Hurricanes and Gasoline Price Gouging

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Abstract:

Conventional wisdom suggests that gasoline price gouging before and after natural disasters is widespread. To explore this conjecture, we compile data on more than 4.7 million daily station-level retail gasoline prices. We combine these data with information on wholesale rack prices, spot prices, hurricane threats and landfalls, weather, traffic, and power outages. We investigate the effect of hurricanes on retail prices, wholesale prices, retailer margins, fuel price pass-through, and share of stations reporting transactions. We exploit the fact that the exact timing and location of hurricane landfalls is conditionally exogenous for identification. We find no evidence for widespread price gouging. Instead, we document evidence consistent with shortages predicted by theory in the presence of restrictions on price movements.

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1. Introduction

Price increases on essential goods before, during, and after emergencies lead to public outrage. Philosophers argue that “price gouging” is morally wrong. Arguments emphasize a basic failure of respect for persons and simple injustice (Sandel 2009; Snyder 2009a, 2009b). Governors and Attorneys General of hurricane-prone states publicly accuse businesses and individuals of ‘unconscionable’ price increases around disasters (Rapp 2005). After Hurricane Katrina, President George W. Bush likened gas station operators to looters (Ball 2011).

In contrast, economists and some legal scholars argue that price increases around natural disasters can be welfare enhancing. Although gouging attributable to excess market power may reduce efficiency, price increases may also reflect real shocks to supply and demand, allocate resources efficiently in times of scarcity, prevent overbuying and hoarding, and incentivize producers and retailers to proactively prepare (Deck and Wilson 2004; Zwolinski 2008, 2009). Alternatives like price controls may lead to shortages, unproductive allocations, and even physical altercations (Hayek 1968; Olmstead and Rhode 1985; Zwolinski 2008). Nobel laureate Milton Friedman once remarked, “gougers deserve a medal” (Stossel 2018).

Despite widespread attention and controversy, we lack systematic evidence on price rises during and after disasters. Is price gouging common in practice? If so, when, where, and how does it occur? This paper provides early evidence. To do so, we compile more than 4.8 million daily station-level gasoline prices from roughly 11,600 retail stations operating in Florida and Louisiana during the 2004 to 2008 hurricane seasons. We merge in bulk upstream prices, wholesale rack prices, and hurricane threat and landfall data. We add detailed station-level characteristics, weather data, hourly traffic data, and hand-collected power outage information. We focus on gas prices because consumers, the popular press, and the small existing literature presume that gouging is especially rampant for gasoline (Deck and Wilson 2004; Rapp 2005). We focus on Florida and Louisiana between 2004 and 2008 because of storm activity. This period includes several of the costliest hurricanes in U.S. history, including Charley, Frances, Ivan, Jeanne, Dennis, Katrina, and Wilma (Blake and Gibney 2011).

The simplest and most transparent way to establish causal identification is to combine rich microdata with conditionally exogenous treatments. Here, our extensive station-level data facilitate straightforward research designs. We begin with an easily interpretable analysis that is equivalent to generalized differences-in-differences under common assumptions. Our main analyses use event study designs that rely on fewer assumptions. In all designs, we exploit the fact that the exact timing and location of hurricane strikes is plausibly exogenous for identification. We address omitted variable concerns, as well as the possibility that impacted populations may be non-representative due to Tiebout sorting or other social dynamics. We confirm robustness across empirical choices.

We find no evidence consistent with price gouging. We fail to reject a null hypothesis of no average effect of hurricanes on wholesale and retail gasoline prices before and during hurricane landfalls. We find statistically significant 7 to 11 cent increases in wholesale and retail gasoline prices 4 to 14 days after nearby hurricane landfalls.[2] However, once we control for input price changes, post-landfall effects on both wholesale and retail prices become small and negative. Once we control for input price changes, point estimates from preferred specifications show that wholesale and retail price margins *declined* on average by roughly 3 cents after hurricanes made nearby landfall.

In contrast to small effects on margins, we find large impacts of hurricanes on the number of stations reporting transactions on a given day. The number of stations in landfall areas reporting at least one transaction falls by approximately 40 percent on the day of landfall and remains below counterfactual levels for roughly 8 to 10 days. Even after controlling for demand and supply shocks proxied by local road traffic, local power outages, disaster declarations, and weather, the number of stations in landfall areas reporting at least one transaction falls by approximately 25 percent on the day of landfall. Conditional price reports remain below counterfactual levels for roughly 6 to 10 days.

Taken as a whole, our results show that neither wholesale racks nor retail gas stations raise prices unusually far above costs around disasters. On average, conditional on remaining open, gasoline stations' pricing behavior follows business as usual or amounts to small temporary losses. Heterogeneity explorations suggest that some retailers fare better than others in ways consistent with predictions from economic theory. In short, while conventional wisdom suggests

[2] Single storm analyses suggest the effects are larger after Hurricanes Katrina, Rita, and Ike.

that gasoline price gouging is rampant, we provide evidence to the contrary. We instead document evidence consistent with shortages predicted by simple economic theory in the presence of restrictions on price movements.

We make three contributions. First, we provide systematic evidence on the extent and nature of gasoline price gouging around disasters. Reports of gasoline price gouging are widespread (Maxouris and Silverman 2018; Puleo 2019). Due to data limitations, however, the most complete existing evidence involves analyses of selected areas around individual storms (USFTC 2006, Neilson 2009).[3] Second, we explore the causes and consequences of price shocks in gasoline markets. A growing literature uses exogenous shocks to illuminate the industrial organization of fuel markets (Borenstein et al. 1997; Hastings 2004; Blair and Rezek 2008; Taylor et al. 2010; Fink et al. 2010; Lewis 2011; Anderson and Elzinga 2014). Related studies explore determinants of gasoline prices and retail margins (Myers et al. 2011, Barrage et al. 2020). We build off these studies to examine a new context: price gouging. Third, we contribute to the literature investigating the effects of hurricanes on economic market outcomes (Vigdor 2008; Groen and Polivka 2008; De Silva et al. 2010; Michel-Kerjan and Kousky 2010; National Academies 2012; Pindyck and Wang 2013; Gallagher 2014; Deryugina 2017; Gallagher and Hartley 2017; Deryugina et al. 2018; Beatty, Shimshack, and Volpe 2019; Deryugina and Molitor 2019). Although we use similar research designs, we explore different questions.

2. Background

Gasoline Marketing and Supply. Gasoline is marketed and traded at spot, rack, and retail levels. The upstream spot market for Louisiana and Florida is the Gulf Coast refining hub located near the Texas-Louisiana border. Here, large volume ('bulk') transactions are traded at prices influenced by New York Mercantile Exchange trading and regional supply shocks (Berhang 2017). From the spot / bulk market, refined gasoline travels by pipeline or ship to wholesale fuel terminals called racks. Louisiana racks are typically served by pipelines, and most Florida racks are served by shipping routes. Rack prices, adjusted daily, are largely determined by spot pricing,

[3] Beatty, Shimshack, and Volpe (2019) noted no statistically significant changes in the prices of batteries, flashlights, and bottled water before, during, or after hurricanes.

transportation costs, fees, and branding requirements (Berhang 2017). Branded gasoline is typically blended at the rack with additives including proprietary chemicals and ethanol.

Gasoline is purchased at wholesale racks by trucking companies called jobbers. Jobbers transport branded or unbranded fuel to downstream retail gasoline stations.[4] Branded retailers are contractually obligated to purchase branded gasoline from specific racks and jobbers, with trading prices typically pinned to the brand's rack price or another index.[5] Unbranded retailers may purchase gasoline from any supplier at more flexible rack and jobber prices. Retail prices are ultimately determined by rack prices (including branding premiums), local transportation costs, taxes, and station-level margins.

At all points in the fuel distribution chain, prices are also influenced by the type of refined product. We focus on conventional regular gasoline with an octane rating between 85 and 88. Conventional gasoline prices are also influenced by vapor pressure regulations. Vapor pressure is a measure of volatility, and thus, volatile organic compound (VOC) pollution discharges. In areas with multiple vapor pressure requirements, we average across prices.[6]

Price gouging from a legal or popular perspective. Legal definitions of price gouging are imprecise and vary across states.[7] Most definitions at least indirectly presume opportunism on the part of the seller (Rapp 2005). State price gouging laws target price increases on possibly essential goods during emergencies. Florida law compares prices to average prices charged over the 30-day period before the disaster, and a "gross disparity" constitutes price gouging. Louisiana law requires prices not to exceed those "ordinarily charged" in the same market area at or immediately before the emergency declaration. Florida and Louisiana laws, however, go on to

[4] Some large retailers and independent jobbers purchase gasoline directly from bulk pipeline locations or refineries, bypassing racks. We assume the price advantage from these marketing strategies is limited through arbitrage.

[5] Branded stations can either be 'company-owned,' owned by the upstream refiner, or operated by leasees. Leasees typically sign long-term contracts with refiners to purchase branded fuel and pay a fee back to the refiner.

[6] Results are not sensitive to using Reid Vapor Pressure (RVP) 7.8 prices during summer months and RVP 9.0 prices during fall months.

[7] The FTC raised concerns over the lack of clear definition of price gouging in their investigation of price gouging after Hurricane Katrina (USFTC 2006). The FTC report notes, "Although widely understood to refer to significant price increases (typically during periods of unusual market conditions), the term "price gouging" …. lacks an accepted definition. It is neither a well-defined term of art in economics, nor does any federal statute identify price gouging as a legal violation. States that prohibit price gouging have not adopted a common definition …."

note that retail price increases attributable to changes in input prices or general market trends are not considered price gouging.

Gouging laws take effect following formal gubernatorial emergency declarations and last up to 30 days after declarations expire. Although statutes apply to many essential emergency commodities, gasoline is a common focus. Florida law provides for civil penalties capped at \$1,000 per transaction and \$25,000 per day for multiple violations by the same seller. Louisiana law permits civil penalties and criminal sanctions capped at \$500 or 6 months imprisonment for violations committed willfully; \$5,000 or 5 years imprisonment for violations leading to serious injury or property damage; and 21 years of hard labor imprisonment for violations associated with one or more deaths.

Citizen concerns about gasoline price gouging around hurricanes are common. Google Trends data suggest that internet searches with keywords like “gasoline prices” and “gas gouging” spike dramatically during landfall weeks and in the weeks immediately following landfall (Appendix Fig. A.2). Consumers concerned about price gouging around disasters are encouraged to report suspicions to local law enforcement, district attorney offices, or state Attorneys General. Reports may be submitted by web hotline, phone, or in person. Media reports indicate that Attorneys General and Consumer Protection agencies receive and investigate hundreds to thousands of complaints of ‘unconscionable’ gasoline price increases per day during major hurricane emergencies (Maxouris and Silverman 2018; Puleo 2019). Many of these complaints are ultimately proven unfounded, and only a very small fraction result in penalties or reimbursements (Puleo 2019).

