Disaster preparedness and disaster response: Evidence from sales of emergency supplies before and after hurricanes[†‡]

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ABSTRACT

Government information warns households to acquire emergency supplies as hurricanes threaten and directs households to stay off roads after hurricanes make landfall. Do households follow this advice? If so, who, when, and how much? We provide novel evidence. We combine forecast and landfall data for U.S. hurricanes between 2002 and 2012 with extensive scanner data on sales of bottled water, batteries, and flashlights. We find that sales of emergency supplies increase when a location is threatened by hurricane. The bulk of the sales increases occur immediately prior to forecasted landfall. The average increase in sales after landfall is large and statistically significant. Observed emergency preparation as hurricanes threaten is moderately higher in coastal, wealthier, and whiter areas. Ex-post emergency responses after hurricanes make landfall are sharply higher in African American, lower income, and less educated areas. Our results suggest that households do not follow government advice.

KEYWORDS: Natural Disasters, Hurricanes, Emergency Supplies, Information, Advisories

JEL CODES: H84, Q54, D12, Q58

THIS DOCUMENT CONTAINS APPENDICES INTENDED FOR REVIEW AND ONLINE POSTING.

[†] Evidence is calculated (or derived) based on data from The Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business and via Third Party Agreement with the U.S. Department of Agriculture. The views in this paper are not attributable to USDA. The conclusions drawn from the Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

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1. Introduction

Governments provide information about contaminants in food, toxics in products, pollutants in the environment, and other risks. The practical effects of this government risk communication can be controversial. A growing literature shows that individual decision-makers may face cognitive limitations and experience bounded rationality. Some risks lack salience or involve uncertain net benefits realized only after over long time horizons. Empirically evaluating endogenous information interventions is challenging.[1]

This paper contributes to an unsettled area of the risk communication literature: emergency preparedness and response. We use supermarket scanner data to provide novel systematic evidence on sales of emergency supplies before and after hurricane landfalls. Government information warns households to acquire bottled water, batteries, and flashlights as soon as hurricanes threaten and directs households to stay off roads after hurricanes make landfall. We ask: Do households follow this advice? Do citizens acquire emergency supplies as storms threaten? If so, who, when, and how much? Do citizens ignore advice to stay off roads in order to acquire emergency supplies in the aftermath of a hurricane strike? If so, who, when, and how much? These are open questions. On the one hand, hurricane threats are salient, immediate, and actionable. On the other hand, individuals may systematically misperceive personal damage risks, mistrust government information, or exhibit unrealistic optimism given frightening warnings.

To explore how sales of emergency supplies change before and after hurricanes, we collect forecast and landfall data for all United States hurricanes between 2002 and 2012 from NOAA's National Weather Service. We merge in geographic, demographic, and weather data from a variety of sources. We combine hurricane and supplemental data with extensive scanner data from The Nielsen Company (US), LLC on sales of emergency supply goods in the hurricane-prone southeastern and gulf coast states. We focus on bottled water, batteries, and flashlights since they make up the key elements of emergency kits, which are emphasized by essentially every government risk communication for disasters.

We build on an interdisciplinary emergency preparedness literature that typically focuses on ex-post surveys and single storm case studies of relatively narrow populations.[2] In this paper,

[1] For reviews of a broader information disclosure literature, see Weil et al. 2006; and Loewenstein et al. 2014.
[2] Important antecedents exploring disaster preparedness and response around hurricanes include Norris et al. 1999; Sattler et al. 2000; Lindell & Hwang 2008; Kim & Kang 2010; and Meyer et al. 2014. More broadly, this paper builds on work exploring avoidance or averting behavior in response to environmental risk information (Graff Zivin &

we analyze actual market transaction data over wide spatial scales and dozens of hurricanes. Our data minimize survey and recall biases and may facilitate enhanced external validity. Purchases of emergency supply kit goods also represent unusually widespread and inexpensive disaster preparation and response behaviors; as the typical hurricane approaches a U.S. coastline, hundreds of thousands of households near and away from coasts are publicly advised to assemble emergency supplies to prepare for electricity and water outages.[3] Finally, this paper investigates heterogeneity in emergency preparedness and disaster response across race, income, and education.[4]

Given readily observable short-run behavioral responses, rich data, and conditionally exogenous treatments, our empirical exercise is straightforward. We first explore hurricane impacts on sales of emergency goods with event study graphs based on simple and intuitive treatment definitions. We compare all event study results to falsification exercises where hurricanes strike the same location on the same month and day, but one year earlier. We then investigate formal regression analyses using more sophisticated treatment definitions. We explore heterogeneity in treatment effects across income, race, education, and coastal proximity. We explore the evidence for potential threats to identification such as near misses, stockpiling, and evacuations. We confirm robustness to alternative empirical choices.

We find that sales of bottled water, batteries, and flashlights increase when a location is threatened by hurricane relative to counterfactual sales – i.e. expected sales in the absence of a hurricane threat. The bulk of the sales increases occur immediately prior to forecasted landfall. Sales of bottled water, batteries, and flashlights also increase relative to counterfactual sales after hurricanes make nearby landfall. We find that observed emergency preparation as hurricanes threaten is moderately higher in coastal, wealthier, and whiter areas. We find ex-post emergency responses after hurricanes make landfalls are sharply higher in African American, lower income, and less educated areas.

On balance, our results suggest that households do not follow government advice to

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Neidell 2009; Graff Zivin et al. 2011; Bäck et al. 2013; Ward & Beatty 2016). This study is also related to papers exploring the causes and consequences of hurricane damages (Vigdor 2008; Groen & Polivka 2008; De Silva et al. 2010; Michel-Kerjan 2010, Michel-Kerjan & Kousky 2010; National Academies 2012; Pindyck & Wang 2013; Gallagher 2014; Deryugina 2017; Deryugina et al. 2017; Gallagher & Hartley 2017).

[3] The scale of the intended audience differs markedly from more targeted hurricane recommendations urging, for example, select homeowners to protect their coastal properties from wind and wind-borne debris.

[4] As discussed later, the related literature suggests that responses to government information may be influenced by: (1) perceptions of risk (Norris et al. 1999; Peacock et al. 2005), (2) income and education, as proxies for ability to access and process information (Ippolito & Mathios 1995; Shimshack et al. 2007; Lindell & Hwang 2008), and (3) race and poverty, as proxies for public trust and other concerns (Fothergill et al. 1999; Alesina & La Ferrara 2002).

adequately stock emergency supply kits when hurricanes first threaten and to stay off roads after hurricanes make landfall. Observed ex-post behavior may generate significant externalities for rescue and emergency workers and slow recovery efforts like debris removal and utility restoration. We also find that gaps between government advice and actual behavior are largest among poorer, less educated, and minority populations. One implication is that more targeted information campaigns may be necessary to reach vulnerable populations that may prepare less, bear more substantial shocks, or both. A more sweeping implication is that interventions beyond risk communication may be necessary to manage emergency preparation and response for select populations.

We remain agnostic as to whether observed behavior is individually rational. On one hand, rational households may accurately assume that emergency supplies will be available when needed. Our results suggest private supply chains are able to manage increased demand following hurricanes, so it may remain individually optimal to ignore government advice and delay purchases of emergency supplies until strictly necessary. On the other hand, our main results and heterogeneity findings may be consistent with behavioral economic realities like bounded rationality, misperceived risks, and an 'ostrich effect' from scary personal threats.[5] Indeed, surveys suggest that coastal residents facing threatening hurricanes are aware of approaching storms, but they report limited worry and systematically underestimate duration of impacts, length of public service outages, and severity of flood damages (Meyer et al. 2014). Minority, lower income, and less educated populations are least likely to access, trust, and respond to risk information.[6]

2. Background

Hurricanes and damages

Tropical cyclones are circulating low pressure systems formed in the tropics or near-tropic regions. The Atlantic tropical cyclone season spans June to November, though storms are particularly likely between mid-August and mid-October. Tropical cyclones are classified according to their wind intensity using the Saffir-Simpson scale. Category 1 and 2 minor hurricanes have maximum sustained winds of 65-95 knots and category 3, 4, and 5 major

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[5] For the literature on behavioral economics and risk, see Viscusi 1990; Jolls et al. 1998; Kuran & Sunstein 1999; Thaler 2002; Kahneman 2003; Schwarz et al. 2007; Galai & Sade 2006; Karlsson et al. 2009; Oster et al. 2013; Lowenstein et al. 2013; Sharot 2011, Trumbo et al. 2013.

[6] See Ippolito & Mathios 1995; Fothergill et al. 1999; Sattler et al. 2000; Alesina & La Ferrara 2002; Shimshack et al. 2007; Lindell & Hwang 2008; Shimshack & Ward 2010; Kim & Kang 2010.

hurricanes have maximum sustained winds of 96 knots or greater.

Hurricane damages stem from high winds, large storm surges, flooding rains, mudslides, and/or landslides. Severe hurricanes can also spawn tornadoes. Damages include injuries and death, economic losses, and disruptions to basic services such as electricity and water. Loss of life caused by tropical cyclones has become far less common over time, and 22 of the 25 deadliest hurricanes between 1851 and 2010 made landfall prior to 1960 (NOAA 2011). Historically, deaths from hurricanes were caused by storm surges in low-lying coastal areas. The majority of hurricane-caused fatalities are now attributable to 'indirect deaths' occurring after storm landfall (Rappaport & Blanchard 2016). Economic damages are most often due to inland flooding (NOAA 2011). Hurricanes typically slow down after making landfall, exacerbating inland rains and flooding.

Utility disruptions

Disruptions to electricity, water, and other utilities are common after tropical cyclones. Department of Energy outage data were available for 14 of the hurricanes in our dataset. For these 14 hurricanes, 32 million customers (roughly 100 million people) lost power after landfall.[7] The average time between first disruption and full grid restoration was 6.9 days. Note that these figures are significant underestimates as they do not include smaller electricity providers, nor do they include disruptions due to more severe weather outside of the hurricane track. More generally, all residents in counties directly in the path of a significant hurricane would be expected to lose power, and localized outages lasting over a week are common in these areas. Large numbers of households in other areas would be expected to lose power as well, depending on the track, size, and severity of the storm.

Drinking water outages and boil water advisories – where water is available but not confirmed safe – are common following hurricane landfalls. Government advice cautions, "Do not drink tap water unless authorities say it is safe."[8] Physical damage to drinking water treatment facilities and debris in water sources can cause disruptions. Untreated sewage from combined sewer overflows may stress even properly functioning treatment systems. Power outages depressurize drinking water systems, with two consequences: decreased pressure may impact water availability at households' taps, and decreased pressure may allow pathogens to enter the

[7] The 14 hurricanes are Lili, Claudette, Isabel, Charley, Frances, Ivan, Jeanne, Dennis, Katrina, Rita, Wilma, Gustav, Ike, and Isaac. Data were collected by Inside Energy (http://insideenergy.org) from DoE Electric Emergency Incident and Disturbance Reports.

[8] See, for example, FEMA's "How to prepare for a hurricane."

distribution system. In either case, the safety of available water remains uncertain until testing is complete. Service disruptions and boil advisories typically last several days, but can and do last weeks or more after severe hurricanes.

While systematic data on drinking water disruptions and boil water advisories are unavailable, EPA data from the aftermath of Hurricane Katrina are illustrative. These data suggest that 100 percent of Alabama's water purification facilities were inoperable for 11 days following Hurricane Katrina. Approximately 40 percent of Mississippi's water purification facilities were inoperable for a week after landfall, and 5 to 10 percent of the state's facilities remained inoperable for one to two months after landfall. Roughly 40 percent of Louisiana's water purification facilities were inoperable after Katrina, falling to around 10 percent one month later before getting worse after Hurricane Rita caused additional disruptions. Scholars estimate that a quarter of the population of AL, LA, and MS lacked access to pure water for the entire month of September 2005, and 4 percent still lacked access to pure water three months later (Crowther et al. 2007).

The health effects of hurricane-related water service disruptions are poorly understood, often because surveillance systems are themselves compromised by hurricanes (CDC 2006a). At least one retrospective study showed marked correlations between extreme weather events and gastrointestinal illnesses among sensitive populations (Chui et al. 2006). Further, CDC observers in areas of AL, LA, and MS with severe infrastructure disruptions following Hurricane Katrina noticed frequent diagnoses of gastrointestinal illness (CDC 2006b).

Risk Communication and emergency supply kits

In light of likely utility disruptions and other threats, government information dissemination seeks to enhance household-level preparedness, reduce household-level impacts, and reduce household travel to acquire supplies after landfall. These programs emphasize education campaigns. Although some programs advise preparation at the start of hurricane season, in practice, the bulk of risk communication occurs as hurricanes approach. Messaging campaigns, conducted in partnerships with media organizations, typically begin about five or six days before storms make landfall. This timeframe extends slightly beyond the typical reach of the National Hurricane Center's forecast cones, which illustrate probable storm tracks accounting for uncertainty.

A central feature of virtually every hurricane preparedness information program is messaging on the importance of having an emergency kit ready ahead of storm landfall. All local, state, and federal preparedness materials include specific recommendations on which emergency supplies to have on hand. See Appendix A1 for FEMA's emergency kit instructions. Key elements of the kit include bottled water, batteries, and flashlights. After a storm, the default assumption is that electricity will not be available so flashlights and batteries are essential. Households are similarly advised to assume that tap water is unsafe until told otherwise. Boiling water is not a household-level coping strategy when power and gas services are unavailable. Note that public agencies advise households to purchase, rather than self-store, water due to the potential contamination of self-stored water when exposed to floods.

