0.009)
Constant	1.000*** (0.002)	0.986*** (0.002)	0.993*** (0.001)	0.987*** (0.001)	0.998*** (0.001)	0.987*** (0.002)	0.990*** (0.001)
Observations	19,277	9,189	53,375	43,678	19,277	9,189	53,375
R-squared	0.011	0.026	0.006	0.010	0.015	0.028	0.010
Counties	All	All	All	All	All	All	All
Price index winsorized at the 2nd and 98th percentiles	No	No	No	Yes	Yes	Yes	Yes
Store fixed effects and sampling weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observed price change at placebo landfall ( $\mathbb{1} + \mathbb{1}$ )=	0.000	0.112	0.000	0.018	-0.005	0.028	0.003
Observed price change at actual landfall ( $\mathbb{1} + \ell$ )=	0.035	-0.017	0.002	0.037	0.029	-0.019	0.007
Testing equality ( $\mathbb{1} + \mathbb{1}$ ) = ( $\mathbb{1} + \ell$ ), p-value=	0.107	0.238	0.770	0.013	0.057	0.193	0.548
Observed price change after placebo strike ( $\varkappa + \mathbb{1}$ )=	-0.079	-0.048	-0.008	0.001	-0.045	-0.048	-0.004
Observed price change after actual strike ( $\varkappa + \ell$ )=	-0.005	0.029	-0.001	0.010	-0.003	0.029	0.000
Testing equality ( $\varkappa + \mathbb{1}$ ) = ( $\varkappa + \ell$ ), p-value=	0.369	0.028	0.452	0.628	0.425	0.028	0.651

Notes: This table reports the results from price estimations using Eq.2.12. The estimation sample covers pre-hurricane years (2014-2015) and includes all counties in the state of Texas. Dependent variable is store-UPC-level price index. In columns 4 through 7, price index is winsorized at the 2nd and 98th percentiles: values smaller than the 2nd percentile are replaced by the 2nd percentile, and values higher than the 98th percentile are replaced by the 98th percentile. Placebo effects are the same as those estimated in Table 2.11, but post-estimation tests of equality of placebo and actual Harvey effects may differ because actual Harvey effects are those estimated using year 2014 only as the base year. The product is batteries (BT) in columns 1 and 5, milk (MK) in columns 2 and 6, bottled water (BW) in columns 3 and 7, and toilet paper (TP) in column 4. Toilet paper test results using non-winsorized price index are not presented because  $\eta^{pre}$  is not significant at the 5% level, and efficient price changes are not computed using OLS (Table A.12, column 1) or IV (Table A.13, column 1). Timing of placebo Harvey landfall in Texas is August 25-26th, 2015. Store fixed effects and sampling weights are used in all regressions. Robust standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

### A.3 Figures

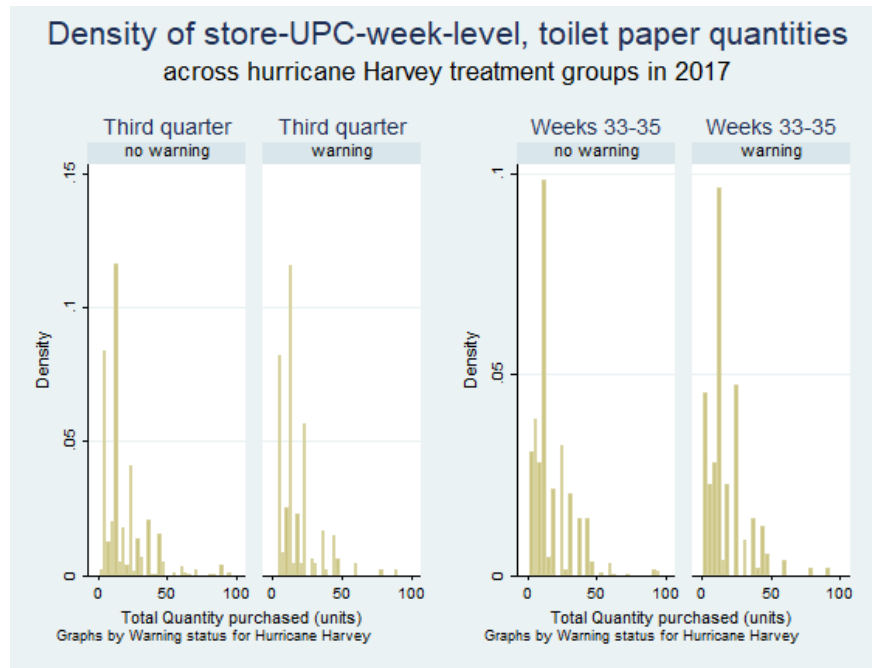


Figure A.1: Density of toilet paper quantities by warning status in the Harvey season