Conceptual framework. Without a sharp economic or even legal definition, price gouging is perhaps most naturally thought of in a residual sense (USFTC 2006). Price increases around natural disasters that are consistent with typical seasonality, day of week variability, or longer-run trends are not price gouging. Price increases around natural disasters that are directly explainable by changes to input costs are not price gouging. An implication is that price gouging requires an abnormal increase in margins above input prices on critical goods, after controlling for seasonality and longer-run trends.

In light of this first-order empirical prediction, we take a reduced-form empirical approach to causally examining the effect of hurricanes on retail prices, wholesale prices, and margins (both retail and wholesale) in the gasoline sector.[8] The literature on fuel market margins and pass-through helps motivate our empirical analysis and guides interpretation of our results (Seade 1985; Doyle and Samphantharak 2008; Marion and Muehlegger 2011; Scharfstein and Sunderam 2016; Stolper 2018). Taken as a whole, the literature predicts that pass-through of cost shocks is a function of the demand characteristics (including elasticities and curvature), supply characteristics (including elasticities and curvature), market power and cost structure, capacity constraints, and possibly other factors. A limitation of our reduced-form approach is that we will not be able to fully disentangle mechanisms, which may include hurricane shocks to supply leading to scarcity rents or hurricane conditions pivoting demand and exogenously changing the firm's ability to exert market power.

We note the following empirical predictions. First, price gouging requires an abnormal increase in margins over input prices during hurricane periods. Second, increases in margins over input prices will vary heterogeneously. Given most forms of imperfect competition, the literature suggests that more positive changes in margins around hurricane shocks will be associated with: branded stations, stations facing less competition, and stations nearer to highways (a proxy for more inelastic demand). Additionally, more positive changes in margins will be associated with stations experiencing larger supply shocks (a proxy for the likelihood of scarcity rents).

3. Data

Gasoline price and station data. We obtain proprietary daily retail-level regular-grade gasoline prices between June and October for each of 2004 to 2008 from the Oil Price Information Service (OPIS).[9] During our sample period, OPIS data were collected using fleet credit card swipes via an agreement with a company called Wright Express. We observe transactions from 11,603 unique stations in the raw data, including roughly 90 percent of Florida stations and 75 percent of

[8] We do not attempt to innovate with regards to theory. We do not observe quantities in our OPIS data so parameterizing a formal model of gasoline pricing is beyond the scope of the present paper.

[9] June through October are the hurricane intensive months in FL and LA. No hurricanes formally threatened or struck FL or LA in November of our sample years. Absent treatments outside of June - October, we saved data acquisition costs by restricting the sample period.

Louisiana stations operating between 2004 and 2008 (Appendix A, Appendix Fig. A1). OPIS retail data are the most extensive gasoline price data available, and sample stations are representative of all owner or operator types (Lewis 2011).

For each station-day, we merge in daily wholesale gasoline prices using proprietary rack prices also obtained from OPIS (Appendix A). Non-branded stations are matched to average regular-grade wholesale prices at the nearest fuel rack terminal. Branded stations are matched to the average branded fuel price at the nearest fuel rack terminal. We also merge in daily refinery-level Gulf Coast spot / bulk prices from the Energy Information Administration.

We collect several time-invariant station-level characteristics. OPIS data provide station address, brand, and station name.[10] We use brand information to construct indicators for whether a station is affiliated with a vertically integrated oil company ('branded') or a major independent retailer ('major retailer'). The former typically have arrangements with their parent companies to purchase branded wholesale fuel, and both types of stations have more sophisticated marketing arrangements compared to independent stations (Hastings, 2004; Lade and Bushnell, 2019). We enter address information into a Google Maps application programming interface to construct latitude and longitude. Given latitude and longitude, we create measures of competition, including the number of stations within 1, 5, and 10 km of the retailer as well as the distance to the nearest competing retailer. We also combine latitude and longitude data with U.S. Census Bureau road data to construct distance to the nearest major highway. Finally, we construct an indicator for whether a station lies within one of NOAA's coastal hurricane forecast ('watch' or 'warning') zones.

For each station-day, we construct measures of local power outages, local traffic, disaster declarations, and weather. We hand-construct a power outage dataset at the county-by-day level using information from the Department of Energy, National Energy Technology Lab Emergency Situation Report outage maps. Outages are measured as the share of county residents without power on a given day. We obtain traffic volume data from the Florida and Louisiana Departments of Transportation. We assign traffic measures to each geocoded station-location based on

[10] Our sample includes thirteen branded companies: Shell, Citgo, Chevron, BP, ExxonMobil, Sunoco, Hess, Texaco, Marathon, Murphy, Valero, Conoco, and Gulf. Major retailers include 7-11, Circle K, etc.

observations at the nearest of 381 permanent (geographically fixed) traffic monitors. We also collect disaster declaration data at the county-by-day level from the Federal Emergency Management Agency (FEMA). We collect weather data from the Global Historical Climatology Network. We assign weather measures to each location based on observations at the nearest weather station with complete data. Results are robust to other assignment mechanisms for outage, traffic, and weather data.

Hurricane data. We define hurricane “treatments” based on nearby landfalls. We collect latitude, longitude, time, date, and intensity of landfall from NOAA’s National Hurricane Center Atlantic Basin Best Tracks HURDAT2 database. We define landfalls following National Weather Service conventions as the intersection of the surface center of a hurricane with coastal land. Although hurricanes may make multiple landfalls, for simplicity, we define treatments based on first landfall.[11] Fourteen hurricanes made landfall within 100 miles of at least one of our Florida and Louisiana retail stations during the 2004 to 2008 hurricane seasons (Appendix Table A.1). These include seven of the costliest hurricanes in U.S. history (Charley, Frances, Ivan, Jeanne, Dennis, Katrina, and Wilma, as per Blake and Gibney 2011).

Final sample. We combine prices, time-invariant station characteristics, and daily weather / traffic / outage / disaster declaration data with hurricane treatments constructed at the station-by-day level. Many station-days have missing retail price data. With retail prices determined by fleet card swipes, a station’s daily average price was automatically recorded if at least one fleet card was swiped at that station on that particular day. However, if no fleet cards were swiped at that station on that particular day, the station-day price is missing in our data.[12] Initially, we follow the literature and limit our primary analysis to stations with the most consistent reporting outside of treatment periods (Lade and Bushnell 2019; Barrage et al. 2020). Our preferred sample includes data from all stations with retail price data from over 75 percent of possible sample days

[11] Results are not sensitive to this choice. Some specifications also control for official National Weather Service hurricane watches and warnings.
[12] With data automatically generated by fleet card swipes, stations themselves had no direct control over data reporting during our sample period.

not immediately preceding or following a nearby hurricane landfall. This final main analysis sample includes roughly 3.19 million retail prices observed over 765 possible days at 4,673 retail stations. Remaining missing data are left missing. We later explore robustness to alternative sampling choices.

Table 1 presents summary statistics for the analysis sample of 4,673 retail stations. Average retail gasoline prices were $2.76 but ranged widely over years (Appendix Text B.1). Retail prices exhibited heterogeneity by type of station, with branded stations charging roughly 4 cents/gallon higher prices than major retailers. Coastal and inland stations had very similar prices. Retail margins - defined as retail prices less wholesale prices and reflecting taxes, transportation costs from racks to stations, and retail markups - averaged just under $0.70/gallon.[13] Stations, on average, were close to highways (0.4 kilometers), but the distribution was highly skewed with the median station under 0.05 kilometers from the nearest highway. We also observe substantial heterogeneity in local market concentration. The average station had two competitors within 5 kilometers, but over 10% of the sample had no competitors within 5 kilometers. Some stations had as many as eight competitors within 5 kilometers.[14]

4. Empirical Strategy

In this section, we lay out our research designs. We begin with a simple differences-in-differences analysis that is easy to interpret under common assumptions. We then turn to event study approaches that rely on fewer assumptions. In principle, our approaches establish causal inference by exploiting the fact that the exact timing and location of hurricane landfalls is plausibly exogenous.

Under common assumptions, our approach to generalized difference-in-difference (DiD) analyses compare changes in outcomes around landfall date for treatment stations near a given landfall relative to control stations not near that landfall. An advantage of this method is it allows a transparent and familiar interpretation of results. The disadvantage is strong assumptions,

[13] The federal gas tax is 18.4 cents/gal, Louisiana's gas tax is 20 cents/gal, and Florida's gas tax is 41.4 cents/gal. OPIS estimates transportation costs are typically 1.5 cents/gal.

[14] Appendix B and Figures B.1 and B.2 present additional summary statistics. The simple plot of prices against time in Figure B.1 supports our more formal assertions that follow. We see strong evidence that bulk and wholesale prices respond more sharply and more quickly to hurricanes than retail prices.

including the stable unit treatment value assumption (Imbens and Rubin 2015), specific parallel trend assumptions (Marcus and Sant'Anna 2020), and homogenous treatment effects across groups and periods (de Chaisemartin and D'Haultfoeuille 2020). For example, SUTVA may be violated in our setting if hurricane treatments spill over to influence control stations. Or, heterogeneous treatment effects could generate estimates that represent the weighted sum of average treatment effects where some of the weights are negative (Goodman-Bacon 2020).

As a result, our main analysis focuses on event study research designs that require fewer assumptions for causal interpretation. These approaches explore if and how outcomes at treatment stations depart from counterfactual outcomes for the same station and time had there been no hurricane impacts on that day. Event studies focus only on treated stations.

In all designs, our initial outcomes of interest are retail prices, wholesale rack prices, and both wholesale and retail margins (i.e., differences between output price and input price). We explore cost pass-through from wholesale rack prices to retail prices in particular detail using a cumulative dynamic multiplier approach. We then investigate the effects of hurricanes on the share of retail gas stations with observed station-day prices, i.e., the effects of hurricanes on the share of stations with at least one fleet card swipe on that day. One notable feature of our data is that during and after hurricane landfalls we observe far fewer stations with at least one fleet card swipe. We explore whether fewer observed transactions can - or cannot - be explained by demand and supply shocks as proxied by local traffic volume, power outages, FEMA disaster declarations, and weather. We also consider heterogeneous treatment effects.

Prices and Margins. We begin with difference-in-difference approach to analyzing retail prices, wholesale prices, retail margins, and wholesale margins. Treatments focus on the two weeks before and after each hurricane first made landfall.[15] To ease interpretation, we aggregate days into three periods: fourteen days to four days before landfall (Before), three days before to three days after landfall (Landfall), and four days to fourteen days after landfall (After). Results are not sensitive to these specific aggregations. We estimate:

[15] We standardize landfall data to the first day a hurricane made landfall in either state. The one exception is Hurricane Katrina that made landfall in Florida on August 25, 2005 and in Louisiana on August 29, 2005. We treat these as separate hurricanes in our analysis.

$$Y_{ist} = \beta_1 1[Before]_{ist} + \beta_2 1[Landfall]_{ist} + \beta_3 1[After]_{ist} + \alpha_{i(s)} + \delta_m + \pi_y + \varphi_w + X'_{ist}\Gamma + \epsilon_{ist} \quad (1)$$

where Y_{ist} is the retail or wholesale price at station i located in state s on day t. *Before*, *Landfall*, and *After* are indicator variables equal to one if station *i* is within 100 miles of a landfall during the relevant time period.