For the majority of tropical cyclones in the United States, large-scale evacuation orders are not issued. Mandatory evacuations orders are issued on relatively small scales to areas adjacent to coasts, near inland waterways, or dominated by temporary structures and mobile homes. Some large and strong storms, however, trigger mandatory evacuation orders for hundreds of thousands. Two features of evacuations are relevant for this research. First, evacuation orders occur shortly before landfall and well after households are formally advised to prepare disaster supply kits. Second, evacuations do not change government information related to acquiring emergency supplies as storms approach. Federal and state evacuation plans state "take your emergency supply kit" with you and "you will need the following supplies when you leave your home … flashlight with plenty of extra batteries …. water (at least one gallon per person [per day] is recommended; more is better)."[9]

Advice for after storms pass

After all significant tropical cyclones, federal, state, and local authorities recommend that individuals "stay off the streets" and "drive only if necessary" until advised otherwise.[10] In many cases, formal mandatory curfews are imposed at the city or county level. Roads may be flooded, blocked, or closed for several days. Flooding in rivers and streams often peaks well after landfall. Both walking and driving can be dangerous due to floods, downed power lines, weakened trees and buildings, and other hazards like dangerous wildlife. Most vehicles will lose control in 6 inches of water, float in 12 inches of water, and be swept away in 24 inches of moving water (Wang et al. 2017).

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[9] See, e.g., https://www.redcross.org/images/MEDIA_CustomProductCatalog/m12140138_Evacuation_Plan.pdf , the American Red Cross Evacuation Plan.

[10] See, for example, FEMA's www.ready.gov/hurricanes .

National Hurricane Center (NHC) data on all tropical cyclones from 2000 to 2014 indicate that the majority of hurricane-caused fatalities were attributable to 'indirect deaths' occurring after storm landfall (Rappaport & Blanchard 2016). Many deaths occurred days after landfall and even after the conclusion of severe weather. Around 20 percent of hurricane-caused indirect deaths between 2000 and 2014 were attributable to outdoor electrocution, falls, and car accidents (Rappaport & Blanchard 2016). For a recent hurricane, 78 percent of all hurricane-related drownings (which made up 42 percent of all hurricane-related deaths) occurred in motor vehicles (Wang et al. 2017). These deaths occurred despite direct messaging to stay off the roads and avoid walking or driving through flood waters.

Although walking or driving after hurricane landfall poses risks to individual health, it is worth emphasizing that mandatory curfews and government information campaigns demanding individuals stay off streets are issued to minimize externalities. Private vehicles may jam debris-strewn roads. Private vehicles hinder rescue, emergency, or recovery operations. Individuals putting themselves at increased risk may endanger first-responders coming to save them and may tie up emergency resources at critical times. Finally, crime and other high externality activities may be common when public safety resources are otherwise strapped (Leitner & Helbich 2011).

3. Data

In order to explore the impact of hurricanes on sales of emergency response goods, we collect hurricane data from NOAA's National Weather Service and National Hurricane Center (NHC). We match hurricane threats and landfalls with extensive scanner data on sales of bottled water, batteries, and flashlights from The Nielsen Company (US), LLC. We merge in geographic, demographic, and weather data from a variety of sources. Our final sample spans the 2002 through 2012 hurricane seasons. This section describes data construction in more detail.

Hurricane data

We define pre-landfall 'threatened' treatments using hurricane forecast data. We obtain forecast information, used to define hurricane tracks with cones of forecast uncertainty, from NOAA's Automated Tropical Cyclone Forecast System (ATCF). To construct forecasted threats, we use the NHC's official forecast. Official forecasts consist of real-time (t=0) location and intensity measures and are issued every 6-hours. In addition, every 6-hours, forecast models predict location and intensity at various points in the future, such as 24 (*t+24*), 48 (*t+48*), 72 (*t+72*), and 96 (*t+96*) hours into the future. Official forecasts are composites of dozens of meteorological models predicting storm track and intensity.

We define "after landfall" treatments using hurricane landfall data. We obtain precise landfall latitude, longitude, time, date, and intensity from NOAA's National Hurricane Center Atlantic Basin Best Tracks HURDAT2 system (the HURricane DATabase). We define "landfall" using the NOAA definition, i.e. the intersection of the surface center of a hurricane with a coastline. To keep the analysis transparent, we define a landfall location on a given day as the first official landfall that day, but still allow the same hurricane to make multiple landfalls on different days.

Appendix A2 details all hurricanes making landfall in the southeastern and gulf coast regions of the United States over the 2002 through 2012 hurricane seasons. Over this period, 22 hurricanes made landfall 26 times. Of the 22 hurricanes, 14 reached major hurricane status (category 3 or higher). These latter storms include ten of the costliest US hurricanes: Charley, Frances, Ivan, Jeanne, Dennis, Katrina, Rita, Wilma, Ike, and Irene.

Scanner data

We obtain our main retail scanner data from The Nielsen Company (US), LLC's ScanTrack supermarket scanner database. Data represent weekly sales from partner stores in ScanTrack markets across the continental United States. Markets correspond roughly to metropolitan statistical areas (MSAs), covering a major urban area and (typically) many surrounding counties. Most stores in the dataset are part of major supermarket chains, and the overall ScanTrack panel captures a substantial portion of total grocery sales in the United States.[11] Retail scanner data provide accurate summaries of sales at any given store at a given time. Retail scanner data avoid the strategic bias, recall bias, and observer bias that arise with survey or diary collection techniques.

Since hurricanes are relatively rare events, moving beyond case studies requires outcome data across many hurricane seasons and multiple geographic areas. We therefore gathered data from two distinct sources, each with strengths and limitations regarding scope and scale. First, we obtained store-by-week supermarket scanner data on bottled water and other beverages from 2002 to 2005. These data were collected by The Nielsen Company (US), LLC and made available for research purposes under a cooperative agreement between The Nielsen Company (US), LLC, the US Department of Agriculture, and our research team. The key advantages of these data are exact

[11] To preserve confidentiality and business sensitive information, we are unable to disclose further details on the supermarket chains and stores.

store street address and thus store-level treatments, as well as a time period during which an unusually large number of hurricanes made landfall. A limitation is that bottled water is the only observed emergency supply good. Second, we obtained store-by-week supermarket scanner data on bottled water, batteries, and flashlights from 2006 to 2012. These data were collected by The Nielsen Company (US), LLC and made available for research purposes by the Kilts Marketing Data Center. The key advantages of these data are multiple emergency supply goods and a long time series. The key limitations of these data are relatively coarse location measures that lead to county-level (rather than store-level) treatments, as well as a time period in which fewer hurricanes made landfall.[12]

Sample construction

We match hurricane forecasts and landfall data with sales data to construct store-week or county-week panels. The matching process is based on geographic distance and time intervals. We use Geographical Information Systems (GIS) to measure geographic distances between each geocoded location and hurricane forecast locations. We use GIS to measure geographic distances between each geocoded location and actually observed landfall locations. In the 2002-2005 sample, locations are defined using exact store addresses. In the 2006-2012 sample, locations are defined using the population-weighted county centroid. We measure time intervals by calculating the number of days between location-week start dates, dates of forecasted threats, and dates of actual landfalls.[13]

We merge area characteristics and weather data to each location. First, we construct socio-demographic indicators for each location using census 2000 measures for the 2002 to 2005 sample and census 2010 data for the 2006 to 2012 sample. Demographic variables include: 'Educated,' where the location lies within an area with above median college attainment for the sample; 'Income,' where the location lies within an area with above median income for the sample; and 'Black / African American (Afr. Amer.),' where the location lies within an area with above median percentage of the population self-identifying as 'black or African American' for the sample. We

[12] In a robustness section, we explore individual-by-day consumer panel data from 2004-2012. These data were collected by The Nielsen Company (US), LLC and made available for research purposes by the Kilts Marketing Data Center. The key advantage of these data are daily observations, which allow us to better understand the timing of responses to forecasts. Limitations are sparse geographic coverage and possible measurement error after landfall.

[13] Reporting weeks for 2002-2005 data begin on a Saturday and end on a Friday. Reporting weeks for 2006-2012 begin on a Sunday and end on a Saturday. We discuss exact treatment variable construction in the next section, and explore implications and robustness to weekly vs. daily data later in the paper.

use indicator variables for all demographic heterogeneity explorations to ease interpretation, although findings are robust to continuous variable definitions. Second, we construct geographic indicators for proximity to coasts. 'Coastal' indicates that the county is adjacent to the Atlantic Ocean, Gulf of Mexico, Lake Okeechobee, or Atlantic or Gulf estuarine bays.[14] Third, we merge in temperature and precipitation data from the Global Historical Climatology Network (GHCN). Specific variables are total precipitation, weekly mean temperature, weekly minimum temperature, and weekly maximum temperature. We assign each location's weather based on observations at the nearest weather station with complete data. Results are robust to other assignment mechanisms.

Sample and outcomes

Our main outcome variables are derived from total revenues for bottled water, battery, and flashlight sales at food stores in a given location during a given week. To construct these variables, we begin with Universal Product Code (UPC) level data on revenues in a given store during a given week. For each product category, we aggregate over all UPC codes to obtain total revenue at the store-by-week level. For example, we aggregate over more than 3,300 unique bottled water UPC codes, more than 1,900 distinct battery UPC codes, and over 1,900 unique flashlight UPC codes to obtain total category revenue for a given store in a given week. When creating county-by-week measures, we further aggregate total category revenues across all observed stores in the county.[15]

Using the above aggregation and matching procedures, we create store-by-week and county-by-week datasets reflecting all tracked sales of bottled water between 2002 and 2005 and all tracked sales of bottled water, batteries, and flashlights between 2006 and 2012. We then restrict our sample to the Gulf coast and Atlantic coast states: Texas, Louisiana, Mississippi, Alabama, Florida, Georgia, South Carolina, North Carolina, and Virginia. Sales from South Carolina are only tracked in our 2006 to 2012 datasets. With or without South Carolina, our states experienced the overwhelming majority of U.S. hurricane activity between 2002 and 2012.

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[14] These coastal indicators correspond to locations where NHC hurricane watches and warnings may be issued. Earlier versions of the paper also included geographic variables like 'store within 5 miles of coasts', 'within 50 miles of landfall', and official 'FEMA' disaster area. Results related to these variables offered little additional insight beyond results in the present draft, so we omit them.

[15] Results for bottled water are robust to defining sales by volume. We begin with UPC-level data on the number of units sold in a given store-week. We convert from units sold (pack size) to a standardized measure like ounces using detailed product-specific information provided by Nielsen. We then aggregate to the store-by-week category-specific quantity and/or the county-by-week category-specific quantity. Because flashlights and batteries are not measured in common units, we cannot aggregate sales of flashlights and batteries across UPC codes in a straightforward manner.

Figure 1 summarizes store locations with hurricane landfall locations overlaid. We highlight counties with one or more sample stores at any point during our 2002 to 2012 sample period. Counties containing stores are nearly identical between the 2002 to 2005 and 2006 to 2012 samples, save for South Carolina. Overall, we observe reasonably broad geographic coverage of stores both near and far from the Atlantic and Gulf coasts. Sample stores are roughly evenly distributed throughout Virginia and the Carolinas. Stores are otherwise somewhat concentrated near the Louisiana, Mississippi, and Alabama Gulf coasts; Dallas; Houston; Atlanta; Tampa and elsewhere on the west coast of Florida; and the east coast of Florida including Miami and Jacksonville.

Table 1 summarizes sales for our roughly 3,000 sample stores. Here we note several features of the data. Over all observed weeks (not just weeks during hurricane seasons), our average store sold \$1,931 (2002 to 2005 sample) and \$2,941 (2006 to 2012 sample) of bottled water per week. Our average store sold \$428 of batteries and \$17 of flashlights per week. For the 2006 to 2012 sample, our average county sold \$16,241 of bottled water, \$2,366 of batteries, and \$94 of flashlights. Sales of all goods were seasonal. Bottled water sales peaked between June and August and fell noticeably during winter months. Battery sales peaked in December and fell during spring months. Flashlight sales in our southern states peaked in August and fell during spring months. Long-run trends were less pronounced. Sales of bottled water increased markedly until about 2007 and remained relatively steady thereafter. Sales of batteries and flashlights did not exhibit any obvious long-run trends. Sales varied markedly across space. Texas stores sold the most bottled water and batteries per store, and Florida stores sold the most flashlights per store. Alabama and South Carolina stores tended to sell less of all goods per store.

Table 1 also summarizes area characteristics and weather. For perspective, the 2010 census suggests that the median household income across the United States was roughly \$49,000, the percent of the population self-identifying as black or African American was roughly 14 percent, and the percent of the population with a bachelor's degree or higher was roughly 28 percent. Compared to these national averages, our gulf coast and southeastern locations are poorer, more African American, and less educated. As expected, our two samples (2002 to 2005 vs. 2006 to 2012) share statistically similar socio-demographic characteristics and weather despite different levels of aggregation. The only possible exception is that 29.8 percent of stores in the 2002 to 2005 sample were located in coastal counties while 18.3 percent of counties in the 2006 to 2012 sample were officially designated as coastal counties. However, these differences are largely composition effects from the unit of observation. 28.5 percent of sample stores in the 2006 to 2012 were located in coastal counties.

4. Empirics

Our empirical objective is to identify how sales of bottled water, batteries, and flashlights change with hurricane threats and after hurricane landfalls. We begin our analysis with a transparent graphical event study approach. For these purposes, we define treatments as simply and intuitively as possible. We subsequently turn to more formal econometric analyses and more sophisticated treatment definitions.

We interpret all results as differences between expected sales and actual sales when hurricanes threaten or make nearby landfall. In event studies, we explore if and how sales at a given location depart from mean sales when storms threaten or after storms make nearby landfall. In econometric analyses, we explore if and how sales depart from counterfactual sales for that same location and time had there been no hurricane threat or strike. In all analyses, our maintained hypothesis for pre-landfall threats is based on a null of “no incremental preparation as hurricanes approach.” Our maintained hypothesis for the post-landfall period is based on a null of “sufficiently prepared.” Our view is that households that had enough emergency supplies on hand would not purchase unusually large amounts of emergency supply goods (relative to a counterfactual) immediately following hurricane landfalls unless they had a need to do so.