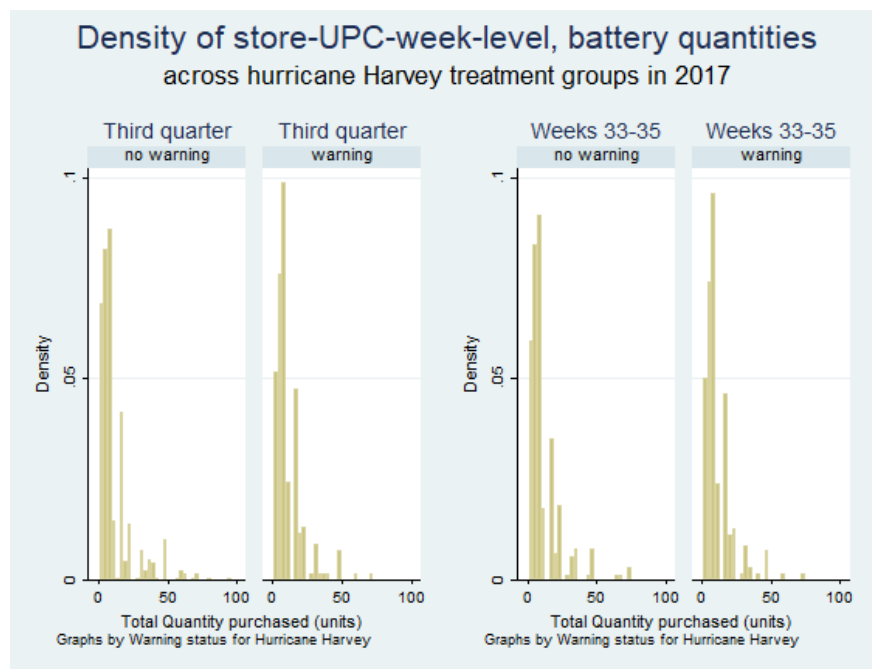


Figure A.2: Density of battery quantities by warning status in the Harvey season

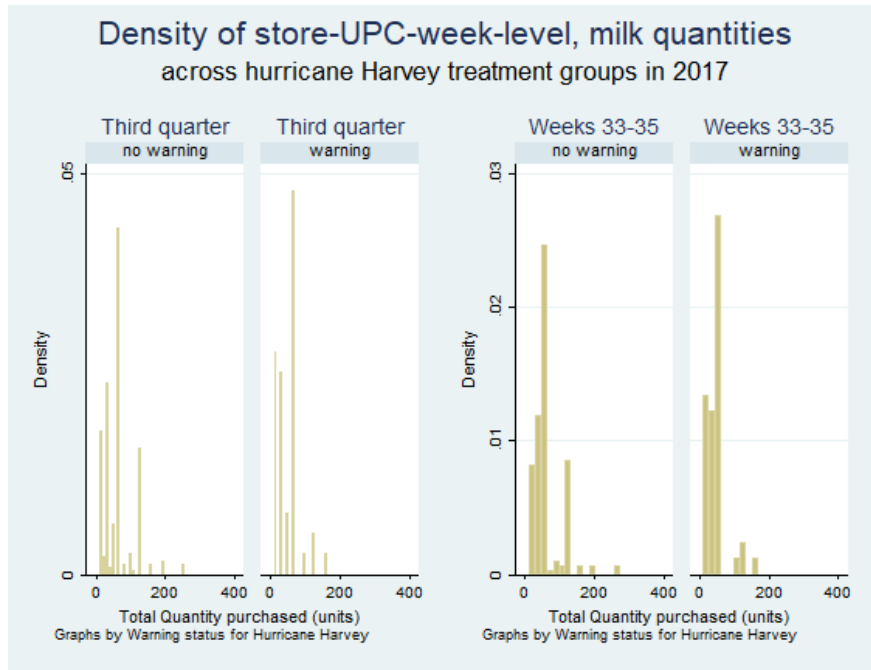


Figure A.3: Density of milk quantities by warning status in the Harvey season

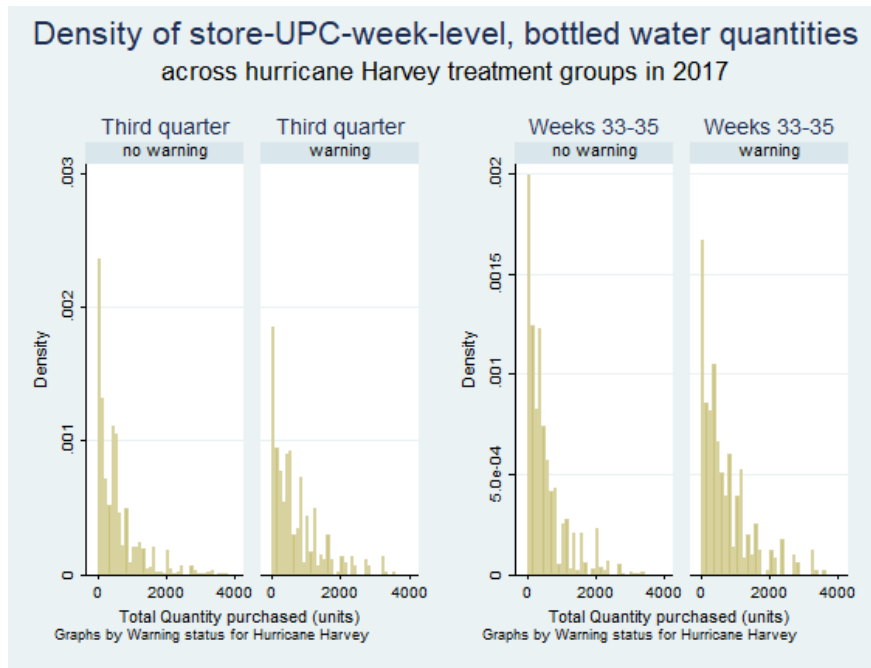


Figure A.4: Density of bottled water quantities by warning status in the Harvey season

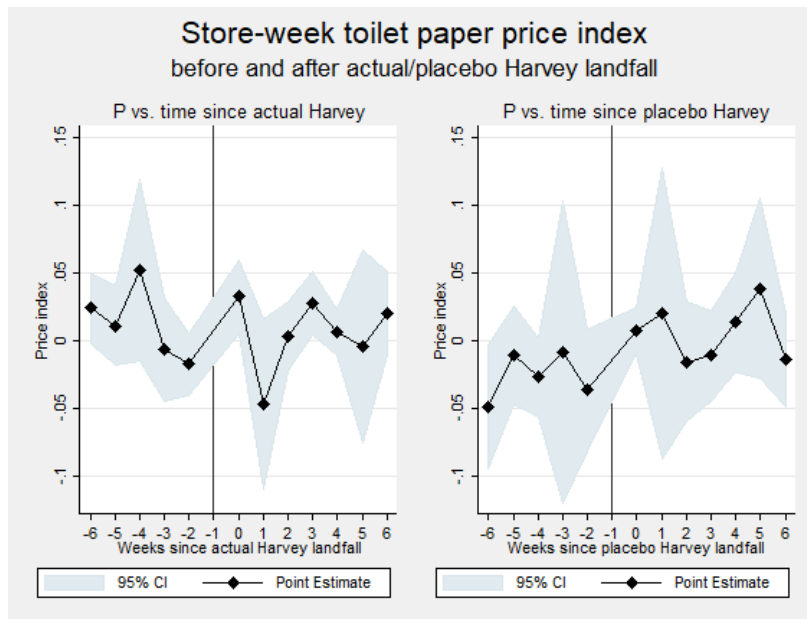


Figure A.5: Toilet paper price index before and after actual/placebo Harvey landfall

Notes: Actual Harvey landfall timing in Texas: August 25-26th, 2017. Placebo Harvey landfall timing in Texas (2 years earlier): August 25-26th, 2015. Point estimates are the coefficients of the regression of observed price index on the set of lags and leads around the event time, after absorbing the time fixed effects. It is standard practice to interpret the coefficients relative to the week before the event.

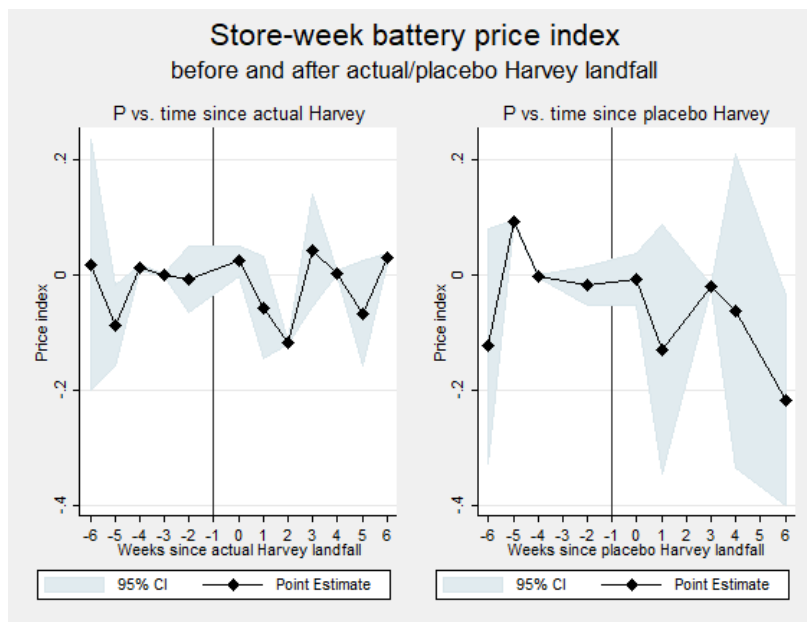


Figure A.6: Battery price index before and after actual/placebo Harvey landfall

Notes: Actual Harvey landfall timing in Texas: August 25-26th, 2017. Placebo Harvey landfall timing in Texas (2 years earlier): August 25-26th, 2015. Point estimates are the coefficients of the regression of observed price index on the set of lags and leads around the event time, after absorbing the time fixed effects. It is standard practice to interpret the coefficients relative to the week before the event.