To control for seasonality, day-of-week variation, and trends within and across years, we include fixed effects for year (π_y), month-of-year (δ_m), and day-of-week (φ_w). Parsimonious specifications include state fixed effects (α_s) to account for different tax rates and other time-invariant factors varying across states. Our preferred specifications include station-level fixed effects (α_i) to account for station-specific cost factors and other time-invariant station characteristics.[16] X'_{ist} represents a vector of controls, including quadratic temperature controls and indicators for whether a station was under a tropical storm or a hurricane warning or watch on day t. Standard errors for retail price regressions are clustered at the county-level and wholesale price regression standard errors are clustered at the rack-level. We later explore robustness to alternative clustering choices.

In specifications with retail prices as the dependent variable, we estimate (1) with and without controls for upstream wholesale fuel prices. In specifications with wholesale prices as the dependent variable, we estimate (1) with and without controls for upstream bulk fuel prices. Regressions without upstream price controls provide evidence for price impacts but do not distinguish between price changes attributable to changes in input costs and those attributable to changes in margins above costs. In contrast, regressions with upstream price controls inform margins above input costs and thus help explore our first-order question of whether or not wholesalers and retailers are engaging in behavior consistent with price gouging.[17]

We also consider the effects of hurricanes on retail margins across the full distribution of stations. To do so, we estimate station-by-station versions of the model in (1):

$$Y_{it} = \beta_{0i} + \beta_{1i} 1[Before]_{it} + \beta_{2i} 1[Landfall]_{it} + \beta_{3i} 1[After]_{it} + \delta_i w_{it} + \gamma_{im} + \pi_{iy} + \epsilon_{it}\,, \quad (2)$$

[16] Station margins may include costs that are common to all stations (e.g., federal and state taxes), region-specific costs (e.g., average trucking costs from wholesale racks), and station-specific costs and markups over wholesale costs (which may account for factors such as local monopoly power).

[17] So long as output and input prices are cointegrated, including contemporaneous input prices sufficiently controls for the long-run relationship between output and input prices. Results are robust to allowing for lagged price adjustment by including lagged input prices in the analysis.

for all stations i, where Y_{it} are station-specific retail prices and w_{it} are station-specific wholesale prices.[18] Station-by-station estimates allow us to investigate the existence and characteristics of "bad apples."

Under common assumptions, the difference-in-difference approaches in equations (1) and (2) are familiar and easily interpreted in levels. One concern is that two way fixed effects specifications assume homogenous treatment effects to generate estimates reliably interpretable as unbiased average treatment effects. We note that the literature's focus on negative weights is commonly driven by treatments that turn on and stay on (Goodman-Bacon 2020, Sun and Abraham 2020, Callaway and Sant'Anna 2020, de Chaisemartin and D'Haultfoeuille 2020).[19] In our setting, treatments turn on and off in the +/- 14 days before and after landfall and do not persist. Nevertheless, we explored robustness by performing a stacked event-by-event difference-in-differences model with 'clean' controls, as suggested by Cengiz et al. (2019) (Appendix Text B.4). This approach ensures that previously treated stations do not serve as controls for stations impacted by future hurricanes.

Another natural concern is that hurricanes may impact prices beyond 100 miles of landfall, thereby violating SUTVA (Imbens and Rubin 2015). Given the potential regional or national impact of hurricanes on fuel markets, hurricanes may spill over to influence outcomes at stations beyond the immediately impacted area. In this case, finding a valid control group for any given hurricane may be challenging.

Our main analyses, therefore, use event study research designs that rely on fewer assumptions. For example, our event study approaches address SUTVA violations where treatments spill over to impact control units. Our event study estimates do not require homogenous treatment effects across groups and events, as long as the pattern of effects remains similar over time (Abraham and Sun 2018, Goodman-Bacon 2019).

[18] We reliably estimate coefficients for 2,770 of 4,663 stations. We confirm that average coefficients for the pre-hurricane, hurricane, and post-hurricane periods all align with the aggregate results for all 4,663 stations in (1).
[19] For example, the literature focuses on heterogenous policy adoption timing across jurisdictions where the treatment effects "stays on" permanently after adoption in any given jurisdiction.

In our event study analyses, we limit the sample to stations within 100 miles of a landfall point and focus only on prices for those stations in the fourteen days before and after landfall. We estimate the model:

$$y_{ist} = \sum_{j=-\tau}^{\tau} \beta_j 1[t = j] + \alpha_i + \delta_m + \pi_y + \varphi_w + X_{ist}'\Gamma + \epsilon_{ist}. \quad (3)$$

Regressions of the form of (3) include the same controls as our preferred specifications in equation (1). We allow for differential treatment effects by event-day, β_j. One difference from the standard event study setting is that some storms in our sample arrived in quick succession so that stations may fall into two event windows at a single time. As a hypothetical example, the same station might be observed both 10 days after one storm and 12 days before another. As a result, unlike classic event studies, the indicators $1[t = \tau]$ are not perfectly collinear. Since the level is unidentified in equation (3), we normalize the coefficients to be relative to β_{-14}, the coefficient on $1[t = -14]$.[20] This normalization should be innocuous since, after controlling for station-level fixed effects, we find no impact of hurricanes two weeks before landfall in our difference-in-differences model.

We study heterogeneous price and margin impacts by interacting event-day indicators in equation (3) with station and location characteristics. We consider differential effects: branded stations vs. retail and unbranded (or independent) stations; stations with fewer local competitors vs. stations with more local competitors; and stations nearer to major highways vs. stations further from major highways. We also consider whether price and margin impacts differ for stronger (category 4 or 5) hurricanes vs. weaker (category 1, 2, or 3) hurricanes.

We test for differential cost pass-through between hurricane periods and untreated periods. The literature studying gasoline markets notes a delayed response of retail fuel prices to changes in upstream fuel costs (Borenstein et al. 1997; Lewis and Noel 2011; Lewis 2011). Pass-through patterns may differ during hurricanes if fuel supply chains are interrupted, if stations are hesitant to pass-through increased wholesale costs in the aftermath of a storm, or for other

[20] In principle, different storms arriving in short succession could impact the same station generating temporal spillovers. This situation is infrequent, and we replicated the analysis with non-overlapping treatments. Results are similar when we drop stations that fall in more than one event window at the same time and estimate a traditional event-study regression relative to the landfall day. We also later conduct a storm-by-storm analysis, which is not influenced by this concern. Results are robust on average.

reasons. Formally, we follow the standard approach in the literature (e.g., Borenstein et al. 1997) and estimate pass-through on sub-samples of our data as follows:

$$y_{it} = \alpha_{\mathrm{i}} + \sum_{j=0}^{L-1} \beta_j \Delta w_{it-j} + \beta_L w_{it-L} + \delta_m + \pi_y + \varphi_w + X'_{it}\Gamma + \epsilon_{it} \qquad (4)$$

Equation (4) is a cumulative dynamic multiplier (CDM) model that estimates the average retail price response in our sample to changes in wholesale fuel costs (w_{it}). Coefficients β_j represent cumulative pass-through rates of wholesale fuel costs to retail prices after *j* days. Based on data exploration and consistent with earlier work, we set *L=30* to allow prices to exhibit lagged adjustment to cost shocks within one month.

Price Reporting. With station-by-day price data determined by fleet card swipes, a station's price is automatically recorded if a fleet card is swiped at that station on that particular day. A station's price is missing if no fleet cards were swiped at that station on that particular day. In all periods, including hurricane periods, missing data on a given day might be explained by reduced demand for that station's gasoline. In hurricane periods, missing data on a given day might also be explained by supply disruptions like power outages, damage, or a closure at that station. It is also possible that a station operator unable to recover variable costs will strategically choose a short-run shutdown or stockout.

Given that direct data on station-level closures and/or quantities sold are unavailable, we use the methods summarized by equations (1) and (3) above to explore the probability that a given station reports a price (experiences a swipe) on a given day. We replicate our event study design to establish estimates of the share of stations reporting prices (i.e., experiencing a swipe) in baseline periods and during hurricane periods. These event studies include station, day-of-week, month-of-year, and year fixed effects. We then attempt to shed some light on the extent to which reporting declines observed during hurricane periods can – or cannot - be explained by changes in common proxies for gasoline demand and supply factors like nearby road traffic, weather, power outages, and FEMA disaster declarations. Finally, we explore heterogeneity.

5. Results: Hurricanes and Retail Prices, Retail Margins, and Fuel Price Pass-Through

Table 2 summarizes results from analyses of the form of (1). Panel A presents results for retail and wholesale prices without conditioning on upstream prices. Panel B presents results for retail and wholesale prices after conditioning on upstream price controls. Panel B provides evidence of hurricane impacts on retail and wholesale fuel price margins.

We first consider the effects of hurricanes on wholesale rack prices in Table 2 columns (3) and (4). In Panel A, we document that wholesale rack prices increased before and during landfall periods, but increases were small and not statistically significant. In the 4 to 14 days after nearby hurricane landfall, wholesale prices increased by roughly 11 cents, and the increase was significantly different from zero at the 10 percent level in our preferred specification. In Panel B, once we control for upstream bulk prices and thus consider changes in wholesale rack margins above input prices, pre-landfall and during-landfall coefficients remain small and statistically insignificant. Wholesale rack price changes after landfall now become small and statistically insignificant. Point estimates suggest that wholesale racks experienced slightly smaller margins than normal, averaging roughly 3 cents lower than counterfactual margins, in the days after nearby hurricane landfall.

We next consider the effects of hurricanes on retail prices in Table 2 columns (1) and (2). In Panel A, we document modest evidence that unconditional retail prices increase before, during, and after hurricanes. Increases in prices before and during landfall are small and not statistically significant. We find a 7 to 9 cent increase in retail gasoline prices 4 to 14 days after nearby hurricane landfalls. These latter increases in observable prices are statistically significant in regressions with state-level fixed effects and not statistically significant in regressions with station-level fixed effects. In Panel B, once we control for upstream wholesale rack prices and thus consider changes in retail margins above input prices, pre-landfall and during-landfall coefficients remain small and statistically insignificant. Post-landfall retail price coefficients become negative once we control for changes in upstream prices. In our preferred retail specification with station-level fixed effects, column (2) of Panel B, post-landfall retail margins are a statistically significant 3 cents lower (relative to a counterfactual) after hurricanes made nearby landfall.

We explore robustness to assumptions necessary to interpret two-way fixed effects approaches in (1) as average treatment effects (Appendix Text B.4). Stacked event-by-event difference-in-differences models with control stations chosen to ensure no contamination yield results similar to those in Table 2. After conditioning on input prices, preferred specifications document no statistical impact of hurricanes on wholesaler or retailer margins during pre-landfall periods and statistically significant yet small negative impacts of hurricanes on wholesaler and retailer margins during post-landfall periods.