Event studies

We begin by defining temporal dimensions of treatment, i.e. what constitutes an ‘event’? We assume that a store-week is impacted by a given hurricane event if the hurricane made landfall nearby during that particular sales week. Thus, we simply examine sales of emergency supplies in the weeks preceding and following the observed landfall date. For each impacted store, we normalize the week containing the storm landfall date to $t=0$. We then assign the 10 sales weeks preceding the landfall date at each impacted location a time value of $t=-1,-2,\ldots,-10$ and the 10 sales weeks following the landfall date at each impacted location a time value of $t=+1,+2,\ldots,+10$. Finally, we discard sales weeks not in the window $t=-10,-9, \ldots, 0, \ldots, +9, +10$.

Given a temporal dimension of treatment, we now turn to spatial aspects of treatment, i.e. what constitutes ‘nearby’? We define a store as impacted by a landfall if the store’s location (defined as either the exact address or the populated weighted county centroid) lies within 100 miles of a storm's landfall location. Figure 1 illustrates treatments defined in this manner – sales at store locations in the shaded areas are analyzed in the event study graphs. We choose a 100 mile radius for four reasons. First, this distance corresponds roughly to the NHC's historical '2/3 probability circle' for Atlantic Basin storms for one to three days prior to expected landfall. This means that approximately two-thirds of historical NHC errors for forecasts made one to three days out will fall inside of this radius. Second, by construction, this distance roughly corresponds to the radius of tropical cyclone forecast 'cones of uncertainty' that residents themselves would encounter in media reports during the days leading up to landfall. Third, the 100 mile radius loosely translates to the '1-2-3 rule' described below that we later use to inform more formal threat treatments based official NHC forecasts. Fourth, the implied 200 mile impact diameter approximates the coastal length of many tropical cyclone 'watches' where residents are directed to be on guard and prepare their homes. It also fully encompasses the NHC's official definition of hurricane 'direct hit', which is based on ovals drawn from landfall points with centers shifted to the right to account of asymmetrically higher winds east of landfall.[16]

For perspective, we also explore falsification exercises that replicate all event study graphs using hurricane landfall dates shifted forward in time by exactly one year. If unobserved location-specific or seasonal confounders are driving results in event study graphs, we would expect falsification checks to produce false positives. Note that simulated landfalls occur in the precise location and on the same month and day as the actually observed landfalls, but one year earlier. [17]

Figure 2 presents the event study and corresponding falsification check results, with pointwise confidence intervals overlaid. In the top panel, we find changes in average bottled water sales around the landfall dates of hurricanes during the 2002 to 2005 seasons. In contrast, we fail to detect large changes in bottled water sales around false landfalls. In the second panel, we find changes in average bottled water sales around the landfall dates of hurricanes during the 2006 to 2012 seasons. Again, we fail to detect large changes in bottled water sales around false landfalls. Results in the third and fourth panels echo these results for battery and flashlight sales. We see large changes in battery and flashlight sales around landfall dates of actual hurricanes but no

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[16] Results are not particularly sensitive to alternative radii choices. Larger radii generate similar quantitative impacts and smaller radii typically generate larger but noisier quantitative impacts. Timing and qualitative results are similar.
[17] To be precise, we advance 2003-2005 storms one year and 'wrap' 2002 storms to 2005. Similarly, we advance 2007-2012 storms one year and 'wrap' 2006 storms to 2012. In principle, one could postpone landfalls by one year for falsification tests but, in practice, hurricanes can have lasting effects so this procedure may be misleading.

discernable changes around falsification landfalls.

Results in Figure 2 provide some evidence of sales responding to hurricane landfalls, but these results are subject to limitations. First, event study findings may fail to fully account for confounders like seasonality, long-run time trends, and location-specific effects. Second, temporal aspects of treatment are likely oversimplified. Locations may be threatened by hurricanes even if they do not subsequently experience strikes. Moreover, simplified temporal definitions of treatment make untangling the timing of changes in sales difficult to assess. The event study graphs can blur distinctions between pre-landfall sales and post-landfall sales. Third, event study approaches make exploring heterogeneity in treatment effects challenging.

Econometric Analysis

In this section, we describe our more formal analysis. We begin by describing treatment definitions that facilitate separate identification of ex-ante disaster preparedness and ex-post disaster response. First, we refine our 'before landfall' treatment definitions to reflect a 'threatened' treatment. We define 'threatened' using official NHC forecasts. At every 6 hour increment following a storm's formation, we obtain the location of the observed eye (time $t=0$) as well as the forecasted locations for the eye 24, 48, 72, and 96 hours into the future (times $t+24$, $t+48$, $t+72$, and $t+96$). We then apply the common '1-2-3 rule' to determine whether a given location is threatened at a given time. The '1-2-3 rule' underpins the familiar 'cone of uncertainty' presented to the general public and reflects the fact that rounded long-run historical average forecast errors were +/- 100 nautical miles (nmi) 24 hours into the future ($t+24$), +/- 200 nmi 48 hours into the future (t+48), and +/- 300 nmi 72 hours into the future ($t+72$). Our treatments mimic NHC's process for creating smoothed cones of uncertainty by drawing circles of different radii around specific forecast points. At 6 hour increments after storm formation, we draw circles with radii of 100 nmi, 100 nmi, 200 nmi, 300 nmi, and 300 nmi around forecasted eye locations at $t=0$, $t+24$, $t+48$, $t+72$, and $t+96$.[18] A location-day is then 'threatened' if the location lies within any circle on that day, and a location-week is 'threatened' if any of its location-days were threatened that week.[19] To be clear, a location for the 2002-2005 sample is defined by exact store addresses

[18] Precise radii used to construct NHC cones of uncertainty for public dissemination have changed over time. We use traditional definitions, but note that our results are robust to alternative radii choices. Our 300 nmi choice for t+96 is small relative to historical forecast errors, but results are not sensitive to this choice.

[19] Earlier versions of this paper used a less transparent definition for 'threatened'. Results based on the current definition are more realistic and interpretable, and lead to results that are similar to results based on earlier definitions.

and a location for the 2006 to 2012 sample is defined by a population-weighted county centroid.

We next operationalize a more formal 'after landfall' treatment definition. We define a location-day as 'after landfall' treated if a hurricane made landfall within 100 miles of the location one, two, three, or four days ago. We choose 100 miles for the reasons outlined in detail in the event study: the implied impact diameter approximates the coastal length of hurricane watches and nests the NHC definition of 'direct hit'. We choose a four day window to maintain parallel structure with 'threatened' treatments based on forecasts up to *t+96* hours into the future. A location-week is deemed 'after landfall' treated if any of its location-days are defined as 'after landfall.'

All regressions also include indicators for 'struck.' 'Struck' equals one for a location-day if a hurricane made landfall within 100 miles of that location at any point during that day. 'Struck' equals one for a location-week if any day during the week contained a nearby landfall.

Note that treated location-weeks can simultaneously contain days 'threatened', 'struck', and 'after landfall.' Variation over a long time series and across dozens of hurricanes facilitates separate identification of these effects. We verify that there is sufficient variation across location-week treatment types and confirmed that location (population) characteristics are statistically indistinguishable across different treatment categories. Appendix Table AX1 summarizes these results. We also later explore robustness to data observed at a daily level, which facilitates another approach to separately identifying exact timing.

Regression approach

Estimating panel models with our data is straightforward. For location i in week t, we regress the log of sales for bottled water, batteries, or flashlights on 'threatened' and 'after landfall' treatments and controls. We log outcome variables because revenues are approximately log-normal with a long right tail, and because semi-log specifications facilitate interpretation of results.

We address possible confounders related to location-specific characteristics, long-run trends, seasonality, and weather with location (store or county) fixed effects α_i, year fixed effects $\lambda_{y(t)}$, month fixed effects $\delta_{m(t)}$, and weather variables $W_{i,t}$. Although the precise location and timing of hurricanes is plausibly exogenous, hurricanes tend to make landfall in warmer locations at warmer times of the year. Hurricanes are not evenly distributed over years. Locations commonly affected by hurricanes may have non-representative socio-demographic characteristics due to long-run Tiebout sorting and other factors. As such, we aim to control for observed and unobserved factors possibly correlated with both hurricane treatments and sales of emergency supplies.[20]

Our empirical specification can be summarized as follows:

$$\{1\} \qquad \ln SALES_{i,t} = \alpha_i + \beta_1 Threatened_{i,t} + \beta_2 Struck_{i,t} + \beta_3 After_{i,t} + W_{i.t}\Omega + \delta_{m(t)} + \lambda_{y(t)} + \varepsilon_{i,t}.$$

We also explore heterogeneity in treatment effects across location-specific characteristics. Responses to government information may be influenced by: (1) perceptions of risk; (2) income and education, as proxies for ability to access and process information; and (3) race and poverty, as proxies for public trust and other concerns. To this end, we explore heterogeneity across proximity to coasts, educational attainment, median household income, and proportion of residents self-identifying as black or African American. Given location characteristics X_i, we explore heterogeneity as follows:

$$\{2\} \quad \ln SALES_{i,t} = \alpha_i + \beta_1 Threatened_{i,\mathrm{t}} + \beta_2 Threatened_{i,t} \times X_i + \beta_3 After_{i,t} + \beta_4 After_{i,t} \times X_i + \beta_5 Struck_{i,t} + W_{i.t}\Omega + \delta_{m(t)} + \lambda_{y(t)} + \varepsilon_{i,t}.$$

We caution against over-interpreting interaction effects, as it remains possible that interacted location-specific factors may be correlated with other observed and unobserved characteristics.

Estimation Notes

Note three estimation details. First, we cluster all standard errors at the county-level. The implicit assumption is that, while fixed effects control for time-invariant cross-sectional correlations, we assume no time-varying spatial correlation across counties. Second, in order to make treatment and control periods as comparable as possible, we run main regressions only for location-weeks during official hurricanes seasons. Third, we include locations that are both treated and never treated in the regression analyses. Given location fixed effects, never treated locations do not directly inform identification of treatment effects but do help to better identify the effects of seasonality, weather, etc.

For the earlier 2002 to 2005 period, our final sample reflects sales from 3,311 stores in 517 counties and 105 weeks during June through November. Since we observe exact street address in the 2002 to 2005 sample, a store-level analysis reduces measurement error. The final dataset contains 313,655 store-week observations, or more than 90 percent of a fully square dataset. 2,626 stores, or approximately 80 percent, have complete bottled water sales data for all 105 weeks. We are unable to detect any correlation between missing data and hurricane treatments; regressions of

[20] Earlier versions of the paper demonstrated robustness to other methods to control for time-invariant factors, approaches that allow time trends to vary locally, and approaches to that allow seasonality effects to vary locally.

'missingness' on treatment with or without controls including month-of-year fixed effects, year fixed effects, and store fixed effects yield no significant relationships.

For the later 2006 to 2012 period, our final sample reflects sales from 2,783 stores in 504 counties and 183 weeks from June through November. Here we observe store location information only at the county-level, so we also define treatments and outcomes at the county-level. As a result, we aggregate individual store sales to the county level and use the county-week as our unit of analysis.[21] By construction, the final dataset contains a square 92,232 county-week observations. We see zero dollar or possibly missing bottled water sales for only 6 of the 509,289 observed store-week observations prior to aggregating to the county-week. We see zero dollar or possibly missing battery sales for only 98 of the observed 509,289 store-week observations prior to county-week aggregation. We see zero dollar or possibly missing flashlight sales for roughly 37 percent of observed 509,289 store-week observations prior to county-week aggregation. Given that store-weeks with zero flashlight sales are essentially always accompanied by sales of other goods during that store-week, we believe the zeros in the flashlight data are true zeros rather than missing data.

5. Results

Regression results, corresponding to equation {1}, appear in Table 2. We find statistically significant increases in sales of all emergency supplies when locations are threatened by hurricanes. For the 2002 to 2005 hurricane seasons, bottled water sales at threatened stores increased roughly 5 percent relative to counterfactual sales. For the 2006 to 2012 hurricane seasons, bottled water sales at threatened locations increased roughly 16 percent relative to counterfactual sales. Battery and flashlight sales at threatened locations increased 51 and 141 percent relative to counterfactual sales.[22] We also find statistically significant increases in sales of all emergency supplies relative to counterfactual sales after nearby landfall. For the 2002 to 2005 hurricane seasons, bottled water sales following a nearby landfall increased roughly 63 percent relative to counterfactual sales. For the 2006 to 2012 hurricane seasons, bottled water sales following a nearby landfall increased roughly 34 percent relative to counterfactual sales. Battery and flashlight sales following nearby landfall increased 215 and 220 percent relative to counterfactual sales. All coefficients are precisely estimated.

Although we defer a full discussion of results in Table 2, we highlight two results here.

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[21] Store-level analysis for the 2006 to 2012 sample yields qualitatively similar results.

[22] Given semi-log specifications with dummy explanatory variables, we interpret coefficients as 100 ($e^{\beta} - 1$).

First, consistent with the event study graphs, we see statistically significant and economically important changes in a location's sales of emergency response goods when a location is threatened by a hurricane. Second, we see statistically significant and economically important increases in sales of emergency supplies after a hurricane makes landfall near the location.