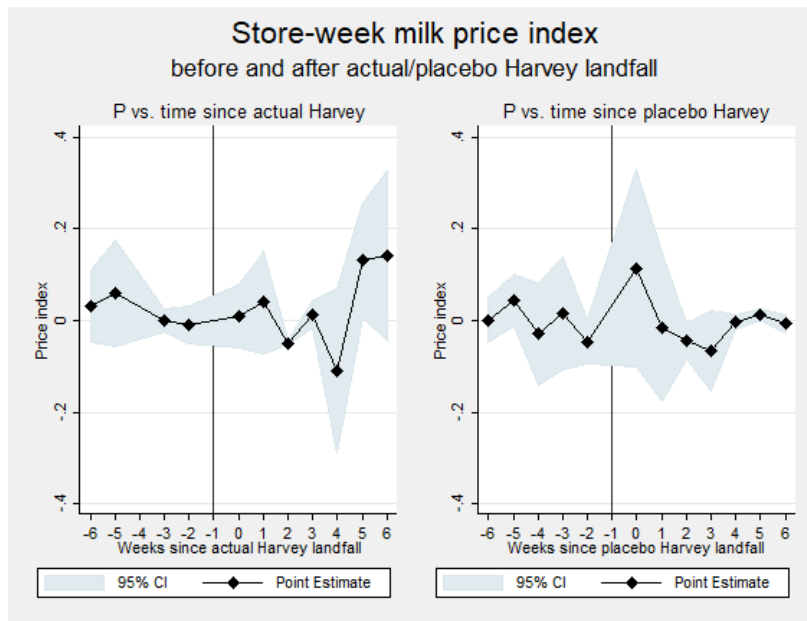


Figure A.7: Milk price index before and after actual/placebo Harvey landfall

Notes: Actual Harvey landfall timing in Texas: August 25-26th, 2017. Placebo Harvey landfall timing in Texas (2 years earlier): August 25-26th, 2015. Point estimates are the coefficients of the regression of observed price index on the set of lags and leads around the event time, after absorbing the time fixed effects. It is standard practice to interpret the coefficients relative to the week before the event.

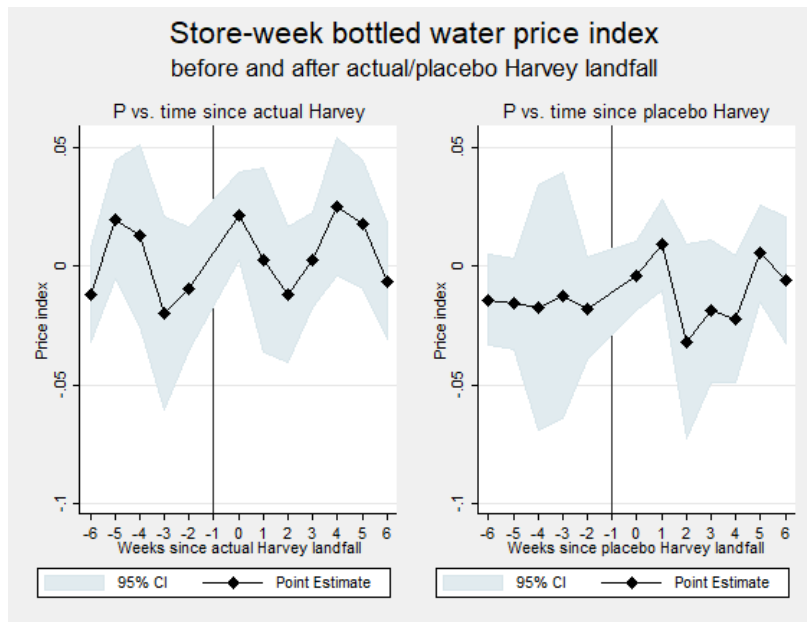


Figure A.8: Bottled water price index before and after actual/placebo Harvey landfall

Notes: Actual Harvey landfall timing in Texas: August 25-26th, 2017. Placebo Harvey landfall timing in Texas (2 years earlier): August 25-26th, 2015. Point estimates are the coefficients of the regression of observed price index on the set of lags and leads around the event time, after absorbing the time fixed effects. It is standard practice to interpret the coefficients relative to the week before the event.

## APPENDIX B

### AS IT RAINS, SO SHOULD WE CARE? DESCRIBING THE STATIC IMPACT OF STORMS ON THE ECONOMY USING A BALANCED PANEL

#### **B.1 Tables**

Table B.1: Static effect of natural disasters on per capita GDP growth (robust to confounding disasters and pre-trends)

VARIABLES	(1) TWFE	(2) S-A	(3) TWFE	(4) S-A	(5) TWFE	(6) S-A	(7) TWFE	(8) S-A	(9) TWFE	(10) S-A
lead10	0.9247 (0.8982)	2.6195* (1.3825)	0.3106 (0.8976)	-1.1925 (1.2322)	-0.0495 (0.9696)	0.9469 (1.4828)	2.2476** (0.9364)	0.8813 (1.1430)	0.5679 (0.9116)	-0.3451 (1.1609)
lead9	0.2956 (1.3360)	1.3295 (1.7921)	-0.7201 (1.2415)	-0.8534 (1.5705)	0.1865 (1.4130)	1.2907 (1.7550)	2.8092** (1.2756)	2.2801 (1.4410)	1.3231 (1.2417)	1.1453 (1.5469)
lead8	-1.0614 (1.3131)	0.2376 (1.8387)	2.1949* (1.2302)	1.4090 (1.6125)	0.7887 (1.4121)	1.4730 (1.7998)	1.6500 (1.2743)	0.8986 (1.4413)	0.2659 (1.2426)	0.4829 (1.5969)
lead7	0.0703 (1.2933)	0.5431 (1.6643)	0.5820 (1.2351)	0.1950 (1.4875)	-1.0988 (1.4110)	-0.9539 (1.7845)	2.7048** (1.2720)	1.4223 (1.7059)	1.3772 (1.2452)	1.5665 (1.6790)
lead6	0.8883 (1.2832)	1.5910 (1.9415)	0.6829 (1.2223)	0.4751 (1.5758)	-0.2411 (1.4014)	0.6861 (1.7105)	1.2156 (1.2689)	0.4860 (1.6469)	0.1312 (1.2431)	0.4346 (1.5592)
lead5	1.1710 (1.2765)	1.8875 (1.7479)	0.4827 (1.2258)	0.4214 (1.5490)	-0.2833 (1.3753)	0.6208 (1.7371)	3.0581** (1.2718)	1.8567 (1.5416)	0.9234 (1.2432)	1.9389 (1.6836)
lead4	-0.5357 (1.2648)	0.1234 (1.5237)	1.3149 (1.2224)	0.9560 (1.4762)	-0.4411 (1.3529)	0.2721 (1.8009)	2.2140* (1.2719)	1.2053 (1.3998)	0.9696 (1.2380)	1.5449 (1.7007)
lead3	0.4730 (1.2450)	0.9441 (1.5596)	-0.6169 (1.2136)	-0.6780 (1.4701)	-0.8926 (1.3396)	0.3139 (1.8519)	2.3965* (1.2724)	1.9931 (1.4717)	0.7295 (1.2372)	1.9639 (1.7526)
lead2	-1.0113 (1.2066)	-1.0891 (1.4336)	1.3870 (1.2180)	1.4064 (1.5389)	0.0510 (1.3190)	0.6161 (1.6187)	0.1160 (1.2669)	-0.8460 (1.4323)	0.7436 (1.2293)	0.6598 (1.5738)
D_flood	0.8387 (0.8782)	0.7416 (0.9622)								
D_storm			0.2570 (0.8761)	0.0259 (0.9826)						
D_earthquake					0.2701 (0.9454)	0.6443 (1.1386)				
D_drought							0.9696 (0.9227)	0.7978 (1.1010)		
D_epidemic									0.3854 (0.8959)	1.1378 (1.2011)
Flood occurred			0.8510*** (0.2700)	0.0080 (0.3171)	0.8013*** (0.2628)	-0.1528 (0.2102)	0.8942*** (0.2554)	0.0496 (0.1480)	0.8478*** (0.2552)	-0.2353 (0.2752)
Storm occurred	-0.1609 (0.3014)	-0.3571 (0.3412)			-0.0315 (0.2924)	-0.5250 (0.4639)	-0.0972 (0.2848)	-0.3285 (0.3616)	-0.1003 (0.2866)	-0.1594 (0.3999)
Earthquake occurred	0.7204* (0.3978)	0.2381 (0.3777)	0.5122 (0.3963)	0.4871 (0.5724)			0.5351 (0.3531)	-0.1352 (0.3381)	0.4532 (0.3545)	0.0645 (0.3533)
Drought occurred	0.0652 (0.4292)	0.1122 (0.3540)	0.0702 (0.4251)	0.0958 (0.4623)	0.0032 (0.4095)	0.2447 (0.2996)			0.1287 (0.4026)	-0.1398 (0.3197)
Epidemic occurred	0.4731 (0.3909)	0.6597 (0.4425)	0.4570 (0.3945)	0.3080 (0.4400)	0.4056 (0.3883)	0.3412 (0.4128)	0.5379 (0.3759)	0.2415 (0.2355)		
Other disaster occurred	0.1911 (0.3066)	-0.0025 (0.2279)	0.1473 (0.3032)	-0.2373 (0.2369)	0.0296 (0.2877)	-0.1893 (0.2560)	0.1229 (0.2814)	0.2665 (0.1806)	0.1005 (0.2824)	-0.0689 (0.1739)
Observations	3,519	3,519	3,588	3,588	3,588	3,588	3,795	3,795	3,795	3,795
R-squared	0.1283		0.1257		0.1229		0.1285		0.1234	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	0.344	0.632	0.370	0.933	0.980	0.984	0.121	0.471	0.974	0.811