Given that, on average, retail stations experience lower margins following hurricanes, a natural question is whether this result masks heterogeneity where a subset of "bad apples" charges large positive margins after nearby landfall. We thus consider the effects of hurricanes on retail margins across the distribution of stations with station-by-station regressions, as specified in equation (2). Figure 1 presents a histogram and kernel density of the station-specific post-hurricane margin estimates. Consistent with aggregate results reported in Table 2, the average post-hurricane margin is roughly 3 cents per gallon lower than the counterfactual margin. Most stations experience negative or small excess margins. Approximately 4 percent of stations charge post-hurricane excess margins that exceed $0.20 per gallon. Further explorations suggest that stations that charge large excess margins after nearby hurricane landfalls are somewhat more likely to charge higher margins in non-hurricane periods. However, we find limited evidence for other systematic associations between post-hurricane excess margins and common correlates of gasoline supply and demand (Appendix Text B.2).

Figure 2 summarizes results from our main event study regressions of the form of equation (3). We focus on event studies with retail prices and margins as the outcome.[21] Figure 2(a) provides event study results for observed retail prices, and Figure 2(b) provides event study results for retailer margins (retail price - wholesale rack price). Figure 2(a) suggests that retail

[21] Economic and public policy attention typically focuses on retail-level price gouging. Additionally, wholesale margins are based on the difference between wholesale rack prices and a region-wide bulk price defined by the Gulf Coast price traded out of Houston. This measure does not perfectly represent racks' input prices, even if those input costs will typically be correlated with this measure. As such, we focus our subsequent analysis on retail prices and margins. We did explore wholesale margins in an event study analysis, where we considered racks serving stations within 100 miles of landfall point and the prices for those racks in the 14 days before and after landfall (Appendix Fig. B.6). We do not find evidence consistent with wholesale rack price gouging. Instead, we continued to find evidence that racks experienced declines in margins around hurricane landfalls.

prices remain statistically similar before, during, and after nearby hurricane landfalls. Point estimates suggest that retail prices begin to climb roughly 10 days after landfall. Figure 2(b) suggests that retailer margins remain statistically unchanged before and during hurricane landfalls, but fall by 2 to 4 cents per gallon in the roughly 4 to 10 days after landfall. Margins appear to recover within 14 days after landfall. Declining retailer margins after landfall are consistent with difference-in-difference (two way fixed effects) results and suggest that wholesale prices increases are not fully passed through to retail prices in the immediate period following landfall.

Figure 3 explores heterogeneity in event study results with retailer margins as the outcome. Although we plot all coefficient estimates, we present standard errors (dashed lines) for the baseline category only (i.e., the first category in the legend). Consistent with earlier results, Figure 3 shows that most stations experience declines in retail margins after hurricanes make landfall. The heterogeneity depicted in Figures 3(a) to 3(d) is consistent with several predictions from the existing literature described in our conceptual framework. Declines in margins are larger for unbranded independent and major retailer stations than for branded retailers. Declines in margins are larger for stations further from a highway than for stations closer to a highway. Heterogeneity results for other predictions are less clear cut. In general, local competition seems to lead to higher margins, though the effect is imprecisely estimated. Stronger storms do not differentially impact margins during or after landfall relative to weaker storms. Corresponding difference-in-differences results suggest similar patterns for branded vs. other station comparisons and nearby competitor vs. few nearby competitor comparisons (Appendix Fig. B.5).

We also explored retail and wholesale rack prices on a storm-by-storm basis (Appendix Text B.2, Appendix Fig. B.10). Retail prices are smoother on average than wholesale rack prices. Consistent with the aggregate results reported above, we observe relatively limited effects of most hurricanes on prices before landfalls. After landfall, wholesale cost shocks are not immediately passed through to retail prices. We observe substantial heterogeneity in these post-landfall effects. Several storms - like Hurricanes Frances, Ivan, Jeanne, and Arlene - had limited effects on average prices. Other storms - like Katrina, Rita, and Ike - generated larger movements in both rack and retail prices. After Hurricane Katrina (Fl.), Hurricane Katrina (La.), and Hurricane Ike, gaps between retail and wholesale rack prices shrank rapidly before later widening.

We next consider the extent to which retail stations experienced declining margins after hurricanes by formally considering pass-through for treated stations. We use the cumulative dynamic multiplier model in equation (4) to consider pass-through from wholesale to retail prices. We focus on stations in landfall areas and prices for those stations in the 30 days before and after landfall. We extend the temporal window to 30 days to allow cost shocks to more completely pass-through to output prices.

Figure 4(a) presents our primary pass-through results. Each point estimate represents the cumulative pass-through of a (normalized) \$1.00/gallon increase in wholesale fuel costs after the corresponding number of days. The cost shock occurs on day 0. For illustration, the point estimate for day 6 shows that, following a \$1.00/gal shock to wholesale rack prices, retail prices increase by \$0.497/gal on average by the sixth day following the shock.

Figure 4(a) shows that full unit pass-through of hurricane costs shocks to retail prices takes roughly 25 days.[22] As such, hurricane shocks to wholesale prices are eventually fully passed-through to the retail prices paid by consumers. However, we observe two additional findings relevant for price gouging considerations. First, although pass-through levels out around 1 (i.e., 100%), pass-through does not exceed 1. On average, hurricane cost shocks do not lead to increases in retail prices that exceed corresponding increases in wholesale rack prices. Second, even though cost shocks are eventually passed through, for roughly 25 days stations sell gasoline at lower than normal margins. For roughly 10 days after hurricane costs shocks, stations selling gasoline are doing so at margins well below typical margins.

To put the results in Figure 4(a) in context, we consider how pass-through of shocks to wholesale prices around hurricanes compares to the pass-through of more general shocks to

[22] With a longer pass-through window, we still find that pass-through flattens after 25-30 days (Appendix Fig. B.15). There are a few interpretations of lagged retail price adjustment in the literature. First, sticky prices may be due to, for example, menu costs (Barro 1972; Mankiw, 1985). This is unlikely in fuel markets given the observed frequency of price changes. Others include slow inventory adjustment, market power, imperfect information, and consumer search costs (Deltas 2008; Lewis 2011; Lewis and Noel, 2011). In the slow inventory adjustment interpretation, fuel stations store fuel on site and purchase fuel from jobbers or racks infrequently. A 50% pass-through rate after two weeks could represent 50% of stations turning over tanks.

wholesale prices. Since essentially all observed shocks around hurricane landfalls in our analysis are positive (i.e., generate increases in rack prices), an appropriate comparison is pass-through of shocks for stations in landfall areas around landfall periods *versus* pass-through of positive shocks for all stations and all periods.[23] Figure 4(b) presents the results. Blue diamonds denote pass-through rates for positive shocks using the all station / all period sample, and red triangles denote pass-through rates at treated stations in the 30 days before and after nearby landfall. We find that average cost pass-through after shocks at treated stations during treatment periods is similar to average cost pass-through after positive shocks in the full sample. If anything, Figure 4(b) depicts slightly slower than usual cost pass-through for treated stations for roughly 10 days after the arrival of the hurricane shock to wholesale prices.

Results from our main approaches summarized by equations (1), (3), and (4) are robust to alternative empirical choices. We considered robustness to alternative sample construction. Price results are largely unchanged when we use a sample of stations that report prices (i.e. at least one fleet card swipe) on more than 95% of sample days or a sample of stations that report prices (i.e. at least one fleet card swipe) at least once every week over the entire sample period (Appendix Figs. B.11-B.12, Appendix Tables B.2-B.3). Results are also robust to choices regarding clustering of standard errors. We replicated our difference-in-difference style analysis to allow for correlated errors across all directly vulnerable coastal stations in each state. We replicated the analysis using two-way clustering where spatial units were defined as previously described and temporal units were defined at the year-month level (Appendix Table B.4.). Results were similar. We explored specifications that allow for lagged adjustment (Appendix Fig. B.13), extended event windows (Appendix Fig. B.14), and longer wholesale pass-through periods (Appendix Fig. B.15).

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[23] Borenstein et al. (1997) document that stations in some retail markets pass-through positive cost shocks more quickly than negative cost shocks, a pattern often referred to as 'rockets and feathers.' We estimated:

$$y_{it} = \alpha_i + \sum_{j=0}^{L} \beta_j^+ \max[\Delta w_{it-j}, 0] + \sum_{j=0}^{L} \beta_j^- \min[\Delta w_{it-j}, 0] + \delta_m + \pi_y + \varphi_w + X'_{it}\Gamma + \epsilon_{it},$$

where the cumulative pass-through rate J days after a positive cost shock is given by $\sum_{j=0}^{J} \beta_j^+$ and the cumulative pass-through rate J days after a negative cost shock is $\sum_{j=0}^{J} \beta_j^-$. Like Borenstein et al. (1997), we found strong evidence for asymmetric pass-through (Appendix Fig. B.7). As such, we make "apples to apples" comparisons of pass-through of hurricane shocks to pass-through of positive non-hurricane shocks.

Taken as a whole, we find no evidence that stations change retail prices in ways that are consistent with price gouging. Around hurricanes, retail prices observed by consumers rise a small amount, or not at all, on average. Retail prices do not rise more than wholesale input prices. Although wholesale price shocks induced by hurricanes are eventually passed through fully to consumers, this pass-through occurs with a considerable delay and occurs somewhat more slowly that pass-through of other wholesale price increases. Our best evidence suggests that margins at treated retail stations decline, on average, in the two weeks following hurricanes. Margins at branded stations and stations near major highways decline the least.

6. Results: Hurricane Impacts on Station Reporting and Closures

Prices are missing in our data when a fleet card is not swiped at a station on a given day.[24] This can be caused by several factors, many of which are exacerbated by hurricanes. On the demand side, reduced road traffic may mean a reduced need for gasoline. Alternatively, a station may be closed so no fleet card can be swiped. Stations may be closed for several reasons, including damage, power outages, safety concerns, and strategic shutdown when operators are unable to recover wholesale fuel cost shocks.

We analyze the the relationship between the probability that a station reports a price (experiences at least one swipe) and nearby hurricane landfalls. To do so, we adapt our event study framework. We estimate a linear probability model where the outcome is whether or not a station reports a price (experiences at least one swipe) on a given day. We include event-time dummies as well as station, day-of-week, month-of-year, and year fixed effects. We run regressions separately for stations within 100 miles of landfall and for all inland stations in areas not under a hurricane watch or warning (loosely speaking, a placebo group).

Figure 5(a) presents results. We see a large statistically significant drop in the share of stations reporting prices (experiencing at least one swipe) in impacted areas during and after landfall. The number of stations reporting a price falls by approximately 40 percent on the day of

[24] By construction, stations included in our sample report price information at least 75% of the time during non-landfall periods (Appendix A).

landfall and remains below counterfactual levels for roughly 8 to 10 days. After 8 days, point estimates remain negative but become small and statistically insignificant.