Table 3 presents heterogeneity results, corresponding to equation {2}. Coefficients on 'threatened' treatments appear above the dotted lines in the table. First, we note statistically significant increases in sales of emergency supplies (relative to counterfactual sales) at baseline locations when threatened. We also find some suggestive evidence for heterogeneity in sales when locations are threatened. For the 2006 to 2012 hurricane seasons, bottled water, battery, and flashlight sales at threatened coastal locations increased around 90, 120, and 100 percent more than bottled water, battery, and flashlight sales increased at threatened inland locations. For the 2006 to 2012 hurricane seasons, bottled water, battery, and flashlight sales at threatened locations with higher income populations increased around 80, 80, and 70 percent more than bottled water, battery, and flashlight sales increased at threatened locations with lower income populations. Similarly, bottled water, battery, and flashlight sales at threatened locations with more educated populations increased around 110, 90, and 110 percent more than bottled water, battery, and flashlight sales at threatened locations with less educated populations. We find no consistent evidence for racial heterogeneity when hurricanes threaten. Despite generally consistent patterns in sales responses to threats across goods for the 2006 to 2012 period, the 2002 to 2005 period generated less consistent heterogeneity patterns for hurricane threats. Bottled water sales at threatened locations with more educated and higher income populations increased less than those at threatened locations with less educated and lower income populations.[23]

Table 3 also presents results on heterogeneity in 'after landfall' treatment effects. Coefficients on 'after landfall' indicators appear below the dotted lines in the table. Here, we document marked heterogeneity in 'after landfall' changes in sales of emergency supplies relative to counterfactual sales. For bottled water and flashlight sales during the 2006 to 2012 period, sales following a nearby landfall did not differ from counterfactual sales in locations with below median proportions of black / African Americans. In contrast, sales of bottled water and flashlights

[23] One possible explanation for the difference in heterogeneous responses to threats across samples is that extensive media coverage of 2005's Hurricane Katrina may have differentially changed the salience of hurricanes. Other possible explanations include different average hurricane intensities or different specific threat and landfall points.

following landfall increased 65 and 240 percent in locations with more black / African Americans. 2002 to 2005 sales of bottled water and 2006 to 2012 sales of batteries following a nearby landfall increased roughly 135 and 285 percent more in locations with more black / African American populations than in locations with fewer black / African Americans. 2002 to 2005 bottled water, 2006 to 2012 bottled water, battery, and flashlight sales following a nearby landfall increased around 30, 95, 81, and 81 percent less in locations with higher income populations than in locations with lower income populations. We also find some evidence that bottled water and battery sales following a nearby landfall increased less in locations with more educated populations. For example, 2006 to 2012 bottled water and battery sales following nearby landfall increased around 80 and 60 percent less in locations with more educated populations than in locations with less educated populations.

As before, we defer full interpretation of these results but highlight key findings. First, we confirm our main results from Table 2. Sales of emergency supplies increase relative to counterfactual sales when locations are threatened prior to landfall. Sales of emergency supplies increase relative to counterfactual sales following nearby landfall, and the magnitudes are large. Second, at least in later years, sales of emergency supplies increase modestly more when threatened in coastal areas and in locations with wealthier and more educated populations. Third, sales of emergency supplies increase substantially more after landfall in locations with more African American, lower income, and less educated populations.

Household-by-Day Analysis

One concern with weekly data is that the analysis above cannot precisely establish when during the week outcomes changed. To explore the issue, we obtained household-by-day consumer panel data collected by The Nielsen Company (US), LLC and made available for research purposes by the Kilts Marketing Data Center. Our final household-by-day sample, which includes all households in our southeastern and gulf coast states over all available years, covers 37,317 households in 923 counties between the years of 2004 and 2012. Although these data provide additional insight into the exact timing of behavior, limitations include noisy data, sparse geographic coverage, and imprecise geolocation information. In the context of research on natural disasters, an additional concern is possible measurement error from reporting bias. Households may underreport purchases when threatened by hurricanes, and measurement error may increase when threats are imminent. Measurement error may be especially likely after hurricane landfalls due to the widespread power outages and other factors discussed in our background section. As such, we will cautiously interpret 'threatened' treatments and we will not fully interpret 'after landfall' treatments.

The above caveats notwithstanding, our household-by-day analysis parallels the analyses above. We begin with graphical event studies based on the simplified treatments. We proceed to more formal econometric analysis based on the refined treatments described previously. In addition to different units of analysis, a distinction of the household analysis is that we use survey weights provided by The Nielsen Company (US), LLC.

Figure 3 presents household-by-day event study results. Note that the graphs depict conditional means, where expenditures are recorded (possibly as zeroes) for a given good on a given day only if the household recorded any purchases from any store that day. In general, the event studies suggest meaningful changes in conditional average bottled water, battery, and flashlight purchases around the time of hurricane landfall. Visual inspection suggests that peak purchases of all emergency response goods occur very shortly (roughly 1 day) before landfall and nearly all increases in purchases in response to hurricane threats occur within 3 days of landfall.

Figure 3 also depicts limited changes in emergency supply purchases after hurricane landfall. Although it possible that this result reflects true behavior, we believe it more likely reflects measurement error due to power outages and other factors that make home-scanning impossible or lower priority. To bolster this conjecture, we explored the impact of our treatments on average total expenditures for all goods (not just emergency supplies) and the percent of households recording zero purchases for all goods. Figure 4 presents results. We find that average total expenditure on all goods falls significantly the day of landfall and remains below average for several days. Similarly, we find that the number of households recording no purchases of any goods increases sharply the day of landfall and remains higher than usual for four to five days. Given strong evidence of increased store-level sales after landfall in our primary analyses, and given sparse geographic coverage in the household-level data, we cautiously (but not definitively) interpret 'after landfall' household-by-day event study results as measurement error rather than actual purchase behavior.

Table 4 presents results from more formal econometric analyses of household-by-day data. For all intents and purposes, the analysis mimics our main analyses summarized in Table 2 with the exception that 'threatened' treatments are now defined by the specific (*t+24*, *t+48*, *t+72*, *t+96*) forecast threatening a given location on a given day and group fixed effects are household fixed effects. Table 4 reports both conditional and unconditional results. We find that average purchases of bottled water, batteries, and flashlights between 2004 and 2012 begin increasing (relative to counterfactual purchases) when households are threatened by a hurricane expected to make landfall in next 96 hours. Average purchases of all emergency supply goods, however, increase more sharply as the time to landfall shrinks. Peak purchases occur 1-2 days before forecasted landfall; for example, increases in conditional purchases of bottled water, batteries, and flashlights are more than 2, 4, and 9 times greater for *t+24* threats than for *t+96* threats.

Other sensitivity and robustness

We explored whether treatment effects differed between 'strikes' and 'near misses.' For this exercise, we constructed all treatment indicators for 'struck' observations as in the main analysis. We defined 'near miss' locations as threatened but not struck by a hurricane making landfall within 100 miles. For these observations, we constructed ex-ante threatened treatments as in the main analysis and constructed ex-post 'after landfall' treatments based on hypothetical simulated landfalls.[24] Table 5 summarizes results. We find that hurricane threats lead to higher sales of emergency response goods in areas that subsequently experience a landfall (i.e. are 'struck'). Increases in purchases of bottled water (2002 to 2005, 2006 to 2012), batteries, and flashlights in response to hurricane threats were roughly 2, 5, 10, and 12 times higher for areas threatened and struck than for areas threatened and not struck. We suspect these results reflect differential treatment intensities, as areas threatened and struck will typically be threatened for more days and on days closer to actual or forecasted landfall.[25] As expected, increases in sales after landfall are also greater for locations threatened and struck than for locations threatened and not struck (i.e. hurricane landfall is more than 100 miles away). Increases in sales of bottled water (2002 to 2005, 2006 to 2012), batteries, and flashlights after landfall were roughly 30, 400, 850, and 5300 percent greater for locations threatened and subsequently struck than for locations threatened and subsequently not struck. Increases in emergency supply sales were positive after

[24] Mechanically, the after landfall 'treatment' effect is essentially a placebo indicator which is turned on during the week of landfall (not nearby, since the location was not actually struck) for locations that were threatened at some point during the current week, the previous week, or the week before that. Results are robust to defining this placebo 'after landfall' treatment for locations that were threatened at some point this week or the previous week only.

[25] An alternative explanation is that locations that are struck have systematically different populations than locations that are threatened and not struck. We found no evidence for this. Summary statistics for income, race, and education are statistically indistinguishable and practically similar between struck and threatened but not struck areas.

landfall in areas threatened and subsequently missed, however, suggesting that areas threatened and 'missed' may have still experienced at least some effects.

We considered the role of stockpiling for our analysis, since disaster response goods are storable over the short to medium run. We acknowledge that we are unable to fully address the issue. However, we note three points. First, we find large increases in sales of flashlights in impacted areas after hurricane landfall. Flashlights are durable goods so they are not typically expended by incremental use. We therefore believe our results are more consistent with insufficiently prepared emergency kits rather than adequate stockpiling. Second, we examined changes in sales of emergency supplies during the first week of each hurricane season. Agencies engage in outreach efforts at the opening of hurricane season in early June. We therefore regressed all store-weeks (not just hurricane season store-weeks) on indicators for first week of the hurricane season and controls. Results in Appendix Table AX2 suggest that average sales of emergency supplies during the first week of hurricane season do increase, but in general the increases are small. Appendix Table AX2 also documents that African Americans areas stockpile less and more coastal, higher income, higher education areas stockpile more during the first week of the hurricane season. Third, we examined changes in sales of emergency supplies during the week of the first hurricane making landfall anywhere in the US in a given year.[26] The idea is that hurricanes in the media may trigger households to prepare emergency kits. We therefore regressed all store-weeks (not just hurricane season store-weeks) on indicators for week of the first hurricane and controls. Results in Appendix Table AX3 are not consistent across goods and across time periods and thus difficult to interpret. Nevertheless, we note that stockpiling around the first hurricane of each season is negative on average and any observed positive stockpiling is small. We caution that stockpiling exercises summarized in Appendix Tables AX2 and AX3 are imperfect, given difficulties fully disentangling treatment effects from short-run seasonality.

We considered the role of evacuations for our analysis. We note three points. First, government information suggests that evacuations do not mitigate the need to acquire emergency supplies when hurricanes threaten. Risk communications directly advise households, "you will need the following supplies when you leave your home … water (at least one gallon per person per day) … flashlights …. batteries." Second, evacuations bias coefficients towards zero so 'after

[26] Earlier versions of this paper explore the effects of experience with hurricane activity in the more distant past. Early season stockpiling was somewhat larger in areas previously affected by a direct hit.

landfall' treatments may be underestimates. Third, we replicated our analysis disaggregated by evacuation status. We compared changes in sales of emergency supplies around hurricanes with and without large-scale mandatory evacuation orders. We identified orders by searching media reports for hurricanes associated with requirements for at least several hundred thousand individuals to leave home. Appendix Table AX4 presents results. Hurricanes without large-scale evacuations were generally associated with small but positive increases in sales of emergency supplies as hurricanes threatened and after landfall. Hurricanes with large-scale evacuations were associated with considerably larger increases in sales of emergency supplies as hurricanes threatened and after landfall. Disaggregating results by evacuation status is an imperfect exercise, since mandatory evacuation orders are correlated with storm intensity. Nevertheless, our robustness checks remain consistent with the main findings of increases in emergency supply sales when hurricanes threaten and increases in emergency supply sales following landfall.

One alternative interpretation of increased sales of emergency supplies following landfall is that households are restocking all supplies after delayed shopping trips. We believe our results are not consistent with this interpretation. First, we find very large increases in sales of flashlights in impacted areas after hurricane landfalls. Since flashlights are not expended during a storm, the result seems more in keeping with emergency supply needs than with a general household restocking effect. Second, we explored purchases of emergency supplies disaggregated by common package sizes. Appendix Table AX5 presents results. We find that increases in purchases of bottled water and batteries after hurricanes are disproportionately concentrated among the largest package sizes. We believe these results are consistent with emergency supply needs, as gallon bottles and 8-packs of batteries are more naturally viewed as disaster response goods than standard 16.9 ounce water bottles and 2-packs of batteries. Third, we explored changes in sales of beverages beyond bottled water. The choice of beverages was driven by data availability during our 2002 to 2005 sample. Appendix Table AX6 summarizes results for the 2002 to 2005 period. Although we observe increases in sales of soda and juice products after hurricane landfalls, magnitudes are roughly 8 times smaller than comparable increases for bottled water.

We performed a host of additional robustness checks. We briefly note three findings. First, results were not especially sensitive to other approaches to defining geographic dimensions of treatment. For illustration, Appendix Table AX7 presents results when defining 'after landfall' impacts by different radii. Radii of 100, 125, and 150 miles or more generated statistically indistinguishable treatment effects. In the store-level analyses, radii smaller than 100 miles tended to produce stronger 'after landfall' effects, as perhaps expected. In the county-level analysis, radii smaller than 100 miles introduced considerably more statistical noise but for the most part generated results that were in the spirit of our key results. Appendix Table AX8 presents results where we exclude locations more than 100 miles from the coast from 'threatened' treatments. This exercise imposes greater symmetry on ex-ante and ex-post treatments; potentially treated locations are essentially the same for ex-ante and ex-post treatments. 'Threatened' treatment effects were similar but slightly larger than our main results. Second, results for bottled water sales were not sensitive to defining outcomes based on volume rather than revenues or expenditures. It is difficult to conceptualize standardized volume measures for batteries and flashlights, so we did not construct volume measures for these goods. We do note that we did not observe large changes in price indices for any of our goods around the time of hurricane threats or landfalls, so revenue and volume should in principle approximate scaled versions of one another. Third, all store-level results were robust to clustering at the store-level.

6. Discussion

Government information tells households to acquire adequate emergency supplies early in the hurricane season and especially when hurricanes begin to threaten. Curfews and government information urge people to stay off roads after hurricanes make landfall. Taken as a whole, we find behavior that is not consistent with this advice.