Notes: This table reports the static effect  $\gamma$  of natural disasters on per capita GDP growth using the Two-Way Fixed Effects specification with confounders and pre-trends in Eq.3.3 (TWFE in odd-numbered columns) and the heterogeneity-robust Sun and Abraham, 2021's estimator (S-A in even-numbered columns). The estimation sample covers 69 years for floods (51 countries), storms and earthquakes (52 countries), and droughts and epidemics (55 countries). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table B.2: Static effect of natural disasters on saving rate (robust to confounding disasters and pre-trends)

VARIABLES	(1) TWFE	(2) S-A	(3) TWFE	(4) S-A	(5) TWFE	(6) S-A	(7) TWFE	(8) S-A	(9) TWFE	(10) S-A
lead10	0.0287*** (0.0094)	0.0171 (0.0123)	0.0226** (0.0094)	-0.0048 (0.0117)	-0.0003 (0.0103)	-0.0099 (0.0151)	-0.0164 (0.0100)	-0.0161 (0.0111)	0.0094 (0.0097)	0.0027 (0.0115)
lead9	0.0173 (0.0140)	0.0154 (0.0185)	-0.0016 (0.0130)	-0.0052 (0.0139)	0.0149 (0.0150)	0.0086 (0.0191)	-0.0150 (0.0136)	-0.0127 (0.0154)	-0.0144 (0.0132)	-0.0212 (0.0154)
lead8	0.0243* (0.0138)	0.0201 (0.0171)	-0.0013 (0.0129)	-0.0096 (0.0140)	0.0075 (0.0150)	0.0001 (0.0186)	-0.0068 (0.0135)	-0.0107 (0.0151)	-0.0096 (0.0132)	-0.0169 (0.0151)
lead7	0.0228* (0.0136)	0.0160 (0.0165)	-0.0004 (0.0130)	-0.0056 (0.0136)	0.0017 (0.0150)	-0.0076 (0.0187)	-0.0068 (0.0135)	-0.0057 (0.0145)	-0.0115 (0.0132)	-0.0183 (0.0143)
lead6	0.0169 (0.0135)	0.0104 (0.0146)	-0.0027 (0.0128)	-0.0062 (0.0131)	-0.0032 (0.0149)	-0.0109 (0.0180)	-0.0063 (0.0135)	-0.0024 (0.0142)	-0.0116 (0.0132)	-0.0205 (0.0141)
lead5	0.0118 (0.0134)	0.0091 (0.0139)	-0.0087 (0.0129)	-0.0105 (0.0135)	-0.0089 (0.0146)	-0.0114 (0.0167)	-0.0003 (0.0135)	0.0028 (0.0146)	-0.0036 (0.0132)	-0.0096 (0.0142)
lead4	0.0181 (0.0133)	0.0134 (0.0146)	-0.0067 (0.0128)	-0.0057 (0.0159)	-0.0170 (0.0144)	-0.0144 (0.0162)	-0.0073 (0.0135)	-0.0061 (0.0142)	-0.0024 (0.0131)	-0.0036 (0.0145)
lead3	0.0127 (0.0131)	0.0124 (0.0135)	-0.0044 (0.0128)	-0.0046 (0.0134)	-0.0124 (0.0142)	-0.0141 (0.0162)	-0.0064 (0.0135)	-0.0052 (0.0138)	-0.0034 (0.0131)	-0.0093 (0.0146)
lead2	0.0079 (0.0127)	0.0100 (0.0123)	0.0008 (0.0128)	0.0023 (0.0122)	-0.0024 (0.0140)	-0.0025 (0.0154)	-0.0021 (0.0135)	0.0059 (0.0133)	0.0001 (0.0130)	-0.0086 (0.0137)
D_flood	0.0066 (0.0092)	0.0230*** (0.0086)								
D_storm			-0.0214** (0.0092)	-0.0140 (0.0092)						
D_earthquake					0.0107 (0.0100)	0.0014 (0.0116)				
D_drought							-0.0030 (0.0098)	0.0030 (0.0106)		
D_epidemic									0.0068 (0.0095)	-0.0099 (0.0119)
Flood occurred			-0.0036 (0.0028)	-0.0037 (0.0030)	-0.0026 (0.0028)	0.0006 (0.0023)	-0.0035 (0.0027)	-0.0028 (0.0018)	-0.0031 (0.0027)	-0.0013 (0.0030)
Storm occurred	0.0057* (0.0032)	-0.0020 (0.0031)			0.0050 (0.0031)	-0.0005 (0.0046)	0.0058* (0.0030)	-0.0025 (0.0035)	0.0059* (0.0030)	0.0027 (0.0040)
Earthquake occurred	-0.0019 (0.0042)	-0.0002 (0.0032)	-0.0046 (0.0042)	-0.0013 (0.0053)			-0.0033 (0.0038)	-0.0062* (0.0032)	-0.0034 (0.0038)	0.0013 (0.0034)
Drought occurred	0.0145*** (0.0045)	0.0021 (0.0031)	0.0098** (0.0045)	-0.0043 (0.0044)	0.0107** (0.0043)	0.0035 (0.0033)			0.0114*** (0.0043)	0.0004 (0.0033)
Epidemic occurred	-0.0166*** (0.0041)	0.0030 (0.0042)	-0.0185*** (0.0041)	-0.0134** (0.0052)	-0.0182*** (0.0041)	-0.0035 (0.0042)	-0.0173*** (0.0040)	-0.0139*** (0.0044)		
Other disaster occurred	0.0032 (0.0032)	0.0011 (0.0021)	0.0027 (0.0032)	0.0016 (0.0022)	0.0039 (0.0031)	0.0044* (0.0027)	0.0035 (0.0030)	0.0022 (0.0017)	0.0038 (0.0030)	-0.0009 (0.0018)
Observations	3,519	3,519	3,588	3,588	3,588	3,588	3,795	3,795	3,795	3,795
R-squared	0.5717		0.5671		0.5751		0.5640		0.5636	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	0.0703	0.511	3.89e-05	0.990	0.670	0.955	0.494	0.909	0.0398	0.507