We attempt to shed some light on the extent to which reporting declines (declines in the number of fleet card swipes) observed in Figure 5(a) can – and cannot - be explained by changes in specific proxies for demand and supply factors such as nearby road traffic, local power outages, weather, and FEMA disaster declarations. Electricity outage and FEMA disaster declaration data proxy for supply shocks. The average station in a landfall area is in a county that goes from having no power outages before landfall to roughly 30% of the county experiencing some disruption on the day after landfall (Appendix Fig. B.4). On average, outages return to baseline levels within seven days of landfall. We proxy for demand conditions using nearby traffic flows. Given the nature of gasoline sales, local road traffic seems like a reasonable proxy for demand changes. On average, traffic falls sharply, roughly 1.5 standard deviations from baseline, after a hurricane makes landfall but returns to normal within four days (Appendix Fig. B.3).

Figure 5(b) replicates the analysis in 5(a) after controlling for supply and demand proxies like local power outages, local traffic, and FEMA disaster declarations. We document that the number of stations in landfall areas reporting a price (experiencing at least one fleet card swipe) falls by approximately 25 percent on the day of landfall and remains below counterfactual levels for 6 to 10 days. When compared to the results in Figure 5(a), Figure 5(b) illustrates that our controls explain an important and statistically significant share of the missing retail prices (i.e. the reduction in fleetcard swipes). In the first three days after landfall, additional controls explain roughly 50 percent of the overall decline in price reporting.

However, although local traffic, power outages, weather, and disaster declarations explain a significant fraction of the reduction in fleetcard swipes, a large share of missing reported prices remains unexplained. One explanation for this residual variation is that our supply and demand proxies are incomplete. For example, supply chain disruptions uncorrelated with local traffic, outages, and disaster declarations are possible. An additional explanation for the residual variation is strategic closure or strategic stockout. We show that wholesale cost shocks around hurricanes are eventually passed through to retail prices. However, for 10 or more days, stations that continue to sell gasoline on average do so at margins significantly below typical margins. If lower margins do not allow some stations to recover their short-run variable costs, incentives for strategic stockouts or strategic shutdown arise.

We explore strategic stockout incentives further by examining heterogeneous impacts across stations. Unbranded (independent) retailers experienced the largest decrease in margins (Figure 3(a)). As a result, they may also experience the largest incentives to engage in strategic closure or strategic stockouts as their pass-through of increased wholesale costs is more limited. We test whether unbranded stations have a larger share of unexplained missing prices (i.e., unexplained reductions in fleet card swipes). We replicated both the difference-in-differences and event study regressions, controlling for demand and supply shocks, for branded, retail, and unbranded stations separately.

Figure 6 summarizes results. In Figure 6(a), we document that unbranded stations are not significantly less likely to report prices before landfall but are significantly less likely to report prices during and especially after landfall. Price reporting for branded stations and stations affiliated with major retailers in landfall areas are not statistically less likely to have differential reporting than stations in the control group. Event study results in Figure 6(b) are less precise but suggest a similar story. In the event studies, branded and unbranded stations have similar declines in reporting when hurricanes make landfall. However, price reporting at branded stations recovers more quickly while unbranded stations take much longer to return to baseline price reporting.

We can not rule out alternative explanations for the results in Figure 6. Branded and major retail stations may be better managed, have more reliable supply chains, or have unobserved qualities that attract fleet card customers to return more quickly after hurricanes. Nonetheless, taken as a whole, results in Figures 5 and 6 are consistent with at least some stations strategically stocking out / strategically closing during and after hurricane landfalls.[25]

7. Discussion and Conclusion

[25] We reemphasize that, despite the increase in missing price data around hurricanes, our main findings that stations do not engage in price gouging behavior are robust using samples of stations that report prices at least once every week over the entire sample period including hurricane periods (Appendix Figs. B.11-B.12, Appendix Tables B.2-B.3).

In this paper, we investigate the effect of hurricanes on retail and wholesale gasoline prices and margins. We find no evidence that wholesalers increase prices above and beyond increases in bulk input prices before, during, or after hurricanes. We establish that retailers do not increase gasoline prices above and beyond increases in wholesale rack prices before, during, or after hurricanes. Point estimates indicate that retailers that continue to sell gasoline do so at margins (retail – wholesale prices) *below* their steady-state margins. We document that the share of stations with at least one fleet card swipe falls markedly during and after hurricanes - even after using local power outages, traffic, weather, and emergency declarations to control for demand and operational shocks.

We note caveats. First, we do not rule out the possibility that a small number of stations price gauge. Our main results document that the average station does not increase prices opportunistically around hurricane landfalls. Our heterogeneity results suggest that some stations' margins fall more than other stations' margins, but the small number of stations that charge higher than normal margins after hurricanes exhibit no systematic characteristics. We find no evidence consistent with unscrupulous or coercive pricing among any subgroup. Nevertheless, our econometric exercise may obscure behavior by a few outlying 'bad apples'. Second, our results are conditional on the Florida and Louisiana 2004 to 2008 hurricane season context. Florida and Louisiana are disproportionately prone to hurricane landfalls, and the 2004 to 2008 period ranks among the most active Atlantic hurricane seasons on record. Nevertheless, different market structures, population characteristics, and other factors may generate different results for other states and other time periods. Since the 2004 to 2008 Florida and Louisiana hurricanes were highly salient (including Katrina), it is possible that results do not generalize.

The above caveats notwithstanding, our results have natural implications for economics and policy. First, we find no evidence consistent with price gouging. Common beliefs that price gouging around hurricanes is widespread are unfounded. We also find evidence consistent with strategic shutdowns or stockouts after hurricanes. Although other explanations are possible, our results may indicate that stations choose to remain closed given an inability or unwillingness to recover higher input costs after hurricanes. Around hurricane landfalls, short-run scarcity pricing might naturally be expected to clear markets. Instead, given intense consumer and media scrutiny, stations may respond to social pressures and fear of legal action by strategically stocking out or underinvesting in the physical capital (like generators) needed to sell fuel after hurricanes. Inefficient short and long run outcomes are plausible, and a complete welfare analysis represents a promising area of future research.

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Tables

Table 1: Summary Statistics (2004-2008)

	Mean	Std. Deviation	N	N (Stations)
Prices				
Retail Price ($/gal)	2.76	0.64	3,196,641	4,673
Branded Major	2.78	0.64	1,203,949	1,777
Major Retailer	2.74	0.63	1,324,135	1,889
Coastal	2.76	0.64	1,266,735	1,863
Inland	2.75	0.64	1,929,906	2,810
Wholesale Price ($/gal)	2.07	0.62	3,574,845	18
Bulk Price ($/gal)	1.98	0.64	3,574,845	--
Station Characteristics				
Distance to Highway (km)	0.42	1.01	3,574,845	4,673
Distance to Competitor (km)	0.78	1.49	3,574,845	4,673
Competitors within 5km	2.19	1.92	3,574,845	4,673

Table 2: Effect of Hurricanes on Retail and Wholesale Prices and Margins

	(1)	(2)	(3)	(4)
Dep. Var	Retail	Retail	Wholesale	Wholesale
Panel A: No upstream price controls				
Pre-Hurricane	0.042	0.022	0.017	0.020
	(0.036)	(0.035)	(0.038)	(0.040)
Hurricane	0.050	0.028	0.035	0.038
	(0.033)	(0.032)	(0.027)	(0.030)
Post-Hurricane	0.089**	0.069	0.114*	0.116
	(0.045)	(0.043)	(0.065)	(0.067)
Observations	3,196,641	3,196,641	3,574,845	3,574,845
Stations/Racks	4673	4673	18	18
Panel B: Upstream price controls				
Pre-Hurricane	0.022	-0.001	0.028	0.025
	(0.016)	(0.014)	(0.016)	(0.016)
Hurricane	0.033	0.007	0.009	0.007
	(0.021)	(0.019)	(0.018)	(0.017)
Post-Hurricane	-0.006	-0.028**	-0.033	-0.036
	(0.014)	(0.011)	(0.026)	(0.025)
Wholesale Price	0.785***	0.786***		
	(0.006)	(0.006)		
Bulk Price			0.719***	0.719***
			(0.005)	(0.004)
Observations	3,196,641	3,196,641	3,574,845	3,574,845
Stations/Racks	4,673	4,673	18	18
State F.E.	Yes	No	Yes	No
Station/Rack FE	No	Yes	No	Yes
Year F.E.	Yes	Yes	Yes	Yes
Month-of-Year FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes

Notes: The dependent variable is station-level retail/wholesale price. "Hurricane" is an indicator for whether a station is within 100 miles of a hurricane landfall in the three days before, during, or three days after landfall. "Pre-Hurricane" and "Post-Hurricane" are similar indicator variables for stations in landfall areas ten to four days before and after landfall, respectively. All regressions include controls for whether a station is under a storm/hurricane watch or warning and quadratic temperature controls. Standard errors are clustered at the county for retail regressions and wholesale rack for wholesale regressions. *, **, and *** denote significance at the 10%, 5% and 1% level.

Figures

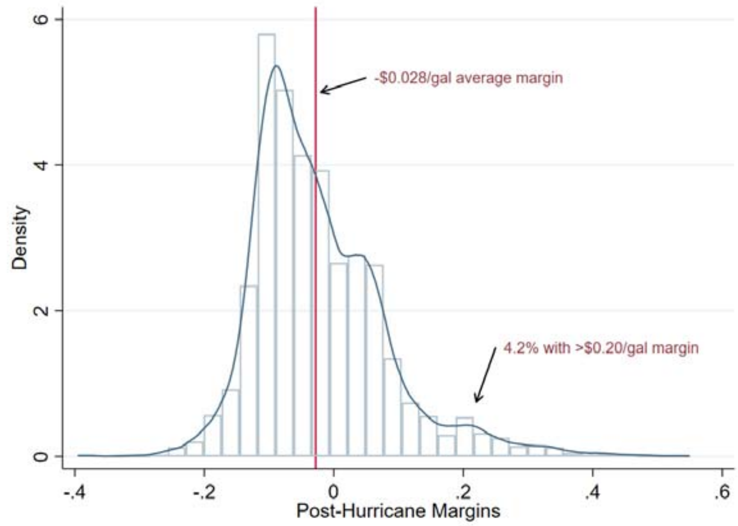
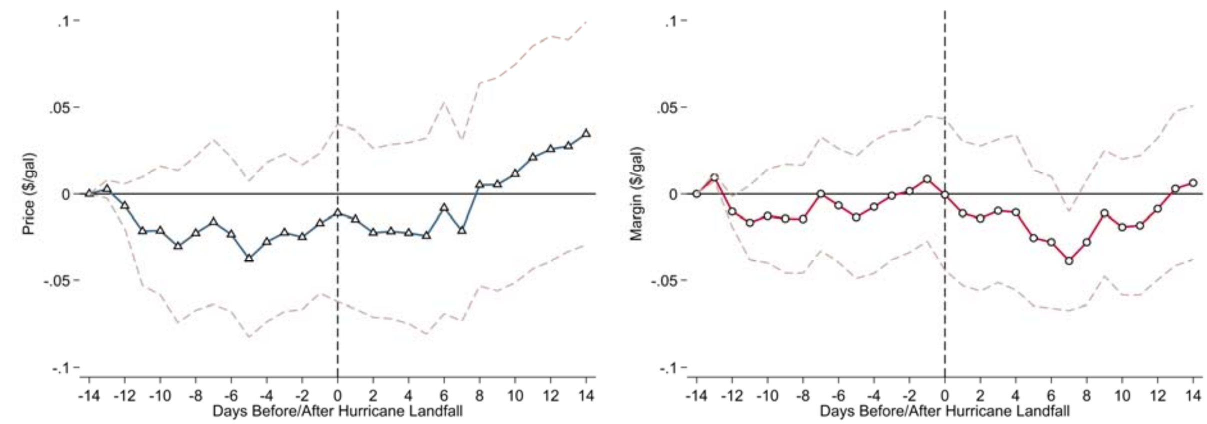


Figure 1 – Post-Hurricane Markup Heterogeneity

Notes: The figure presents a histogram and kernel density of station-specific post-hurricane margin estimates. The red vertical line denotes the average impact.