Our findings – based on the systematic evidence provided by actual sales of emergency goods observed at thousands of locations before and after dozens of hurricanes – are new to the literature. Observed early season stockpiling of emergency supplies is small. Sales of bottled water, batteries, and flashlights increase relative to counterfactual sales as storms threaten but magnitudes are modest and most increases occur immediately prior to landfall. After landfall, sales of bottled water, batteries, and flashlights increase considerably relative to counterfactual sales. We document less ex-ante preparation and higher post-landfall increases among less educated, lower income, and minority populations. Collectively, our results provide extensive observational evidence to support case-study-based survey responses suggesting that even the very highest risk households may systematically underprepare for hurricanes. Moreover, we show these findings apply more broadly to hundreds of thousands of households well beyond the most high-risk areas (i.e. beyond beachfront communities and coastal cities alone). Our heterogeneity results are consistent with a broader literature that suggests less educated, lower income, and minority populations may be least likely to receive, trust, and respond to information or may face more severe damages due to poor infrastructure or slower emergency responses.[27]

Even with novel data and 'clean' sources of empirical identification, we note several caveats. First, we are unable to exactly isolate timing of behavior changes. Our weekly sales data limit our ability to precisely identify some timing effects and our daily data are subject to at least some measurement error. Second, we only study sales of bottled water, batteries, and flashlights. Although millions of households are advised to acquire these items as hurricanes threaten, it is unclear whether results based on these goods should extend to more specialized disaster preparedness activities like boarding up windows in coastal properties or purchasing expensive emergency generators. Third, we analyze sales at large food stores only. Although the vast majority of tracked bottled water sales and roughly half of tracked battery sales occur in food stores, if hurricanes systematically shift shopping behavior away from large grocery stores towards smaller neighborhood stores or warehouse retailers then our results may be biased.[28] Fourth, hurricanes are rare and we are unable to fully examine how treatment effects vary with storm intensity. Future research might apply regression discontinuity tools that exploit category definitions. Fifth, we observe sales rather than total stocks and flows of emergency supplies. We empirically examine aspects of stockpiling, evacuations, and stock-outs, but we do not directly observe households'

[27] An alternative explanation for our heterogeneity results is that some demographic groups are more likely to be hit by storms. However, as shown in Appendix Table AX9, we find that demographics are statistically indistinguishable and similar in magnitude between locations that are struck and not struck. We are unable to definitively untangle or identify other possible mechanisms. African American, less educated, and lower income populations being less likely to receive or process risk information, less likely to trust publicly provided information, and/or suffering greater damages due to poor infrastructure or slower emergency responses is consistent with growing information and risk, trust, environmental justice, and natural disasters literatures. For broad discussions, see Ippolito & Mathios 1995; Shimshack et al. 2007; Lindell & Hwang 2008; Fothergill et al. 1999; Alesina & La Ferrara 2002. For case study evidence for the hurricane preparedness context, see Norris et al. 1999; Sattler et al. 2000; Lindell & Hwang 2008; Kim & Kang 2010; Baker 2011; Meyer et al. 2014.

[28] We do not observe mass marketer sales during the first period and we lack the data to credibly explain how sample composition for mass marketers changes during the second period. So, we cannot credibly explore whether threats and landfalls shifted sales between retailer channel types using our main store-level scanner data. However, the household-level data do allow us to characterize the universe of stores at which any given household transacts (transactions are reported by the household rather than the store and indicate the type of outlet where the purchase was made). We replicated our analysis using the share of spending at grocery stores, rather than total spending, as the outcome variable. We found evidence that hurricane threats modestly shifted expenditures towards grocery stores so our ex-ante 'threatened' treatment effect results may be slight overestimates. As in our main analysis, we are reluctant to over-interpret the results on the 'after landfall' indicator using the household data. Nevertheless, we found no consistent evidence that expenditures shifted significantly towards or away from grocery stores following landfall.

emergency supplies on hand at any given time.[29]

Our analysis is positive rather than normative. A related caveat is that our results are less informative for the question of whether observed behavior is individually utility maximizing. Our findings suggest that supply chains manage increased demand following hurricanes so it may be individually rational to disregard government advice and delay purchases of emergency supplies until strictly necessary. That said, behavioral economics suggests that cognitive bias may be expected when households face hurricane threats and landfalls. The economic psychology literature suggests that behaviors around rare events, like hurricanes, are particularly prone to bounded rationality, misperceived risks, and heuristic decision making (Posner 2004; Schwarz et al. 2007; Sunstein 2007; Pindyck & Wang 2013). Scary personal risks, like hurricane threats, spur optimism bias, complacency, and 'ostrich effects' where individuals intentionally avoid contemplating unpleasant dangers (Galai & Sade 2006, Karlsson et al. 2009, Oster et al. 2013, Loewenstein et al. 2013, Sharot 2011, Shepperd et al. 2013, Trumbo et al. 2013). Indeed, consistent with the predictions of behavioral economics, survey evidence from coastal residents facing threatening hurricanes finds that respondents are aware of approaching storms but report limited worry and underestimate the duration of impacts, the duration of public service outages, and severity of flood damages (Meyer et al. 2014). And walking or driving to acquire bottled water, flashlights, and batteries after hurricane landfall poses individual risks; as noted, more injuries and deaths stem from indirect causes after hurricane landfall than from the hurricanes themselves (Rappaport & Blanchard 2016).

The above caveats notwithstanding, our results have implications for economics and policy. First, preparedness appears to pay off. Locations that acquire more emergency supplies prior to landfall (i.e. areas with fewer African Americans, higher incomes, and more education) tend to experience smaller increases in sales of those goods after landfall. In this sense, the basic premise of government information campaigns designed to spur preparation appears sound. Second, and on the other hand, the distributional consequences of government information campaigns for hurricane preparedness and response are pronounced. Lower income, less educated, and minority populations experience smaller increases in sales of emergency goods as hurricanes

[29] As a consequence, sales increases after landfall may be underestimates if emergency assistance agencies distribute water, batteries, and flashlights to local populations. Sales increases after landfall may be underestimates of average behavior changes if tourists in hurricane prone areas are no longer present after landfall. Etc.

approach and experience larger increases in sales of emergency response goods after landfall. Potentially vulnerable groups may be less prepared for disasters, suffer greater damages from disasters, or both. Third, government information campaigns for hurricane preparedness and response are far from fully mitigating the externalities that provide much of their economic justification. Individuals traveling after landfalls may jam debris-strewn roads, hinder rescue and recovery operations, endanger first-responders, and possibly tie up emergency resources.

In light of these cautionary conclusions, more targeted messaging campaigns may be necessary to completely reach at-risk populations not well served by current communication strategies. Results like ours, or perhaps real-time monitoring of sales, provide a means to identify specific neighborhoods where enhanced outreach would be beneficial. More costly alternatives to government messaging might involve direct public provision of emergency preparedness goods or public/private partnerships to increase sales of emergency goods before storms arrive (Meyer et al. 2014).

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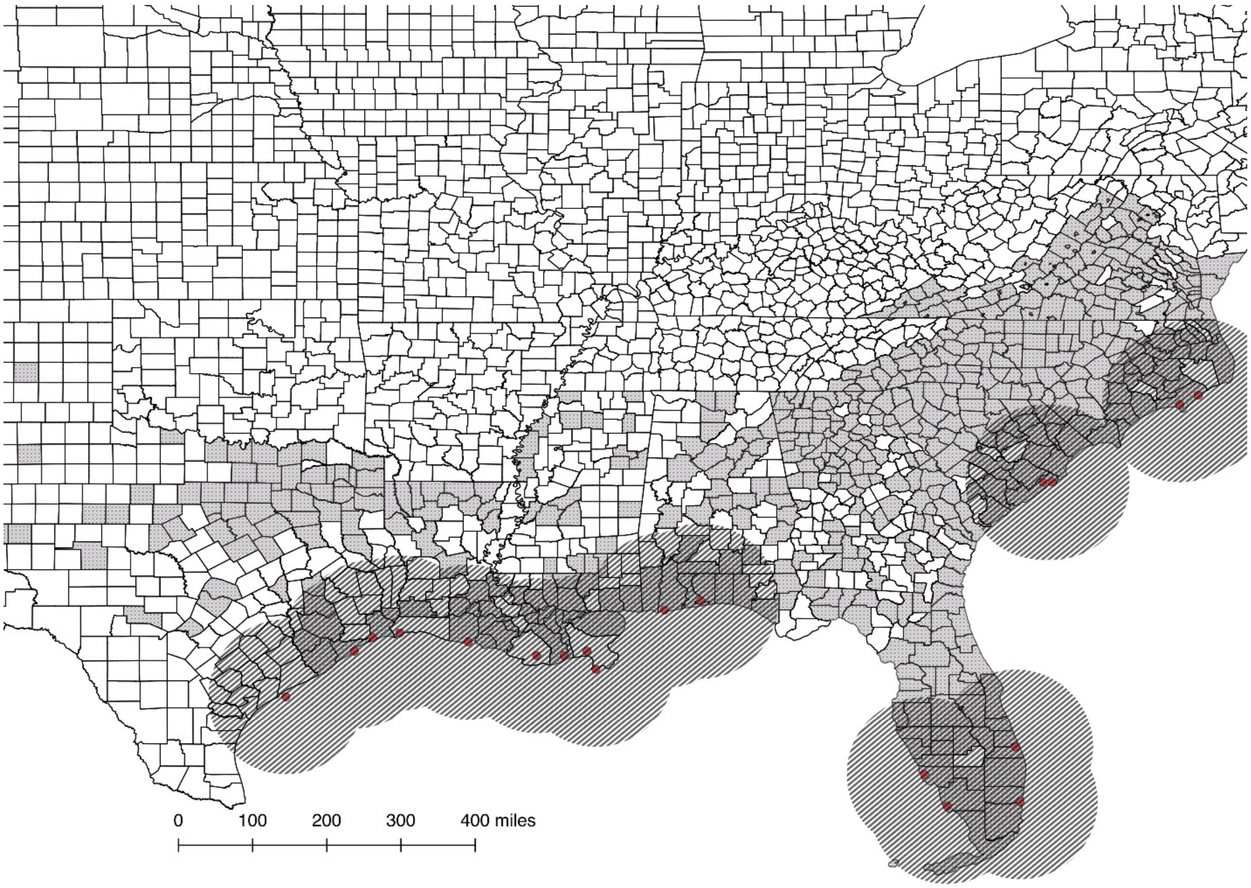

Figure 1. Store locations and hurricane landfalls. Shaded counties include one or more stores. Stores are roughly evenly distributed throughout VA, NC, and SC. Stores are otherwise concentrated around the FL and Gulf coasts, Atlanta, and Dallas. Counties in northern LA, MS, and AL do not contain many stores. Small (red) dots indicate hurricane landfall locations. Surrounding circles represent 100-mile radius strike areas. During the 2002 through 2012 hurricane seasons, hurricanes made landfall at many points along the southeastern and gulf coasts.

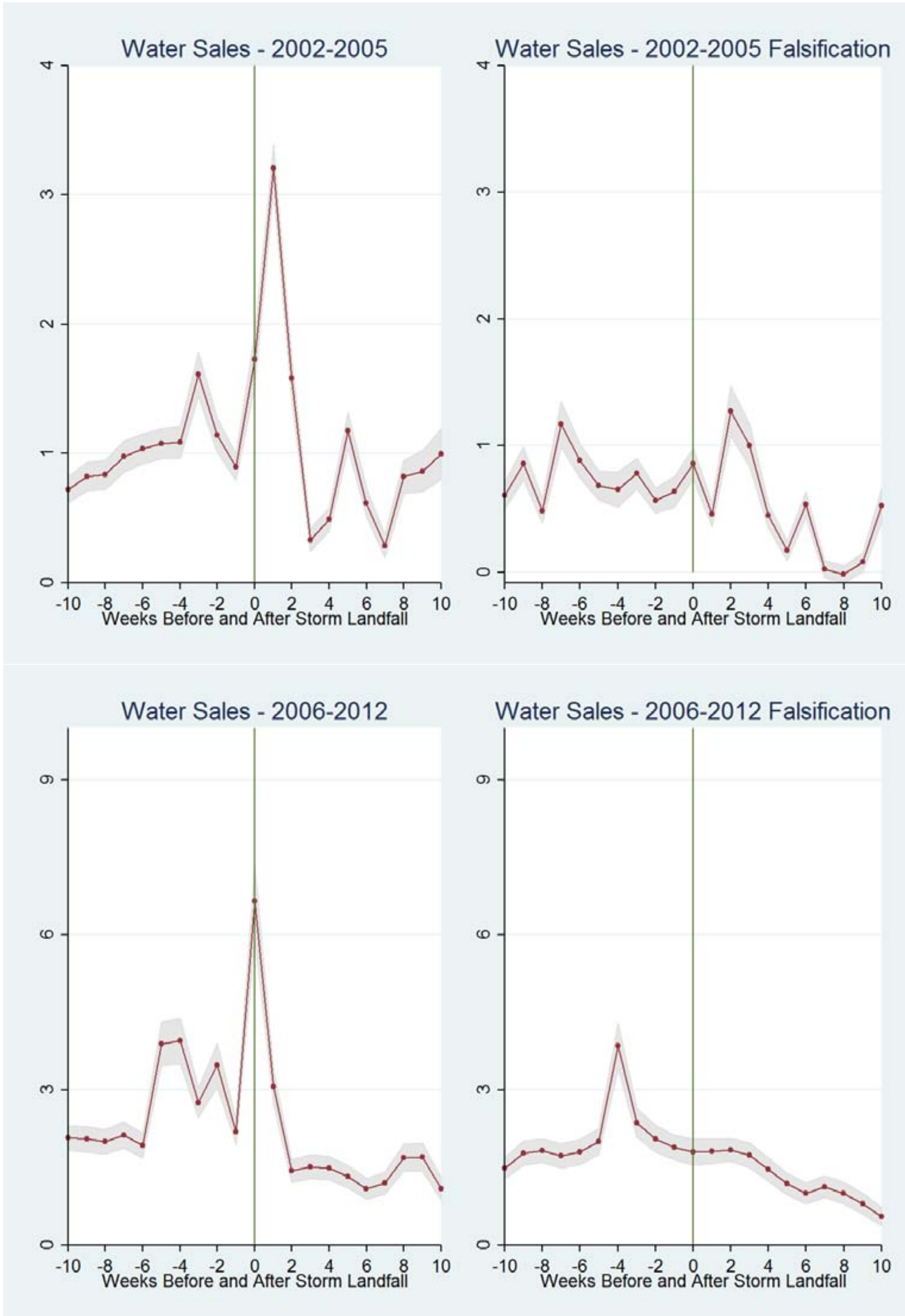

Figure 2, Panels A and B. Store-week bottled water sales before and after hurricanes. The event study graphs depict bottled water sales before and after hurricane landfall for all stores geo-located within 100 miles of the landfall point. Left and right panels are based upon actual and placebo hurricane landfall timing. Y-axes measure average weekly sales in thousands of dollars, relative to overall sample means. Average sales in the figures may not be zero due to seasonality. Key results are sharp changes in sales of bottled around actual hurricane landfalls, and limited changes in sales around placebo hurricane landfalls created by having storms strike the same location one year earlier.