Notes: This table reports the static effect  $\gamma$  of natural disasters on saving rate using the Two-Way Fixed Effects specification with confounders and pre-trends in Eq.3.3 (TWFE in odd-numbered columns) and the heterogeneity-robust Sun and Abraham, 2021's estimator (S-A in even-numbered columns). The estimation sample covers 69 years for floods (51 countries), storms and earthquakes (52 countries), and droughts and epidemics (55 countries). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table B.3: Static effect of natural disasters on inflation rate (robust to confounding disasters and pre-trends)

VARIABLES	(1) TWFE	(2) S-A	(3) TWFE	(4) S-A	(5) TWFE	(6) S-A	(7) TWFE	(8) S-A	(9) TWFE	(10) S-A
lead10	0.6696 (1.7197)	-0.7843 (2.6327)	0.4176 (1.6889)	0.5944 (2.3700)	1.0800 (1.8697)	0.9722 (2.8497)	-0.3811 (1.7869)	0.2172 (2.2234)	-0.5780 (1.7361)	-0.8055 (2.2241)
lead9	1.1255 (2.5582)	0.7674 (3.2849)	-1.1580 (2.3359)	-1.5282 (2.9192)	-0.4040 (2.7247)	-0.9172 (3.6926)	-0.4680 (2.4341)	-1.5379 (3.2381)	-1.3324 (2.3646)	-0.8465 (3.0229)
lead8	-2.7578 (2.5142)	-2.7840 (3.1454)	1.1478 (2.3147)	1.4262 (3.1388)	0.5730 (2.7230)	-0.1261 (3.8087)	0.4146 (2.4317)	-0.2655 (3.2673)	0.5161 (2.3664)	1.3135 (2.7078)
lead7	0.4383 (2.4763)	1.7212 (3.6158)	-0.3852 (2.3238)	-0.5153 (2.7076)	1.2693 (2.7209)	0.6329 (4.0239)	0.7064 (2.4273)	-0.4475 (3.3437)	-0.6654 (2.3713)	-0.4656 (2.9888)
lead6	-0.9271 (2.4570)	0.0322 (3.5324)	2.4315 (2.2998)	1.1107 (2.8643)	3.2279 (2.7023)	2.3615 (3.6133)	0.6523 (2.4214)	0.1390 (3.5748)	0.3622 (2.3674)	-0.1468 (2.9936)
lead5	3.3448 (2.4442)	3.3220 (3.5492)	0.7625 (2.3063)	-0.0056 (2.8854)	-0.5496 (2.6520)	-0.5438 (3.8148)	1.3240 (2.4269)	0.3244 (3.3757)	-3.1092 (2.3675)	-3.0013 (2.9886)
lead4	-0.1064 (2.4217)	-0.1156 (3.0830)	2.0456 (2.2999)	2.1618 (2.7718)	-1.1344 (2.6088)	-1.8874 (3.3407)	2.1026 (2.4270)	0.7465 (3.3750)	-2.9252 (2.3577)	-2.9361 (3.1901)
lead3	1.6989 (2.3839)	1.6639 (3.1174)	2.0003 (2.2835)	1.3071 (2.8415)	-2.1103 (2.5833)	-2.1751 (3.5114)	3.4909 (2.4281)	1.8209 (3.4272)	-2.7782 (2.3562)	-2.7757 (3.2202)
lead2	0.8670 (2.3103)	1.0074 (2.9259)	-0.0579 (2.2918)	-0.7866 (2.7764)	-0.4144 (2.5434)	-0.0381 (3.3300)	1.9057 (2.4175)	0.3908 (3.5632)	-0.4413 (2.3411)	-1.3767 (2.9503)
D_flood	-0.5356 (1.6814)	0.7055 (1.8430)								
D_storm			0.6661 (1.6484)	-0.7331 (1.8909)						
D_earthquake					1.1906 (1.8232)	1.6366 (2.1875)				
D_drought							1.9056 (1.7608)	-0.8534 (2.0920)		
D_epidemic									-0.3162 (1.7061)	-0.0317 (2.2928)
Flood occurred			-0.3009 (0.5079)	0.1370 (0.5723)	-0.3682 (0.5067)	-0.1074 (0.4043)	-0.3158 (0.4873)	-0.3536 (0.2753)	-0.2182 (0.4860)	0.0414 (0.5248)
Storm occurred	-0.1838 (0.5771)	0.3701 (0.6443)			-0.1673 (0.5639)	-0.5229 (0.8807)	-0.2062 (0.5435)	-0.1084 (0.6798)	-0.2004 (0.5457)	-0.0522 (0.7610)
Earthquake occurred	0.2719 (0.7617)	1.6640** (0.7091)	0.0558 (0.7456)	0.2440 (1.1080)			0.0543 (0.6739)	0.4657 (0.6234)	0.1324 (0.6751)	0.1491 (0.6697)
Drought occurred	0.3858 (0.8217)	0.1217 (0.6831)	0.4448 (0.7998)	-1.4262 (0.9595)	0.1644 (0.7897)	0.3557 (0.6124)			0.2753 (0.7668)	0.1666 (0.6320)
Epidemic occurred	-0.7304 (0.7486)	-1.5051* (0.8854)	-0.7434 (0.7424)	-3.7942*** (0.8921)	-0.5869 (0.7488)	-1.5183* (0.8251)	-0.7052 (0.7172)	-0.8898* (0.5054)		
Other disaster occurred	-0.4696 (0.5871)	-0.3140 (0.4348)	-0.4259 (0.5705)	-0.5039 (0.4469)	-0.1219 (0.5547)	0.4658 (0.4983)	-0.3684 (0.5370)	-0.0358 (0.3284)	-0.3138 (0.5378)	0.3509 (0.3478)
Observations	3,519	3,519	3,588	3,588	3,588	3,588	3,795	3,795	3,795	3,795
R-squared	0.1965		0.2055		0.2018		0.2015		0.2001	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	0.654	0.983	0.866	0.996	0.702	0.998	0.529	1	0.683	0.952

Notes: This table reports the static effect  $\gamma$  of natural disasters on inflation rate using the Two-Way Fixed Effects specification with confounders and pre-trends in Eq.3.3 (TWFE in odd-numbered columns) and the heterogeneity-robust Sun and Abraham, 2021's estimator (S-A in even-numbered columns). The estimation sample covers 69 years for floods (51 countries), storms and earthquakes (52 countries), and droughts and epidemics (55 countries). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table B.4: Static effect of natural disasters on the depreciation rate of the capital stock (robust to confounding disasters and pre-trends)