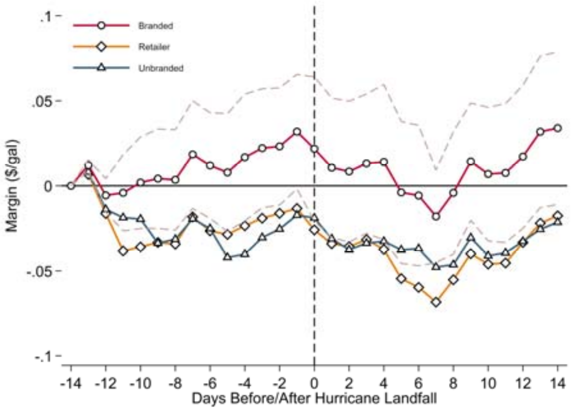


2(a): Price Impacts

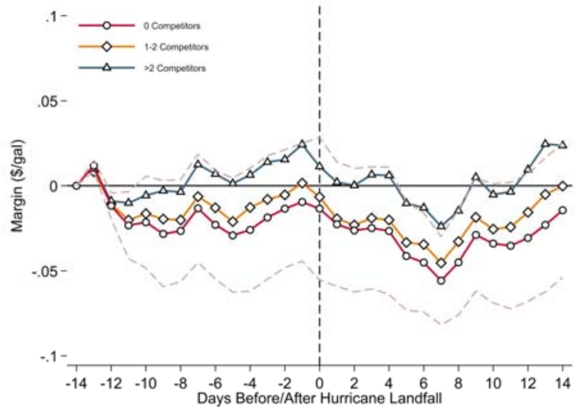
2(b): Margin Impacts

Figure 2 – Retail Price and Margin Event Studies

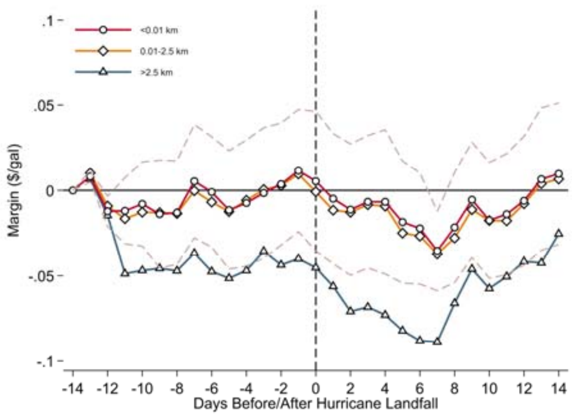
Notes: The Figure presents event study results of the impact of hurricanes on prices (a) and margins (b). All coefficients are normalized relative to day -14. Standard errors are clustered at the county level, and grey dashed lines are 95% confidence intervals.



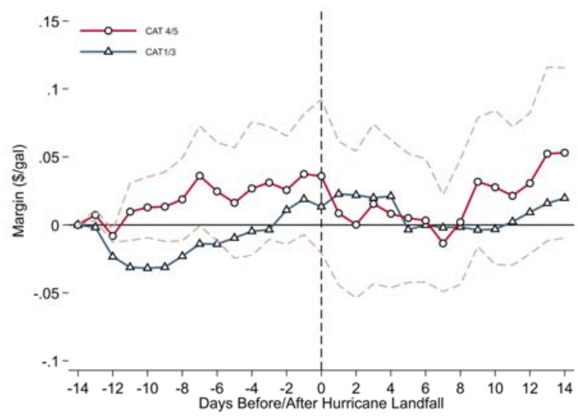
3(a) Station Ownership



3(b) Local Competition



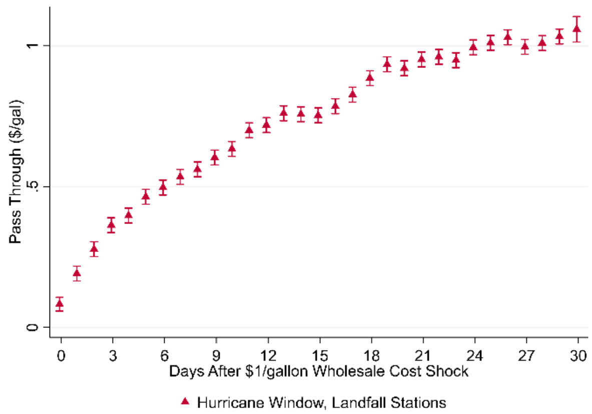
3(c) Distance to Highway



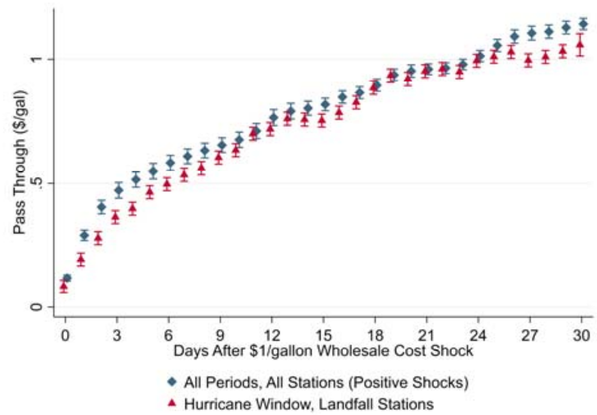
3(d) Storm Intensity

Figure 3: Treatment Effect Heterogeneity

Notes: The Figure presents event study results of the impact of hurricanes on station margins, allowing for heterogeneous impacts across station ownership (a), local competition (b), the stations' distance to a highway (c), and the storm intensity (d). All coefficients are normalized relative to day -14. Standard errors are clustered at the county level, and grey dashed lines are 95% confidence intervals. All figures present standard errors for the first legend category only.



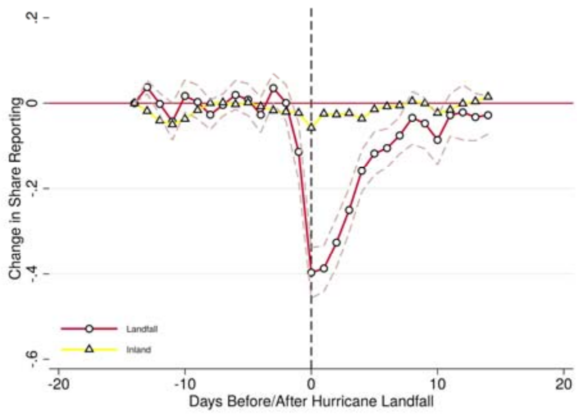
4(a) Hurricane Shocks



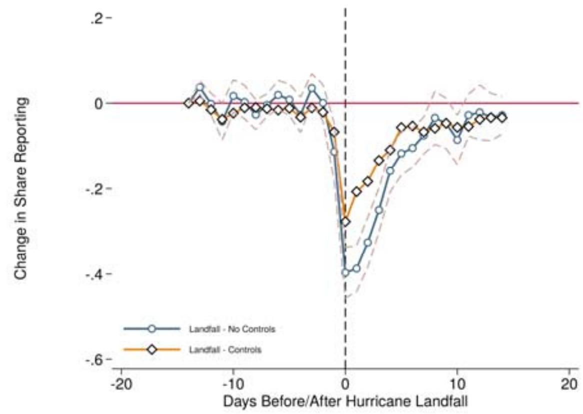
4(b) Positive Shocks (All Stations/Periods) vs. Hurricane Shocks

Figure 4: Wholesale Cost Pass-Through to Retail Prices

Notes: Figure 4(a) presents cumulative retail cost pass-through after a $1.00/gal cost shock for stations in landfall areas in the 30 days before or after a hurricane makes landfall. The cost shock occurs on day 0. Figure 4(b) compares pass-through at treated landfall stations (red triangles) to pass-through of positive cost shocks at all stations in all periods (blue diamonds). Standard errors are clustered at the county level, and vertical bars are 95% confidence intervals.



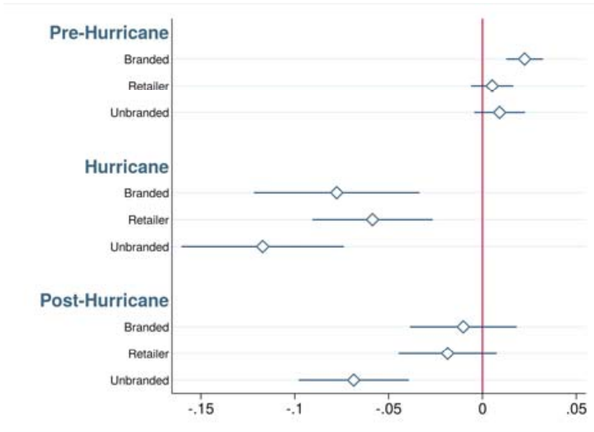
5(a) Inland vs. Landfall Areas



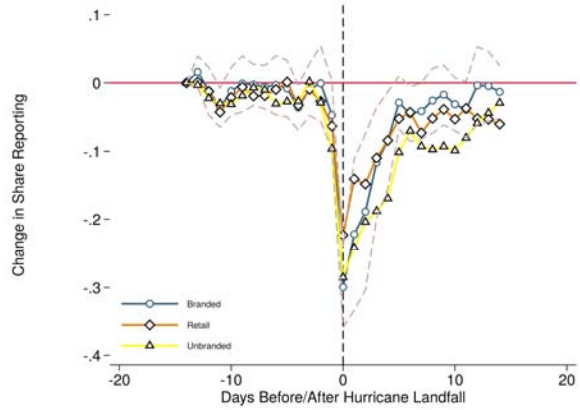
5(b) Landfall Areas with and without Demand/Supply Controls

Figure 5: Changes in Reporting Behavior

Notes: The Figure presents event study results of the impact of hurricanes on price reporting. Figure 5(a) compares the share of stations reporting at least one fleetcard swipe for stations in landfall areas (circles) and for stations in inland areas (triangles). Figure 5(b) compares reporting at stations in landfall areas when we exclude supply/demand proxies (circles) to reporting at stations in landfall areas when we include supply/demand proxies (diamonds). Standard errors are clustered at the county level, and grey dashed lines are 95% confidence intervals.