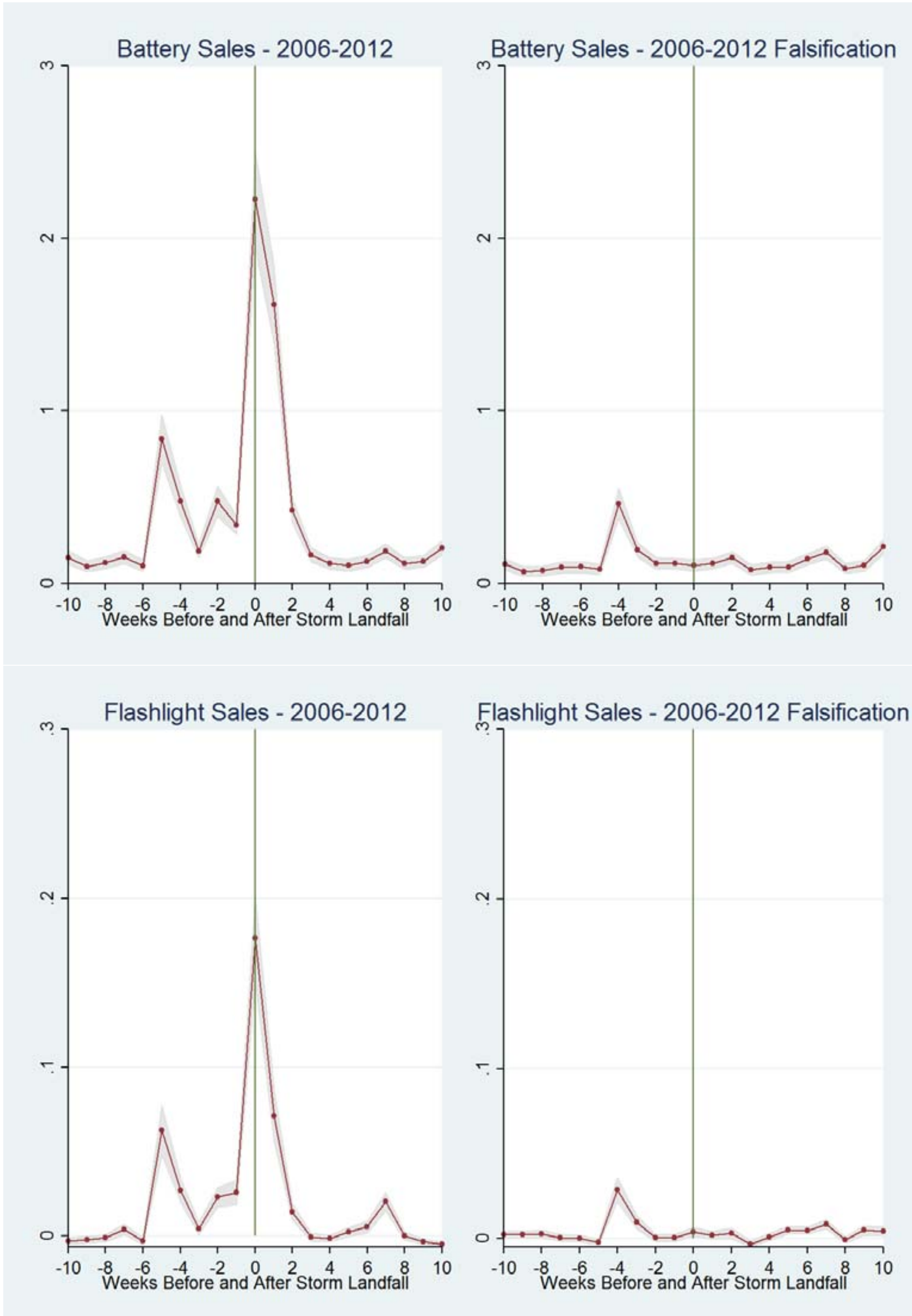

Figure 2, Panels C and D. Store-week battery and flashlight sales before and after hurricanes. The event study graphs depict battery and flashlight sales before and after hurricane landfall for all stores geo-located within 100 miles of the landfall point. Left and right panels are based upon actual and placebo hurricane landfall timing. Y-axes measure average weekly sales in thousands of dollars, relative to overall sample means. Average sales in the figures may not be zero due to seasonality. Key results are sharp changes in sales of batteries and flashlights around actual hurricane landfalls, and limited changes in sales around placebo hurricane landfalls created by having storms strike the same location one year earlier.

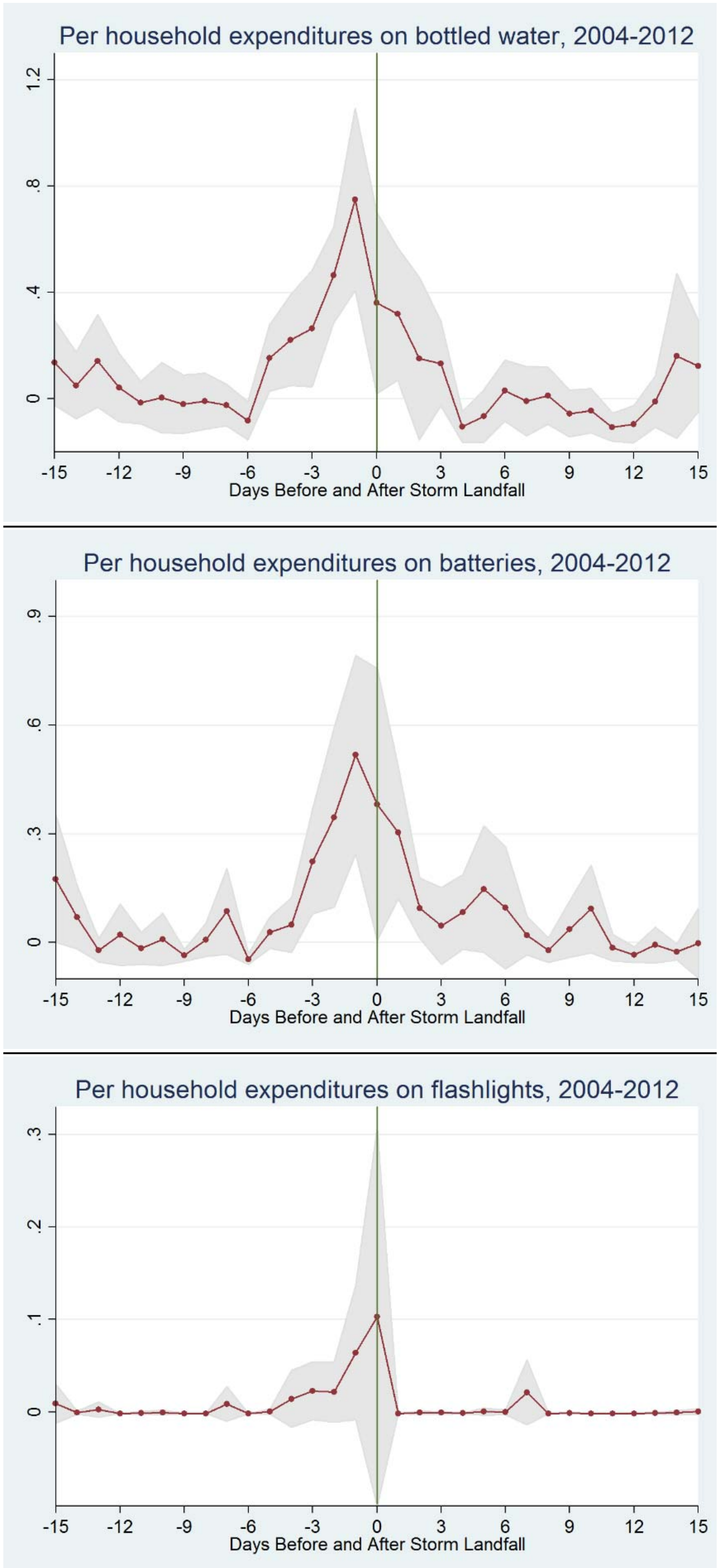

Fig. 3. Household-day purchases before and after hurricanes. The event study graphs depict recorded per-household purchases of bottled water, batteries, and flashlights (conditional on any recorded shopping on a given day) in the days before and after hurricane landfalls for all households geo-located within 100 miles of the landfall point. Y-axes measure average household sales, relative to overall sample means. Average sales may not be zero due to seasonality. Flashlights are rarely purchased during non-storm periods, so data are noisy. Key results are sharp increases in sales of emergency supplies beginning roughly 5 days before landfall, with peak increases occurring roughly 1 day before landfall.

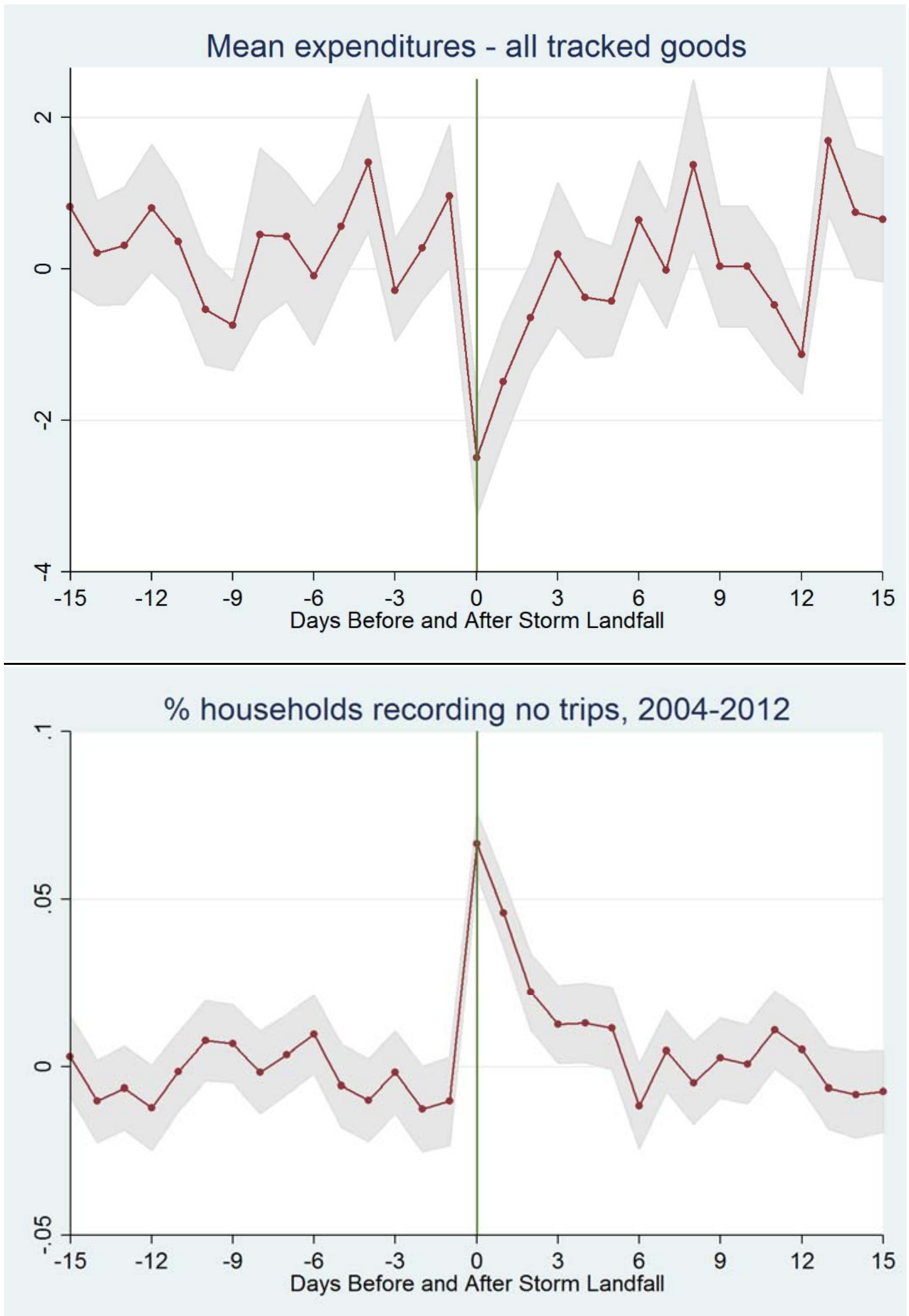

Figure 4. Household-day recorded purchases of all goods before and after hurricanes. The event study graphs depict per-household purchases of all goods, and the percent of households not recording a shopping trip, in the days before and after hurricane landfalls for all households geo-located within 100 miles of the landfall point. Y-axes measure average household sales, relative to overall sample means. Average sales may not be zero due to seasonality. Key results are sharp declines in recorded purchases of any goods, and sharp increases in households recording no purchases of any goods, beginning the day of landfall and lasting for several days after landfall.

Table 1. Summary statistics

Sample Composition	2002 to 2005 3311 stores in 517 counties		2006 to 2012 2783 stores in 504 counties	
Unit of Observation	Store-week		County-week	
	MEAN	ST. DEV.	MEAN	ST. DEV.
Weekly revenues				
Bottled Water Sales	1,931	1,472	16,241	41,665
Battery Sales	n/a	n/a	2,366	6,288
Flashlight Sales	n/a	n/a	93.8	430.3
Location characteristics				
Hh income (median)	45,381	16,071	44,821	12,456
% black or Afr Amer.	18.3	17.5	21.8	16.4
College or higher	25.5	14.8	20.1	09.2
Weather				
Precipitation	36.6	46.6	32.4	44.8
Mean Temp	65.3	14.4	63.5	14.7
Min Temp	48.8	17.2	45.3	17.1
Max Temp	82.1	11.6	81.9	12.4

NOTES: Statistics summarize raw location-by-week data, over all observed weeks in the year. We have two samples, one focused on bottled water sales observed the store-week level from 2002 to 2005 and one focused on bottled water, battery, and flashlight sales observed at the county-week level from 2006 to 2012.