VARIABLES	(1) TWFE	(2) S-A	(3) TWFE	(4) S-A	(5) TWFE	(6) S-A	(7) TWFE	(8) S-A	(9) TWFE	(10) S-A
lead10	0.0046*** (0.0008)	0.0024** (0.0012)	-0.0007 (0.0008)	-0.0006 (0.0010)	0.0017* (0.0009)	-0.0011 (0.0012)	-0.0006 (0.0009)	-0.0016 (0.0010)	-0.0021** (0.0008)	-0.0023** (0.0010)
lead9	0.0019 (0.0012)	0.0010 (0.0014)	-0.0012 (0.0012)	-0.0016 (0.0012)	0.0014 (0.0013)	-0.0002 (0.0015)	-0.0001 (0.0012)	-0.0002 (0.0013)	-0.0013 (0.0011)	-0.0009 (0.0013)
lead8	0.0016 (0.0012)	0.0009 (0.0013)	-0.0012 (0.0011)	-0.0015 (0.0012)	0.0013 (0.0013)	0.0001 (0.0015)	0.0000 (0.0012)	-0.0005 (0.0013)	-0.0012 (0.0012)	-0.0008 (0.0012)
lead7	0.0015 (0.0012)	0.0008 (0.0013)	-0.0011 (0.0012)	-0.0014 (0.0012)	0.0012 (0.0013)	0.0001 (0.0015)	0.0000 (0.0012)	-0.0005 (0.0013)	-0.0010 (0.0012)	-0.0008 (0.0012)
lead6	0.0013 (0.0012)	0.0004 (0.0012)	-0.0011 (0.0011)	-0.0013 (0.0012)	0.0012 (0.0013)	-0.0004 (0.0014)	-0.0001 (0.0012)	-0.0004 (0.0013)	-0.0008 (0.0012)	-0.0009 (0.0012)
lead5	0.0012 (0.0012)	0.0005 (0.0012)	-0.0007 (0.0011)	-0.0012 (0.0011)	0.0009 (0.0013)	-0.0005 (0.0013)	-0.0002 (0.0012)	-0.0005 (0.0013)	-0.0007 (0.0012)	-0.0008 (0.0012)
lead4	0.0011 (0.0012)	0.0002 (0.0012)	-0.0004 (0.0011)	-0.0007 (0.0011)	0.0000 (0.0013)	-0.0003 (0.0013)	-0.0002 (0.0012)	-0.0004 (0.0013)	-0.0005 (0.0011)	-0.0007 (0.0012)
lead3	0.0002 (0.0011)	0.0002 (0.0012)	-0.0001 (0.0011)	-0.0004 (0.0011)	-0.0001 (0.0013)	-0.0002 (0.0013)	-0.0002 (0.0012)	0.0000 (0.0013)	-0.0004 (0.0011)	-0.0005 (0.0012)
lead2	0.0002 (0.0011)	0.0002 (0.0011)	0.0001 (0.0011)	-0.0002 (0.0011)	-0.0002 (0.0012)	-0.0004 (0.0012)	-0.0001 (0.0012)	-0.0003 (0.0012)	-0.0001 (0.0011)	-0.0003 (0.0011)
D_flood	0.0005 (0.0008)	0.0018** (0.0008)								
D_storm			0.0006 (0.0008)	-0.0000 (0.0008)						
D_earthquake					0.0022** (0.0009)	0.0030*** (0.0009)				
D_drought							0.0006 (0.0009)	0.0005 (0.0010)		
D_epidemic									0.0003 (0.0008)	0.0012 (0.0010)
Flood occurred			-0.0000 (0.0003)	0.0005* (0.0003)	0.0000 (0.0002)	0.0008*** (0.0002)	-0.0000 (0.0002)	-0.0002 (0.0002)	-0.0001 (0.0002)	0.0001 (0.0002)
Storm occurred	0.0004 (0.0003)	0.0002 (0.0003)			0.0004 (0.0003)	0.0005 (0.0004)	0.0004 (0.0003)	0.0000 (0.0003)	0.0003 (0.0003)	0.0004 (0.0003)
Earthquake occurred	0.0004 (0.0004)	0.0003 (0.0003)	0.0005 (0.0004)	0.0004 (0.0005)			0.0004 (0.0003)	-0.0002 (0.0003)	0.0004 (0.0003)	0.0001 (0.0003)
Drought occurred	0.0003 (0.0004)	-0.0002 (0.0003)	0.0005 (0.0004)	0.0003 (0.0004)	0.0005 (0.0004)	-0.0000 (0.0003)			0.0003 (0.0004)	0.0001 (0.0003)
Epidemic occurred	-0.0008** (0.0004)	-0.0012*** (0.0004)	-0.0010*** (0.0004)	-0.0001 (0.0004)	-0.0012*** (0.0004)	-0.0005 (0.0004)	-0.0012*** (0.0004)	-0.0002 (0.0003)		
Other disaster occurred	0.0002 (0.0003)	-0.0002 (0.0002)	0.0006** (0.0003)	0.0004* (0.0002)	0.0004 (0.0003)	0.0002 (0.0002)	0.0004* (0.0003)	-0.0002 (0.0002)	0.0003 (0.0003)	-0.0001 (0.0002)
Observations	3,519	3,519	3,588	3,588	3,588	3,588	3,795	3,795	3,795	3,795
R-squared	0.8064		0.7982		0.7930		0.7982		0.7997	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	0	0.749	0.951	0.545	0.253	0.999	0.982	0.964	0.0297	0.512

Notes: This table reports the static effect  $\gamma$  of natural disasters on the depreciation rate of the capital stock using the Two-Way Fixed Effects specification with confounders and pre-trends in Eq.3.3 (TWFE in odd-numbered columns) and the heterogeneity-robust Sun and Abraham, 2021's estimator (S-A in even-numbered columns). The estimation sample covers 69 years for floods (51 countries), storms and earthquakes (52 countries), and droughts and epidemics (55 countries). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table B.5: Static effect of natural disasters on employment rate (robust to confounding disasters and pre-trends)

VARIABLES	(1) TWFE	(2) S-A	(3) TWFE	(4) S-A	(5) TWFE	(6) S-A	(7) TWFE	(8) S-A	(9) TWFE	(10) S-A
lead10	0.3731 (0.5867)	1.3591* (0.7840)	0.4686 (0.5796)	-0.4346 (0.7324)	1.6154*** (0.6180)	1.1017 (0.8347)	-0.9966 (0.6146)	-1.4606** (0.7061)	0.8925 (0.5981)	1.0273 (0.6392)
lead9	-0.2837 (0.8864)	0.4568 (1.0348)	-0.0111 (0.8062)	-0.3156 (0.8518)	0.4770 (0.9037)	-0.1115 (0.9900)	0.1579 (0.8377)	-0.0078 (0.9116)	0.3521 (0.8160)	0.2323 (0.8819)
lead8	-0.0133 (0.8696)	0.6047 (0.9714)	-0.1491 (0.8000)	-0.2322 (0.8455)	0.3825 (0.9034)	-0.1203 (0.9907)	0.3346 (0.8364)	0.4265 (0.8694)	0.0976 (0.8166)	0.0021 (0.8786)
lead7	0.0687 (0.8538)	0.5836 (0.9055)	-0.2849 (0.8014)	-0.3414 (0.8395)	0.3671 (0.9032)	-0.1615 (0.9962)	0.5043 (0.8355)	0.4720 (0.8784)	0.0457 (0.8185)	0.4206 (0.8413)
lead6	-0.3216 (0.8458)	0.3037 (0.8441)	-0.2641 (0.7937)	-0.3592 (0.8022)	0.2151 (0.8958)	-0.3640 (0.9439)	0.5135 (0.8347)	0.5193 (0.8624)	0.0107 (0.8181)	-0.1417 (0.8068)
lead5	0.0423 (0.8400)	0.0967 (0.8432)	-0.3829 (0.7960)	-0.2691 (0.8254)	0.3828 (0.8783)	-0.4068 (0.9089)	0.5367 (0.8353)	0.1967 (0.8704)	-0.1890 (0.8177)	-0.1336 (0.8173)
lead4	-0.0877 (0.8317)	-0.1340 (0.8399)	-0.2721 (0.7930)	-0.2356 (0.7939)	0.3919 (0.8631)	-0.2726 (0.8637)	0.4053 (0.8352)	0.1149 (0.8346)	-0.0388 (0.8142)	-0.1294 (0.8319)
lead3	0.0509 (0.8180)	-0.1964 (0.7902)	-0.1114 (0.7875)	0.0967 (0.7903)	0.3362 (0.8537)	-0.4116 (0.8558)	0.4604 (0.8353)	0.0228 (0.8411)	-0.0848 (0.8146)	0.0655 (0.8349)
lead2	-0.0400 (0.7908)	-0.2843 (0.7196)	-0.2035 (0.7907)	0.0840 (0.7918)	0.0285 (0.8405)	-0.1143 (0.8281)	0.2745 (0.8323)	0.1520 (0.8263)	-0.1730 (0.8077)	-0.0842 (0.7831)
D_flood	-0.3031 (0.5745)	-0.5754 (0.5417)								
D_storm			-0.4080 (0.5670)	0.2002 (0.5988)						
D_earthquake					-0.3867 (0.6017)	-0.0769 (0.6339)				
D_drought							-0.2500 (0.6057)	-0.2130 (0.6653)		
D_epidemic									-0.0018 (0.5886)	0.1906 (0.6779)
Flood occurred			-0.6462*** (0.1760)	-0.0069 (0.2005)	-0.4864*** (0.1670)	0.2373* (0.1339)	-0.5783*** (0.1671)	0.0613 (0.0978)	-0.5590*** (0.1676)	0.2662 (0.1765)
Storm occurred	-0.4949** (0.1983)	0.1356 (0.2160)			-0.4629** (0.1857)	-0.1122 (0.2569)	-0.4722** (0.1862)	0.2251 (0.2927)	-0.4454** (0.1884)	0.0384 (0.2292)
Earthquake occurred	-0.0736 (0.2620)	0.1968 (0.2138)	-0.1493 (0.2580)	0.2525 (0.3627)			-0.1441 (0.2297)	0.0727 (0.1620)	-0.1332 (0.2315)	-0.0089 (0.2108)
Drought occurred	0.0655 (0.2869)	0.1019 (0.2225)	-0.1130 (0.2806)	-0.2475 (0.3105)	0.0369 (0.2620)	-0.3898** (0.1840)			0.0734 (0.2680)	-0.3838** (0.1880)
Epidemic occurred	-1.6001*** (0.2586)	-0.5915** (0.2719)	-1.6018*** (0.2588)	-0.7939*** (0.2930)	-1.6494*** (0.2469)	-0.6879*** (0.2482)	-1.4258*** (0.2474)	-1.0831*** (0.2427)		
Other disaster occurred	-0.5346*** (0.1967)	-0.1081 (0.1299)	-0.5633*** (0.1925)	-0.1309 (0.1433)	-0.3445* (0.1796)	0.0416 (0.1102)	-0.4352** (0.1795)	0.0601 (0.1025)	-0.4386** (0.1808)	0.1132 (0.0844)
Observations	3,243	3,243	3,312	3,312	3,450	3,450	3,519	3,519	3,519	3,519
R-squared	0.8040		0.8086		0.8226		0.8166		0.8139	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	0.972	0.884	0.669	0.999	0.00736	0.983	0.000629	0.810	0.156	0.964