6(a) Difference-in-Differences



6(b) Event Study

Figure 6: Changes in Reporting by Station Type

Notes: The Figure summarizes results of the impact of hurricanes on price reporting by station type. Figure 6(a) reports the coefficients on the differences in differences results. Results are presented for three different station-types: branded, major retailer, and unbranded. Figure 6(b) presents event study results of the impact of hurricanes on price reporting. Circles represent branded retailers, diamonds represent major retailers, and triangles represent unbranded retailers. Standard errors are clustered at the county level, and grey dashed lines are 95% confidence intervals.

Online Appendix for

Hurricanes and Gas Price Gouging

Tim Beatty, Gabriel E. Lade, and Jay Shimshack

October 16, 2020

A. Supplemental Data and Data Details

Wholesale terminal data. We purchased wholesale terminal (or rack) price data from OPIS for all gasoline terminals in Florida and Louisiana for hurricane-season months (June to October) for 2004 to 2008. The data include daily wholesale gasoline prices for a total of 19 racks. OPIS reports several wholesale prices for each rack. We keep the OPIS average price for each rack and all major branded gasoline prices if reported by the stations.

OPIS average gasoline prices have full data coverage over our sample. Branded wholesale prices is sparse at some racks. Thus, we calculate the fraction of days with non-missing prices over the full sample and keep only rack-brand prices with greater than 75% reporting over our full sample. We replace all remaining missing branded prices with the average OPIS gasoline price at each corresponding rack.

We match stations to the nearest wholesale rack. If the nearest wholesale rack includes a branded wholesale price, we match branded stations to those prices. All other stations, and branded stations where no reliable branded price is available, are matched to the average OPIS wholesale price.

Retail Sample Restrictions. Retail price data from OPIS include 4,782,500 daily prices reported for 11,603 gasoline stations in Florida and Louisiana. Prices are reported at irregular intervals spanning hurricane-season months (June to October) for 2004 to 2008.

We filter the retail price data in the following ways to define our final sample. First, we create a balanced panel for each station, filling in all days with no reported prices with missing prices. The resulting data include the original 11,603 stations with missing and non-missing retail price data for 765 days for a total of 8,876,295 observations. We define a hurricane-window as the 14 days before, during, or after any hurricane made landfall in Florida or Louisiana. We then calculate the percentage of days that a station reports prices in each year and over the entire sample outside of hurricane windows.

Our main station sample includes only stations that report price data for at least 75% of non-hurricane days over all five years. This includes just over 3.5 million observations of 4,673 stations reporting over 3.19 million prices. First, we use the sample of stations that report prices for 75% of non-hurricane days but impose the restriction by year instead of over the entire sample. Second, we use the sample of stations that report prices for 75% of non-hurricane days in 2004, our first year, and follow them through all years regardless of reporting in later years. Last, we use all stations included in the original OPIS data. Figure A.1 graphs the location of all stations in our sample.

Hurricane and Landfall Data. Hurricane and landfall data are from NOAA. Fourteen hurricanes made landfall in Florida or Louisiana over our sample. Table A.1 lists the hurricanes, the first day a hurricane warning was issued in either state, the first day the hurricane made landfall in either state, the stations impacted by the hurricane and the stations reporting prices in a landfall area, and the average and highest sustained wind speed in the landfall areas. Two hurricanes, Bonnie and Charlie, came in quick succession. As such, we treat the two hurricanes as a single storm. Katrina made landfall in Florida three days before making landfall in Louisiana, so we treat Katrina as two separate hurricanes.

The number of stations impacted by each hurricane and average wind of each hurricane vary substantially. Katrina impacted over 1,250 stations, almost 300 of which don’t report prices during the landfall event. Six of the hurricanes had sustained winds exceeding 100 miles per hour. As expected, the storms with the highest winds are typically the same storms with the largest gap between stations in the landfall area and stations reporting prices in the landfall area.

Google Trends. Figure A.2 shows Google trends data for internet searches for “gas/gasoline prices” and “gas gouging” in Florida and Louisiana during the 2004 and 2005 hurricane seasons. The figures show dramatic upticks in search activity for these terms during and immediately following landfall weeks. Other state and year combinations illustrate the same patterns, although less starkly.

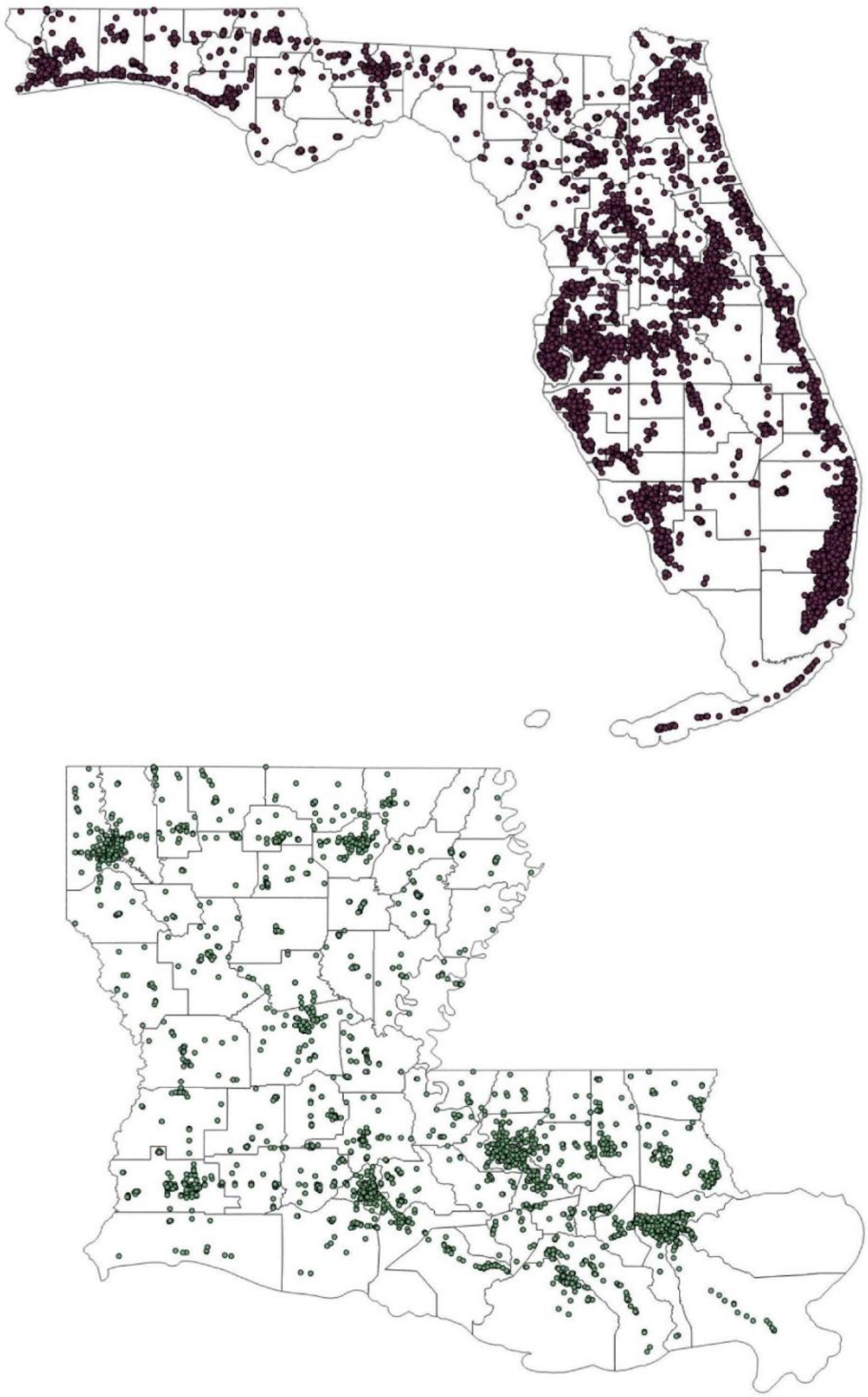
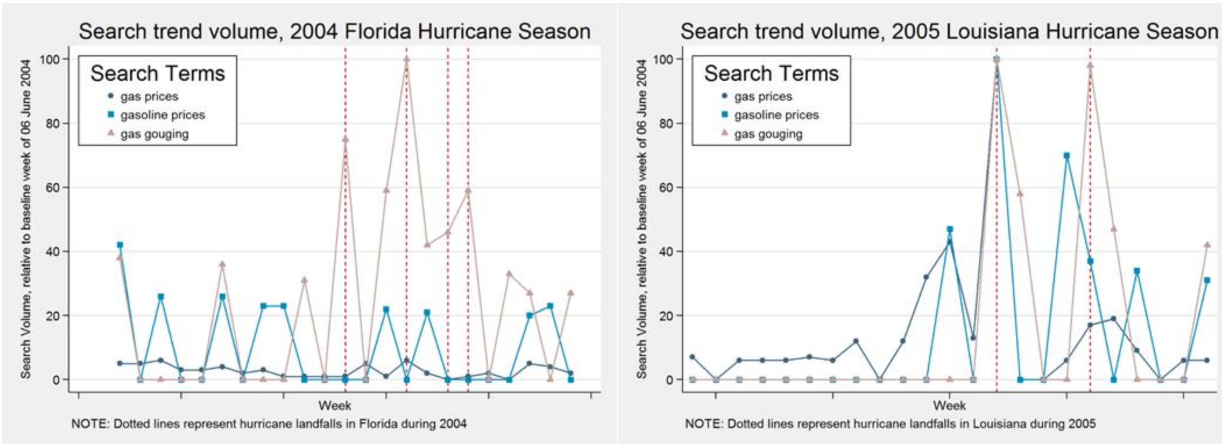