Table 2. The effect of hurricane threats and hurricane strikes on sales of emergency supplies

	(1)	(2)	(3)	(4)
Sample	2002-2005	2006-2012		
Unit of observation	Store-week	County-week		
Dependent Variables (in logs)	BOTTLED WATER SALES	BOTTLED WATER SALES	BATTERY SALES	FLASHLIGHT SALES
THREATENED	0.054***	0.154***	0.407***	0.878***
	(0.012)	(0.011)	(0.023)	(0.044)
AFTER landfall	0.490***	0.287***	1.146***	1.162***
	(0.090)	(0.090)	(0.164)	(0.246)
Observations	313,655	92,232	92,232	92,232
# of groups (stores or counties)	3,311	504	504	504

NOTES: Table 2 presents results corresponding to regression equation {1}. Standard errors, clustered at the county level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.10. Regressions include group fixed effects (stores or counties), year fixed effects, month fixed effects, precipitation, max temperature, mean temperature, min temperature, and an indicator for location-week ‘contains landfall.’

Table 3. Heterogeneity in treatment effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Var. (Logs)	**2002-05 BOTTLED WATER SALES**				**2006-12 BOTTLED WATER SALES**			
THREATENED	0.053***	0.055***	0.070***	0.078***	0.121***	0.185***	0.111***	0.100***
	(0.015)	(0.014)	(0.013)	(0.013)	(0.014)	(0.015)	(0.017)	(0.017)
× Coastal	0.001				0.100***			
	(0.024)				(0.021)			
× Afr. Amer		-0.003				-0.054**		
		(0.012)				(0.022)		
× Income			-0.036**				0.079***	
			(0.097)				(0.022)	
× College				-0.054***				0.100***
				(0.012)				(0.022)
AFTER	0.785***	0.352***	0.547***	0.489***	0.361**	-0.016	0.701***	0.497***
	(0.123)	(0.068)	(0.103)	(0.090)	(0.186)	(0.113)	(0.099)	(0.160)
× Coastal	-0.359**				-0.129			
	(0.157)				(0.206)			
× Afr. Amer		0.340***				0.503***		
		(0.095)				(0.150)		
× Income			-0.142**				-0.650***	
			(0.073)				(0.130)	
× College				0.002				-0.377**
				(0.064)				(0.184)
Dependent Var. (Logs)	**2006-12 BATTERY SALES**				**2006-12 FLASHLIGHT SALES**			
THREATENED	0.313***	0.425***	0.305***	0.294***	0.735***	0.924***	0.711***	0.646***
	(0.029)	(0.032)	(0.034)	(0.036)	(0.056)	(0.064)	(0.069)	(0.073)
× Coastal	0.278***				0.425***			
	(0.047)				(0.090)			
× Afr. Amer		-0.032				-0.083		
		(0.048)				(0.091)		
× Income			0.188***				0.308***	
			(0.047)				(0.091)	
× College				0.211***				0.432***
				(0.048)				(0.091)
AFTER	1.103***	0.671***	1.909***	1.486***	1.378***	0.410	1.888***	1.332***
	(0.309)	(0.200)	(0.101)	(0.260)	(0.440)	(0.297)	(0.318)	(0.386)
× Coastal	0.049				-0.376			
	(0.358)				(0.529)			
× Afr. Amer		0.872***				1.217***		
		(0.275)				(0.432)		
× Income			-1.178***				-1.162***	
			(0.197)				(0.418)	
× College				-0.598*				-0.306
				(0.326)				(0.503)

NOTES: Table 3 presents results corresponding to equation {2}. Standard errors, clustered at the county level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.10. Regressions include group fixed effects (stores or counties), year fixed effects, month fixed effects, precipitation, max temperature, mean temperature, and min temperature. All regressions also include indicators for location-week ‘contains landfall’, not interacted and interacted following the same conventions as the ‘threatened’ and ‘after landfall’ treatments.

Table 4. The effect of specific hurricane threats on household-level purchases of emergency supplies

	(1)	(2)	(3)	(4)	(5)	(6)
Sample		2004-2012			2004-2012	
Unit of observation		Household-day			Household-day	
Outcome		Conditional on recorded shopping for any good			Unconditional	
Dependent Variables (in logs)	BOTTLED WATER EXPENDITURE	BATTERY EXPENDITURE	FLASHLIGHT EXPENDITURE	BOTTLED WATER EXPENDITURE	BATTERY EXPENDITURE	FLASHLIGHT EXPENDITURE
County-day THREATENED	0.0674***	0.0193***	0.0027*	0.0112***	0.0031***	0.0004*
By landfall in next 96 hours	(0.0097)	(0.0051)	(0.0014)	(0.0018)	(0.0008)	(0.0002)
County-day THREATENED	0.0688***	0.0416***	0.0053***	0.0134***	0.0075***	0.0009***
By landfall in next 72 hours	(0.0092)	(0.0082)	(0.0020)	(0.0018)	(0.0014)	(0.0003)
County-day THREATENED	0.1241***	0.0518***	0.0021	0.0224***	0.0087***	0.0004
By landfall in next 48 hours	(0.0138)	(0.0083)	(0.0033)	(0.0031)	(0.0015)	(0.0005)
County-day THREATENED	0.1454***	0.0663***	0.0252**	0.0112**	0.0060**	0.0026*
By landfall in next 24 hours	(0.0427)	(0.0228)	(0.0119)	(0.0049)	(0.0026)	(0.0014)
Observations	3,495,264	3,495,264	3,495,264	24,255,351	24,255,351	24,255,351
Number of households	37,317	37,317	37,317	37,317	37,317	37,317
Number of counties	923	923	923	923	923	923

Specifications mimic Table 2, except that the analysis is at the household-level and the threatened treatments are disaggregated to the specific forecast. Standard errors, clustered at the county level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.10. Regressions include household fixed effects, year fixed effects, month fixed effects, precipitation, max temperature, mean temperature, min temperature, an ‘after landfall’ treatment indicator, and an indicator for household-day ‘experiencing landfall.’

Table 5. Direct hits and near misses:
Treatments defined by 'threatened and struck' vs. 'threatened and not struck'

Sample	2002-2005		2006-2012	
Dependent Variable (in logs)	BOTTLED WATER SALES		BOTTLED WATER SALES	
	Threatened and not struck	Threatened and struck	Threatened and not struck	Threatened and struck
Store-week THREATENED	0.051***	0.109*	0.093***	0.426***
	(0.015)	(0.066)	(0.012)	(0.065)
Store-week AFTER actual or simulated landfall	0.403***	0.494***	0.079***	0.299***
	(0.043)	(0.088)	(0.012)	(0.090)

Sample	2006-2012		2006-2012	
Dependent Variable (in logs)	BATTERY SALES		FLASHLIGHT SALES	
	Threatened and not struck	Threatened and struck	Threatened and not struck	Threatened and struck
Store-week THREATENED	0.250***	1.182***	0.512***	2.229***
	(0.023)	(0.132)	(0.044)	(0.311)
Store-week AFTER actual or simulated landfall	0.234***	1.181***	0.0385***	1.229***
	(0.023)	(0.162)	(0.047)	(0.249)

NOTES: Other than treatment definition, Table 5 mimics Table 2. We define 'near miss' / 'threatened and not struck' locations as those that are threatened but not struck by a hurricane making landfall within 100 miles. 'Threatened and struck' locations are those that are threatened and subsequently experienced a landfall within 100 miles. Standard errors, clustered at the county level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.10. Regressions include group fixed effects (stores or counties), year fixed effects, month fixed effects, precipitation, max temperature, mean temperature, min temperature, and an indicator for location-week 'contains landfall.'

Appendix A1. FEMA Emergency Kit Information

Recommended Items to Include in a Basic Emergency Supply Kit:

- ❑ Water, one gallon of water per person per day for at least three days, for drinking and sanitation
- ❑ Food, at least a three-day supply of non-perishable food
- ❑ Battery-powered or hand crank radio and a NOAA Weather Radio with tone alert and extra batteries for both
- ❑ Flashlight and extra batteries
- ❑ First aid kit
- ❑ Whistle to signal for help
- ❑ Dust mask, to help filter contaminated air and plastic sheeting and duct tape to shelter-in-place
- ❑ Moist towelettes, garbage bags and plastic ties for personal sanitation
- ❑ Wrench or pliers to turn off utilities
- ❑ Can opener for food (if kit contains canned food)
- ❑ Local maps

Through its *Ready* Campaign, the Federal Emergency Management Agency educates and empowers Americans to take some simple steps to prepare for and respond to potential emergencies, including natural disasters and terrorist attacks. *Ready* asks individuals to do three key things: get an emergency supply kit, make a family emergency plan, and be informed about the different types of emergencies that could occur and their appropriate responses.

All Americans should have some basic supplies on hand in order to survive for at least three days if an emergency occurs. Following is a listing of some basic items that every emergency supply kit should include. However, it is important that individuals review this list and consider where they live and the unique needs of their family in order to create an emergency supply kit that will meet these needs. Individuals should also consider having at least two emergency supply kits, one full kit at home and smaller portable kits in their workplace, vehicle or other places they spend time.

Ready FEMA

Federal Emergency Management Agency
Washington, DC 20472

Appendix A1. FEMA Bottled Water Information

Public information campaigns typically advise households to have one gallon of potable water per person per day on hand, for at least three days to one week.[1] FEMA's Ready.gov site recommends, as storms approach, that households "purchase commercially bottled water, in order to prepare the safest and most reliable emergency water supply. Keep bottled water in its original container and do not open until you need to use it." FEMA and state agencies advise that self-stored water is acceptable for cleaning and flushing toilets, but not for consumption or preparing food. The Food and Drug Administration, the Centers for Disease Control, and nearly all state and local agencies recommend that households avoid drinking or preparing food with tap or well water until the household is informed by public authorities that these sources are safe.

WATER

Water is an essential element to survival and a necessary item in an emergency supplies kit. Following a disaster, clean drinking water may not be available. Your regular water source could be cut-off or compromised through contamination. Prepare yourself by building a supply of water that will meet your family's needs during an emergency.

HOW MUCH WATER DO I NEED?

You should store at least one gallon of water per person per day. A normally active person needs at least one gallon of water daily just for drinking however individual needs vary, depending on age, physical condition, activity, diet and climate.

Recommended Supplies List (PDF)

To determine your water needs, take the following into account:

- One gallon of water per person per day, for drinking and sanitation.
- Children, nursing mothers and sick people may need more water.
- A medical emergency might require additional water.
- If you live in a warm weather climate more water may be necessary. In very hot temperatures, water needs can double.
- Keep at least a three-day supply of water per person.

HOW SHOULD I STORE WATER?

It is recommended you purchase commercially bottled water, in order to prepare the safest and most reliable emergency water supply. Keep bottled water in its original container and do not open until you need to use it. Observe the expiration or "use by" date. Store in cool, dark place.

[1] See, for example, the FEMA's Ready.gov "Emergency Supply List", the Louisiana Governor's Office of Homeland Security and Emergency Preparedness "Official Louisiana Hurricane Survival Guide," or NOAA's "Tropical Cyclones: A Preparedness Guide."

Appendix A2. Hurricane landfalls in gulf coast and southeastern states, 2002-2012

HURDAT STORM ID	STORM NAME	LANDFALL DATE	LANDFALL POINT	HIGHEST SUSTAINED WIND	HIGHEST CATEGORY
200210	ISIDORE	26-Sep-02	LA	110	CAT3
200212	KYLE	11-Oct-02	SC	75	CAT1
200213	LILI	3-Oct-02	LA	125	CAT4
200304	CLAUDETTE	15-Jul-03	TX	80	CAT1
200313	ISABEL	18-Sep-03	NC	145	CAT5
200403	CHARLEY	13-Aug-04	FL	130	CAT4
200403	CHARLEY	14-Aug-04	SC	130	CAT4
200406	FRANCES	6-Sep-04	FL	125	CAT4
200407	GASTON	29-Aug-04	SC	65	CAT1
200409	IVAN	16-Sep-04	AL	145	CAT5
200409	IVAN	24-Sep-04	LA	145	CAT5
200411	JEANNE	26-Sep-04	FL	105	CAT3
200503	CINDY	6-Jul-05	LA	65	CAT1
200504	DENNIS	10-Jul-05	FL	130	CAT4
200512	KATRINA	25-Aug-05	FL	150	CAT5
200512	KATRINA	29-Aug-05	LA	150	CAT5
200518	RITA	24-Sep-05	LA	155	CAT5
200525	WILMA	24-Oct-05	FL	160	CAT5
200606	ERNESTO	30-Aug-06	FL	65	CAT1
200606	ERNESTO	01-Sep-06	NC	65	CAT1
200709	HUMBERTO	13-Sep-07	TX	80	CAT1
200807	GUSTAV	01-Sep-08	LA	135	CAT4
200808	HANNA	06-Sep-08	SC	75	CAT1
200809	IKE	13-Sep-08	TX	125	CAT4
201109	IRENE	27-Aug-11	NC	105	CAT3
201209	ISAAC	29-Aug-12	LA	70	CAT1

Appendix Table AX1: Summary statistics for positive treatment observations

Period 1 (2002-2005) hurricane seasons

Row	Threatened	Struck	After Landfall	Obs	Share of treated	Median Income	Percent Black	Percent College
[1]	1	0	0	11417	0.88	42676	17.52	23.36
						(13646)	(17.60)	(12.66)
[2]	0	0	1	476	0.04	41238	17.43	22.93
						(11423)	(18.56)	(11.08)
[3]	1	1	0	155	0.01	39998	8.71	20.50
						(9893)	(12.39)	(8.76)
[4]	0	1	1	136	0.01	42033	19.24	20.64
						(15761)	(21.11)	(13.25)
[5]	1	1	1	748	0.06	41153	17.69	22.57
						(11349)	(18.40)	(10.77)
[6]	0	1	0	0	0.00	n/a	n/a	n/a
						n/a	n/a	n/a

Period 2 (2006-2012) hurricane seasons

Row	**Threatened**	**Struck**	**After Landfall**	**Obs**	**Share of treated**	**Median Income**	**Percent Black**	**Percent College**
[1]	1	0	0	7778	0.90	50877	21.17	26.83
						(13295)	(12.65)	(10.24)
[2]	0	0	1	404	0.05	52861	19.40	27.33
						(9678)	(7.58)	(5.58)
[3]	1	1	0	238	0.03	51227	19.67	26.79
						(9956)	(8.40)	(6.43)
[4]	0	1	1	25	0.01	73017	13.25	35.26
						(7253)	(8.87)	(5.46)
[5]	1	1	1	185	0.02	51298	22.04	26.25
						(5329)	(9.28)	(3.99)
[6]	0	1	0	0	0.00	n/a	n/a	n/a
						n/a	n/a	n/a

TABLE NOTES: “Threatened,” “struck,” and “after landfall” treatments are separately identified using combinations of treatment types and thus identification does not necessarily require many observations in all cells of the requested table. Location-weeks will not typically be “struck” without being “threatened” or “after landfall” treated, so rows [6] should contain zero observations. Landfalls occur on a single day, so a location-week could only be “struck” but not “threatened” or “after landfall” treated if the storm made landfall on the last day of the location-week and forecasts were extraordinarily inaccurate. Location-weeks that are “struck” and “after landfall” treated without being “threatened’ might be expected to be relatively rare. This treatment scenario requires hurricane landfall on the first day of the location-week (or, again, extraordinarily inaccurate forecasts). Rows [4] confirm this intuition Location weeks containing threatened days but no “landfall” or “after landfall” treatments should be very common relative to other treatments. Forecast cones of uncertainty imply many areas are threatened but not ultimately struck. Indeed, this is what we see in rows [1].