Notes: This table reports the static effect  $\gamma$  of natural disasters on employment rate using the Two-Way Fixed Effects specification with confounders and pre-trends in Eq.3.3 (TWFE in odd-numbered columns) and the heterogeneity-robust Sun and Abraham, 2021's estimator (S-A in even-numbered columns). The estimation sample covers 69 years for floods (47 countries), storms (48 countries), earthquakes (50 countries), and droughts and epidemics (51 countries). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table B.6: Static effect of natural disasters on per capita labor income growth (robust to confounding disasters and pre-trends)

VARIABLES	(1) TWFE	(2) S-A	(3) TWFE	(4) S-A	(5) TWFE	(6) S-A	(7) TWFE	(8) S-A	(9) TWFE	(10) S-A
lead10	1.2561 (1.0274)	2.2665 (1.6244)	0.4572 (1.0121)	-0.6647 (1.3446)	-0.3143 (1.1062)	1.3262 (1.7493)	2.7526** (1.0790)	2.5357* (1.3596)	-0.0748 (1.0452)	-0.5323 (1.3101)
lead9	-0.1689 (1.5339)	0.6989 (2.1931)	-0.1192 (1.4055)	-0.8083 (1.8266)	-0.9923 (1.6440)	0.7647 (2.0707)	3.0131** (1.4719)	2.9088 (1.9355)	0.7885 (1.4277)	0.6516 (1.8301)
lead8	-1.5086 (1.5218)	-0.3176 (2.2546)	2.3799* (1.3949)	1.6853 (1.8187)	0.2810 (1.6443)	1.8599 (2.1999)	3.0905** (1.4684)	2.8564* (1.6816)	0.1140 (1.4300)	0.7466 (1.7868)
lead7	-0.5310 (1.4898)	0.2337 (1.9498)	0.9238 (1.3964)	0.0940 (1.6672)	-1.1502 (1.6447)	0.2404 (2.1949)	3.1013** (1.4670)	2.5277 (2.2016)	0.8015 (1.4340)	0.8976 (1.8472)
lead6	-0.3171 (1.4773)	0.1114 (1.9347)	0.3499 (1.3819)	-0.1336 (1.6435)	-0.1646 (1.6271)	1.3660 (2.0125)	2.6752* (1.4662)	2.3995 (1.8932)	-0.4298 (1.4325)	-0.0261 (1.8457)
lead5	0.7512 (1.4663)	1.4713 (2.0319)	1.0922 (1.3879)	1.0379 (1.7708)	-0.0351 (1.5901)	1.4584 (1.9738)	3.3049** (1.4668)	3.0319 (1.9665)	0.7989 (1.4318)	2.1171 (2.0021)
lead4	-0.8599 (1.4500)	-0.2472 (1.8223)	1.6595 (1.3804)	1.5753 (1.6385)	-1.7811 (1.5562)	-0.3026 (1.9221)	2.8145* (1.4671)	1.5865 (1.8053)	0.4034 (1.4245)	1.3228 (1.9525)
lead3	-0.2201 (1.4360)	0.5580 (1.7899)	-0.5009 (1.3708)	-0.8326 (1.6726)	-1.4375 (1.5394)	0.4046 (2.0010)	3.6361** (1.4684)	3.4344* (1.8210)	0.0591 (1.4260)	1.4496 (1.8610)
lead2	-0.6906 (1.3835)	-0.9275 (1.6354)	1.5122 (1.3770)	1.5855 (1.7357)	-0.4260 (1.5105)	0.9595 (1.9022)	1.7019 (1.4642)	0.6869 (1.7949)	0.1226 (1.4157)	0.3341 (1.6689)
D_flood	0.4124 (1.0038)	0.5016 (1.1358)								
D_storm			0.0275 (0.9876)	-0.2754 (1.0821)						
D_earthquake					0.0633 (1.0790)	1.0501 (1.3109)				
D_drought							1.3414 (1.0645)	0.3595 (1.3074)		
D_epidemic									0.0169 (1.0297)	0.4068 (1.4046)
Flood occurred			0.9721*** (0.3091)	0.0122 (0.4006)	0.9242*** (0.2949)	-0.1775 (0.2293)	1.0548*** (0.2899)	-0.1152 (0.2201)	0.9472*** (0.2898)	-0.0872 (0.3439)
Storm occurred	-0.5021 (0.3406)	-0.5249 (0.5890)			-0.3253 (0.3235)	-0.2632 (0.5122)	-0.4205 (0.3189)	-0.4676 (0.4677)	-0.4195 (0.3214)	0.0637 (0.4268)
Earthquake occurred	0.1978 (0.4667)	0.8413* (0.4722)	0.0043 (0.4618)	0.8788 (0.6864)			0.1367 (0.4035)	0.1396 (0.3107)	0.0601 (0.4048)	0.4214 (0.4306)
Drought occurred	-0.3279 (0.5144)	-0.1362 (0.3336)	-0.1171 (0.5057)	0.0654 (0.5622)	-0.2282 (0.4735)	-0.1771 (0.3217)			-0.1791 (0.4743)	-0.1024 (0.3064)
Epidemic occurred	0.2450 (0.4850)	-0.7445 (0.5169)	0.2105 (0.4911)	-1.1324** (0.5728)	0.3298 (0.4702)	0.2609 (0.4259)	0.3969 (0.4612)	-0.1476 (0.3937)		
Other disaster occurred	0.5647 (0.3451)	-0.1821 (0.2700)	0.5172 (0.3396)	-0.4103 (0.2655)	0.3833 (0.3182)	-0.2244 (0.2169)	0.4841 (0.3124)	-0.0037 (0.2231)	0.4391 (0.3133)	-0.1319 (0.1736)
Observations	2,967	2,967	3,036	3,036	3,174	3,174	3,243	3,243	3,243	3,243
R-squared	0.1088		0.1066		0.1064		0.1103		0.1047	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	0.136	0.962	0.559	0.932	0.942	0.973	0.417	0.0312	0.988	0.967