Appendix Table AX2. Stockpiling – First Week of Hurricane Season

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SAMPLE	2002-2005					2006-2012				
Dep. Var. (in logs)	**BOTTLED WATER SALES**					**BOTTLED WATER SALES**				
FIRST	0.016***	0.031***	0.002	0.010***	0.015***	-0.019***	-0.001	-0.036***	-0.021***	-0.028***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.005)
FIRST × Afr. Amer.		-0.030***					-0.035***			
		(0.005)					(0.008)			
FIRST × Income			0.029***					0.035***		
			(0.005)					(0.007)		
FIRST × Educated				0.012**					0.005	
				(0.005)					(0.008)	
FIRST × Coastal					0.005					0.053***
					(0.010)					(0.010)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SAMPLE	2006-2012					2006-2012				
Dep. Var. (in logs)	**BATTERY SALES**					**FLASHLIGHT SALES**				
FIRST	0.111***	0.128***	0.082***	0.098***	0.086***	0.139***	0.184***	0.101***	0.138***	0.097***
	(0.006)	(0.008)	(0.008)	(0.009)	(0.006)	(0.019)	(0.024)	(0.028)	(0.027)	(0.020)
FIRST × Afr. Amer.		-0.034***					-0.091***			
		(0.012)					(0.035)			
FIRST × Income			0.056***					0.075**		
			(0.012)					(0.035)		
FIRST × Educated				0.026**					0.002	
				(0.012)					(0.035)	
FIRST × Coastal					0.132***					0.224***
					(0.016)					(0.044)

Appendix Table AX3. Stockpiling – Week of First Hurricane this Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SAMPLE	2002-2005					2006-2012				
Dep. Var. (in logs)	**BOTTLED WATER SALES**					**BOTTLED WATER SALES**				
FIRST	-0.023***	-0.016***	-0.007	-0.011***	-0.031***	-0.011***	-0.005	-0.030***	-0.035***	-0.018***
	(0.005)	(0.006)	(0.004)	(0.007)	(0.006)	(0.004)	(0.006)	(0.005)	(0.005)	(0.004)
FIRST × Afr. Amer.		-0.015**					-0.013			
		(0.006)					(0.008)			
FIRST × Income			-0.032***					0.041***		
			(0.005)					(0.008)		
FIRST × Educated				-0.025***					0.051***	
				(0.007)					(0.008)	
FIRST × Coastal					0.045***					0.056***
					(0.012)					(0.012)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SAMPLE	2006-2012					2006-2012				
Dep. Var. (in logs)	**BATTERY SALES**					**FLASHLIGHT SALES**				
FIRST	0.006	-0.013	-0.009	-0.015	-0.002	0.028	0.014	-0.000	-0.031	-0.003
	(0.008)	(0.009)	(0.012)	(0.012)	(0.008)	(0.029)	(0.036)	(0.044)	(0.043)	(0.030)
FIRST × Afr. Amer.		0.039**					0.029			
		(0.017)					(0.060)			
FIRST × Income			0.031*					0.059		
			(0.016)					(0.059)		
FIRST × Educated				0.044***					0.122**	
				(0.016)					(0.059)	
FIRST × Coastal					0.060**					0.229**
					(0.027)					(0.093)

Appendix Table AX4. Response by Evacuation Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	2002-2005		2006-2012		2006-2012		2006-2012	
Dependent Variable (in logs)	Bottled Water Sales		Bottled Water Sales		Battery Sales		Flashlight Sales	
	HURRICANES WITHOUT LARGE-SCALE EVACUATIONS							
THREATENED	-0.005	0.046	0.096***	0.068***	0.301***	0.237***	0.735***	0.835***
	(0.021)	(0.021)	(0.012)	(0.015)	(0.024)	(0.028)	(0.056)	(0.044)
THREATENED × Coastal County		-0.088**		0.067***		0.156***		0.425***
		(0.032)		(0.025)		(0.052)		(0.090)
AFTER Storm Landfall	0.195***	0.070	-0.011	-0.005	0.422	-0.070	1.374***	1.496***
	(0.042)	(0.044)	(0.029)	(0.024)	(0.268)	(0.092)	(0.440)	(0.448)
AFTER × Coastal County		0.156**		-0.010		0.772**		-0.376
		(0.062)		(0.050)		(0.390)		(0.529)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	2002-2005		2006-2012		2006-2012		2006-2012	
Dependent Variable (in logs)	Bottled Water Sales		Bottled Water Sales		Battery Sales		Flashlight Sales	
	HURRICANES WITH LARGE-SCALE EVACUATIONS							
THREATENED	0.071***	0.050**	0.204***	0.156***	0.485***	0.351***	1.019***	0.797***
	(0.015)	(0.025)	(0.014)	(0.017)	(0.031)	(0.036)	(0.065)	(0.077)
THREATENED × Coastal County		0.037		0.175***		0.489***		0.815***
		(0.031)		(0.027)		(0.063)		(0.133)
AFTER Storm Landfall	0.556***	0.908***	0.468***	0.619***	1.595***	1.759***	1.758***	2.320***
	(0.102)	(0.119)	(0.086)	(0.166)	(0.096)	(0.179)	(0.233)	(0.261)
AFTER × Coastal County		-0.430***		-0.268		-0.322		-1.013**
		(0.163)		(0.186)		(0.208)		(0.413)

Appendix Table AX5. Product Mix Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	2002-2005				2006-2012			
Dependent Variable (in logs)	**BOTTLED WATER SALES BY SIZE**							
	All	6 x 16.9oz	12 x 16.9oz	1 x 128oz	All	6 x 16.9oz	12 x 16.9oz	1 x 128oz
Store-week THREATENED	0.054***	0.020	-0.003	0.087***	0.154***	0.052***	0.074***	0.224***
	(0.012)	(0.034)	(0.016)	(0.014)	(0.011)	(0.011)	(0.013)	(0.013)
Store-week CONTAINS landfall	-0.356***	-0.226***	-0.367***	-0.485***	0.294***	0.203***	0.298***	0.185***
	(0.085)	(0.066)	(0.082)	(0.106)	(0.052)	(0.045)	(0.051)	(0.062)
Store-week AFTER landfall	0.490***	0.331***	0.585***	0.643***	0.287***	0.015	0.124	0.311***
	(0.090)	(0.068)	(0.080)	(0.117)	(0.090)	(0.055)	(0.078)	(0.100)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	2006-2012				2006-2012			
Dependent Variable (in logs)	**BATTERY SALES BY SIZE**				**FLASHLIGHT SALES BY SIZE**			
	All	2 pack	4 pack	8 pack	All	n/a	n/a	n/a
Store-week THREATENED	0.407***	0.307***	0.519***	0.360***	0.878***	-	-	-
	(0.023)	(0.024)	(0.027)	(0.023)	(0.007)			
Store-week CONTAINS landfall	0.280***	0.403***	0.311***	0.105	0.235***	-	-	-
	(0.085)	(0.072)	(0.100)	(0.131)	(0.006)			
Store-week AFTER landfall	1.146***	1.033***	1.403***	1.051***	1.162***	-	-	-
	(0.164)	(0.156)	(0.194)	(0.165)	(0.011)			

NOTES: virtually all flashlights are sold as a single unit.

Appendix Table AX6. Effects of Hurricanes on Sales of Common Beverages

DEPENDENT VARIABLES (in logs)	(1) BOTTLED WATER SALES	(2) SODA SALES	(3) DIET SODA SALES	(4) APPLE JUICE SALES	(5) ORANGE JUICE SALES
Store-week BEFORE storm landfall	0.054***	0.029***	0.013**	0.027***	-0.003
	(0.012)	(0.006)	(0.005)	(0.005)	(0.006)
Store-week CONTAINS storm landfall	-0.356***	-0.093***	-0.102***	-0.111***	-0.022
	(0.085)	(0.018)	(0.024)	(0.004)	(0.013)
Store-week AFTER storm landfall	0.490***	0.087***	0.091***	0.075***	-0.009
	(0.090)	(0.018)	(0.017)	(0.004)	(0.012)
Observations	313,655	313,664	313,663	315,035	315,037
Number of stores	3,311	3,311	3,311	3,311	3,311

NOTES: Mean revenues by category: $2085 for water, $6381 for soda, $3045 for diet soda, $393 for apple juice, and $1703 for orange juice.

Appendix Table AX7: Sensitivity to 'struck' radii

Effects of Hurricanes on Sales: After landfall defined by 50m radius

SAMPLE	2002-2005	2006-2012		
DEPENDENT VARIABLES (in logs)	BOTTLED WATER SALES	BOTTLED WATER SALES	BATTERY SALES	FLASHLIGHT SALES
THREATENED	0.059***	0.167***	0.434***	0.908***
	(0.011)	(0.011)	(0.025)	(0.046)
AFTER landfall	0.561***	0.063	0.702***	0.482*
	(0.160)	(0.130)	(0.240)	(0.283)

Effects of Hurricanes on Sales: After landfall defined by 75m radius

SAMPLE	2002-2005	2006-2012		
DEPENDENT VARIABLES (in logs)	BOTTLED WATER SALES	BOTTLED WATER SALES	BATTERY SALES	FLASHLIGHT SALES
THREATENED	0.069***	0.111***	0.423***	0.891***
	(0.011)	(0.011)	(0.024)	(0.045)
AFTER landfall	0.520***	0.161	0.875***	0.812***
	(0.120)	(0.099)	(0.189)	(0.305)

Effects of Hurricanes on Sales: After landfall defined by 100m radius

SAMPLE	2002-2005	2006-2012		
DEPENDENT VARIABLES (in logs)	BOTTLED WATER SALES	BOTTLED WATER SALES	BATTERY SALES	FLASHLIGHT SALES
THREATENED	0.054***	0.154***	0.407***	0.878***
	(0.012)	(0.011)	(0.023)	(0.007)
AFTER landfall	0.490***	0.287***	1.146***	1.162***
	(0.090)	(0.090)	(0.164)	(0.011)

Effects of Hurricanes on Sales: After landfall defined by 125m radius

SAMPLE	2002-2005	2006-2012		
DEPENDENT VARIABLES (in logs)	BOTTLED WATER SALES	BOTTLED WATER SALES	BATTERY SALES	FLASHLIGHT SALES
THREATENED	0.051***	0.143***	0.385***	0.850***
	(0.014)	(0.011)	(0.022)	(0.042)
AFTER landfall	0.429***	0.324***	1.206***	1.207***
	(0.067)	(0.071)	(0.131)	(0.210)

Effects of Hurricanes on Sales: After landfall defined by 150m radius

SAMPLE	2002-2005	2006-2012		
DEPENDENT VARIABLES (in logs)	BOTTLED WATER SALES	BOTTLED WATER SALES	BATTERY SALES	FLASHLIGHT SALES
THREATENED	0.058***	0.139***	0.373***	0.842***
	(0.015)	(0.011)	(0.023)	(0.042)
AFTER landfall	0.446***	0.311***	1.116***	1.122***
	(0.064)	(0.052)	(0.095)	(0.158)

Table AX8: The effect of hurricane threats and strikes on sales of emergency supplies where threats only include locations within 100 miles of coast

	(1)	(2)	(3)	(4)
Sample	2002-2005	2006-2012		
Unit of observation	Store-week	County-week		
Dependent Variables (in logs)	BOTTLED WATER SALES	BOTTLED WATER SALES	BATTERY SALES	FLASHLIGHT SALES
THREATENED	0.065***	0.198***	0.530***	1.119***
	(0.015)	(0.013)	(0.029)	(0.052)
AFTER landfall	0.489***	0.285***	1.141***	1.151***
	(0.090)	(0.089)	(0.163)	(0.244)

Table AX9-1. Summary statistics for locations that are struck vs. locations that are never struck: Period 1 (2002-2005) hurricane seasons

	Percent Black	Percent College	Median Income	Miles to Coast
Never Struck	18.74	26.33	46406	134.32
	(17.39)	(15.38)	(16748)	(96.69)
Struck	16.34	22.05	41074	15.89
	(18.01)	(11.07)	(11909)	(18.04)

Table AX9-2. Summary statistics for locations that are struck vs. locations that are never struck: Period 2 (2006-2012) hurricane seasons

	Percent Black	Percent College	Median Income	Miles to Coast
Never struck	20.51	27.43	51533	118.81
	(13.74)	(11.81)	(15326)	(91.68_
Struck	21.22	26.35	50862	30.18
	(10.01)	(6.56)	(9662)	(17.26)