Notes: This table reports the static effect  $\gamma$  of natural disasters on per capita labor income growth using the Two-Way Fixed Effects specification with confounders and pre-trends in Eq.3.3 (TWFE in odd-numbered columns) and the heterogeneity-robust Sun and Abraham, 2021's estimator (S-A in even-numbered columns). The estimation sample covers 69 years for floods (43 countries), storms (44 countries), earthquakes (46 countries), and droughts and epidemics (47 countries). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

Table B.7: Static effect of natural disasters on the internal rate of return (robust to confounding disasters and pre-trends)

VARIABLES	(1) TWFE	(2) S-A	(3) TWFE	(4) S-A	(5) TWFE	(6) S-A	(7) TWFE	(8) S-A	(9) TWFE	(10) S-A
lead10	-0.0360*** (0.0068)	-0.0225** (0.0101)	-0.0111* (0.0066)	-0.0089 (0.0080)	0.0043 (0.0071)	0.0376*** (0.0107)	-0.0148** (0.0073)	-0.0224** (0.0088)	-0.0088 (0.0070)	-0.0080 (0.0083)
lead9	-0.0226** (0.0103)	-0.0113 (0.0120)	0.0003 (0.0092)	0.0007 (0.0120)	0.0028 (0.0106)	0.0260 (0.0167)	-0.0093 (0.0100)	-0.0104 (0.0110)	-0.0043 (0.0096)	-0.0097 (0.0118)
lead8	-0.0212** (0.0100)	-0.0098 (0.0118)	0.0041 (0.0091)	0.0055 (0.0108)	0.0055 (0.0106)	0.0268 (0.0171)	-0.0101 (0.0100)	-0.0089 (0.0108)	-0.0029 (0.0096)	-0.0038 (0.0113)
lead7	-0.0202** (0.0098)	-0.0098 (0.0112)	0.0101 (0.0091)	0.0100 (0.0110)	0.0022 (0.0106)	0.0251 (0.0160)	-0.0077 (0.0099)	-0.0114 (0.0111)	0.0068 (0.0097)	0.0010 (0.0116)
lead6	-0.0174* (0.0097)	-0.0077 (0.0110)	0.0147 (0.0090)	0.0137 (0.0116)	0.0081 (0.0105)	0.0266 (0.0165)	-0.0062 (0.0099)	-0.0109 (0.0105)	0.0075 (0.0096)	0.0020 (0.0121)
lead5	-0.0097 (0.0097)	-0.0037 (0.0110)	0.0073 (0.0091)	0.0066 (0.0107)	0.0058 (0.0102)	0.0234 (0.0153)	-0.0038 (0.0099)	-0.0072 (0.0109)	0.0003 (0.0096)	-0.0018 (0.0108)
lead4	-0.0109 (0.0096)	-0.0010 (0.0106)	0.0052 (0.0090)	0.0017 (0.0093)	0.0057 (0.0100)	0.0026 (0.0104)	-0.0021 (0.0099)	-0.0048 (0.0107)	0.0013 (0.0096)	-0.0045 (0.0108)
lead3	-0.0056 (0.0095)	-0.0011 (0.0108)	0.0036 (0.0089)	0.0036 (0.0091)	0.0054 (0.0099)	0.0174 (0.0137)	0.0033 (0.0100)	-0.0017 (0.0105)	0.0011 (0.0096)	-0.0016 (0.0111)
lead2	-0.0082 (0.0091)	-0.0035 (0.0095)	0.0031 (0.0090)	0.0042 (0.0091)	0.0037 (0.0097)	0.0142 (0.0133)	0.0027 (0.0099)	-0.0053 (0.0105)	-0.0004 (0.0095)	-0.0033 (0.0103)
D_flood	0.0090 (0.0066)	0.0022 (0.0072)								
D_storm			0.0030 (0.0064)	0.0039 (0.0063)						
D_earthquake					-0.0055 (0.0069)	0.0041 (0.0077)				
D_drought							-0.0080 (0.0072)	-0.0068 (0.0085)		
D_epidemic									-0.0171** (0.0069)	-0.0187** (0.0090)
Flood occurred			-0.0018 (0.0020)	-0.0046** (0.0023)	-0.0038** (0.0019)	-0.0076*** (0.0020)	-0.0040** (0.0020)	-0.0037*** (0.0014)	-0.0031 (0.0020)	-0.0035 (0.0022)
Storm occurred	0.0008 (0.0022)	-0.0012 (0.0037)			-0.0009 (0.0021)	-0.0025 (0.0028)	-0.0002 (0.0022)	-0.0016 (0.0031)	-0.0001 (0.0022)	0.0004 (0.0027)
Earthquake occurred	-0.0052* (0.0031)	0.0043 (0.0029)	-0.0026 (0.0030)	0.0038 (0.0040)			-0.0043 (0.0027)	0.0021 (0.0020)	-0.0032 (0.0027)	0.0024 (0.0027)
Drought occurred	-0.0048 (0.0034)	-0.0022 (0.0022)	-0.0028 (0.0033)	-0.0050 (0.0034)	-0.0053* (0.0030)	0.0010 (0.0018)			-0.0030 (0.0032)	-0.0015 (0.0019)
Epidemic occurred	0.0058* (0.0032)	0.0029 (0.0032)	0.0074** (0.0032)	0.0008 (0.0031)	0.0043 (0.0030)	-0.0016 (0.0024)	0.0073** (0.0031)	0.0026 (0.0027)		
Other disaster occurred	-0.0020 (0.0023)	0.0015 (0.0019)	-0.0024 (0.0022)	-0.0015 (0.0017)	-0.0020 (0.0020)	0.0026* (0.0013)	-0.0038* (0.0021)	-0.0010 (0.0015)	-0.0033 (0.0021)	0.0003 (0.0012)
Observations	2,967	2,967	3,036	3,036	3,174	3,174	3,243	3,243	3,243	3,243
R-squared	0.7835		0.7737		0.7613		0.7698		0.7713	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint test of leads: p=	2.92e-10	0.527	9.46e-05	0.874	1	0.00127	0.0411	0.268	0.130	0.990

Notes: This table reports the static effect  $\gamma$  of natural disasters on internal rate of return using the Two-Way Fixed Effects specification with confounders and pre-trends in Eq.3.3 (TWFE in odd-numbered columns) and the heterogeneity-robust Sun and Abraham, 2021's estimator (S-A in even-numbered columns). The estimation sample covers 69 years for floods (43 countries), storms (44 countries), earthquakes (46 countries), and droughts and epidemics (47 countries). Standard errors are in parentheses below the coefficients. Significance levels: \*\*\* 1%, \*\* 5%, \* 10%.

# APPENDIX C

## SAVING FOR A RAINY DAY: THE DYNAMIC IMPACT OF STORMS ON SAVING RATES

### **C.1 Tables**

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

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

*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.

## C.2 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  $\geq$  \$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  $\geq$  \$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 C.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\_multiplot* 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: Upper-ratio 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: Upper-ratio 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: Lower-ratio 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: Lower-ratio single storm (% damage to GDP  $<$  0.04). Outcome: Saving rate**

Figure C.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\_multiply* 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 C.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\_multiplot* 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.