Figure A.1: Stations Locations



(a) 2004 Florida *(b) 2005 Louisiana*

Figure A.2: Search Volume Trends 2004 FL and 2005 LA hurricane seasons

Table A.1 - Hurricanes in Sample

Hurricane	First Warning	First Landfall	Stations in Landfall Area (Reporting)	Wind (High)	Highest Category
Bonnie/Charlie	8/11/2004	8/12/2004	887 (548)	130 (130)	4
Frances	9/2/2004	9/6/2004	184 (113)	50 (125)	4
Ivan	9/14/2004	9/16/2004	153 (7)	105 (145)	5
Jeanne	9/24/2004	9/26/2004	1007 (246)	105 (105)	3
Arlene	6/10/2005	6/11/2005	143 (90)	50 (60)	<1
Dennis	7/7/2005	7/10/2005	176 (5)	105 (130)	4
Katrina (FL)	8/24/2005	8/25/2005	1038 (966)	70 (150)	5
Katrina (LA)	8/27/2005	8/29/2005	216 (5)	110 (150)	5
Rita	9/18/2005	9/24/2005	99 (4)	100 (155)	5
Wilma	10/22/2005	10/24/2005	988 (146)	105 (160)	5
Alberto	6/12/2006	6/13/2006	324 (306)	40 (60)	<1
Humberto	9/13/2007	9/13/2007	67 (64)	80 (80)	1
Gustav	8/31/2008	9/1/2008	567 (68)	90 (135)	4
Ike	9/11/2008	9/13/2008	8 (2)	95 (125)	4

B. Summary Statistics, Robustness, and Additional Results

1. Additional Summary Statistics

Prices. Figure B.1 shows daily average retail, wholesale, and bulk prices for each year in our sample. Gray vertical bars represent a hurricane. We include the average wholesale and retail price across all stations in our sample in the bolded line. Thinner lines around each overall average are averages for subsamples of our data. These include stations and corresponding wholesale racks in each state, in coastal areas, and in the landfall area of each storm that made landfall in each year. The Figure shows the limited heterogeneity in retail and wholesale price around each storm. The Figure also highlights that while bulk and wholesale prices show substantial volatility, particularly around hurricanes, retail prices are much smoother in comparison. Another notable feature of the data are the large bulk price spikes after Katrina, Rita and Ike. These are due to the hurricanes impacting Gulf refinery operations. In all cases, wholesale and retail prices do not respond with nearly the same magnitude, likely due to low transactions occurring around these events and availability of emergency supplies.

Precipitation. Figure B.2 presents a corresponding figure for precipitation. The Figure highlights the substantial variation in the intensity of hurricanes, both across storms and across different areas during the same storm.

Traffic. We use a normalized measure of traffic at the traffic monitor closest to each station as a proxy for gasoline demand. Figure B.3 shows an event study of traffic around hurricane landfalls. We see that potential demand falls by about one and a quarter standard deviations on the day of landfall, but by the fourth day after landfall, traffic has largely returned to normal.

Power Outages. As a proxy for supply disruptions we use a measure of power outages at the county level. Figure B.4 shows an event study of power outages around hurricane landfalls. Power outages are widespread in the immediate aftermath of a hurricane landfall but return to pre-landfall levels within 6 to 7 days.

2. Additional Results

Difference-in-Differences Heterogeneity. Figure B.5 presents coefficient plot estimates from estimating our difference-in-differences model (equation 3) and interacting with each treatment variable the same indicators as used to produce Figure 2. While the levels are different due to the normalization in the event study, the relative effects of hurricanes before, during, and after landfall are generally consistent between Figure 2. Branded stations see no to very small impacts while independent stations see negative margins in all periods. One area of difference is among retail stations, who in the event study also experience negative margins, but in the difference-in-differences model show similar patterns as branded stations. We again find limited evidence of heterogeneity based on the competitiveness of local markets. We see some slight differences in the highway estimates. We find more limited heterogeneity in distance to highway in the difference-in-differences model where the event-study estimates show that stations >2.5 kms from a highway experience steady, negative margins around hurricanes.

Wholesale Rack Markups. Figure B.6 shows event-studies of rack margins for all hurricanes (Panel a) and all hurricanes except for Katrina/Rita/Ike (Panel b). We delineated these categories since Figure B.1 shows that Katrina, Rita, and Ike caused major bulk-price disruptions due to refinery outages. When we consider all racks, we see an immediate dip in margins in the two weeks leading up to landfall that is sustained after the hurricane, only recovering a week after the hurricane. We see a similar profile when we consider all hurricanes except for Katrina, Rita, and Ike. In all cases 'margins' are less clearly defined. While we carefully match wholesale gasoline prices based on station type to the nearest wholesale rack, bulk prices are defined as the Gulf Coast gasoline price traded out of Houston. This is not the fuel cost to the racks, though it is certainly correlated with it.

Asymmetric Pass-Through. Figure B.7(a) shows the average pass-through of positive and negative shocks for all stations in all periods. We find clear evidence of asymmetric pass-through. Stations pass-through positive cost shocks more quickly than they do negative cost shocks, on average. This phenomenon also holds if we look at the sample of stations that were ever in a landfall area and estimate the pass-through regression for all periods. Figure B.7(b) shows a similar asymmetric pattern among landfall stations, though negative cost shocks are estimated with much less precision due to there being few decreases in prices around hurricanes.

Gasoline Station Heterogeneity. We find no systematic evidence of gasoline price gouging following hurricanes in our sample. The result does not preclude some stations exhibiting gouging-like behavior. We explore whether there is evidence of some 'bad apples' in the data by estimating station-by-station versions of our difference-in-differences model. Specifically, we estimate:

$$y_{it} = \beta_{0i} + \beta_{1i}1[Before]_{it} + \beta_{2i}1[Landfall]_{it} + \beta_{3i}1[After]_{it} + \delta w_{it} + \gamma_m + \pi_y + \epsilon_{it}$$

for all stations *i* where w_{it} are station-specific wholesale prices. Given sample size limitations, we are only able to estimate station-specific markups for 2,770 of 4,663 stations.

Consistent with our main results in Table 2: Panel A, the average pre-hurricane, hurricane, and post-hurricane markups are centered around \$0.00, \$0.00, and -\$0.028/gal, respectively. Given in both the event study and difference-in-differences model we observe the most changes in prices and margins, we focus in the paper on analyzing heterogeneity in post-hurricane markups. Figure 1 highlights a right-skewed distribution centered around the negative average margin post hurricanes. Some stations (4.2% of the sample) charge an average post-hurricane margin \$0.20/gal above their normal margin.

The findings in the histogram could simply be due to statistical noise. We, therefore, further explore whether the station-specific, post-hurricane margins are correlated in a manner that is consistent with economic theory. We first correlate post-hurricane margins with an estimate of stations' baseline markups in Figure B.8. Baseline markups are using a similar estimate to the equation above but with no controls and collecting β_{0i}. Baseline markups, which include taxes, transportation costs, and station markups, mostly range between \$0.40/gal and \$1.00/gal. The estimated baseline markups are positively correlated with post-hurricane margins, suggesting that stations with higher initial markups fare better after hurricanes.

Further, we follow others (Lade and Bushnell, 2019; Stolper, 2018) and correlate the estimated station markups with observable supply and demand factors that should correlate with mark-ups if stations have local market power. Figure B.9 shows the correlations with two supply-side factors (competitor stations within 1 KM and whether stations are branded) and two demand-side factors (Census tract income and population from the 2010 Census). Stations' post-hurricane markups are positively correlated with the number of nearby competitors, whether a station is a branded major station, as well as the median income and population in the stations' Census tract. The latter three correlations are consistent with stations' exercising market power, i.e., exercising greater market power at stations with potentially more inelastic customers (more brand loyal, higher-income, and population-dense customers). Stolper (2018) and Lade and Bushnell (2019) find similar correlations. However, the first is inconsistent with firms adjusting markups based on greater market power after a hurricane -- stations in areas with more competition have higher post-hurricane markups than those in local areas with fewer competitors.

The correlations are unconditional. We, therefore, explore the relationship between post-hurricane excess markups and supply/demand observables using a regression framework. Table B.1 shows our results. Conditional correlations of the four variables above follow similar patterns when we include all four variables in column (1), though all factors except median income decrease in size and significance when we include county fixed effects that control for fine geographic factors that affect stations' post-hurricane markups in column (2). Including more demographic controls and county fixed effects in column (4) diminishes the importance of all of the four main factors. The only significant predictors of post-hurricane margins in column (4) are distance to nearest wholesale rack (positive correlation) and non-white population (negative correlation).

The coefficients do not give a sense of the relative importance of each variable above. To explore this, we multiply each coefficient in columns (3) and (4) by the range of each variable in our data. This allows us to estimate the potential impact on the post-hurricane markup of moving a station in, for example, a Census Tract with the lowest median income to the highest median income holding all other factors equal. Figure B.10 shows the estimates. In the state fixed effects model, post-hurricane markups are most associated with median income, distance to the nearest rack, non-white population, total population, and the percentage of vacant housing. When we compare stations within a county, however, all factors except distance to the nearest rack diminish in size and significance.

None of the above analysis is causal. Stations' location decisions are endogenous, and branded stations likely choose to locate in areas where they can exercise market power. Nonetheless, the above correlations provide weak evidence that some stations may adjust markups after a hurricane in a manner that is consistent with them exercising market power. However, the number of 'bad apples' is small, and the evidence is not entirely clear.

Hurricane-Specific Event Studies. Figure 3 explores the impacts across the most obvious margin, the size of hurricanes. Figure B.11 explores these heterogeneous impacts in more detail. The Figure includes before / during / after retail and wholesale rack price data for each storm. We observe two patterns. First, consistent with our main results, retail prices are smoother on average than wholesale rack prices. We observe relatively limited effects of most hurricanes on prices before nearby landfalls. After landfall, wholesale cost shocks are not immediately passed through to retail prices. One would expect modest declines in retailer margins, on average. Second, we observe substantial heterogeneity in the post-landfall effects of hurricanes. Several storms - like Hurricanes Frances, Ivan, Jeanne, and Arlene - have limited effects on average prices. Other storms - like Katrina, Rita, and Ike - generate larger movements in both rack and retail prices. After Hurricane Katrina (FL), Hurricane Katrina (LA), and Hurricane Ike, gaps between retail and wholesale rack prices shrank rapidly before later widening. After Hurricane Rita, gaps between retail and wholesale rack prices remained constant for a few days before widening markedly.

3. Robustness Checks

Difference-in-Differences Balanced Sample. Tables B.2 and B.3 re-estimate our difference in differences results using two definitions of a 'balanced' panel. Table B.2 reports results where we keep only stations that report prices for over 95% of our sample. Table B.3 reports results for stations that report at least one price in 100% of the weeks in our sample. Results are very similar, particularly for our preferred models.

Difference-in-Differences Alternative Standard Errors. Table B.4 reports two variants of our standard errors. Standard errors in parentheses are coastal-area-adjusted standard errors. For these, we replace county indicators in coastal areas with separate indicators. Specifically, we define a station as being 'coastal' if it was ever in a hurricane watch/warning/landfall area. We then define one coastal cluster for each state, but retain county clusters for inland stations. The number of clusters in this case decreases from 124 to 104. Standard errors in brackets are two-way clustered standard errors clustered at the coastal-area-adjusted-county and year-month. This allows for correlation over time within a common area (county or coastal area) as well as over space within a year-month. While this allows for more flexible correlation structures in the error term, we interpret these standard errors with caution since we only have 25 year-month clusters. The coastal-area-adjusted standard errors are slightly smaller than county clusters, while the two way clustered errors are larger. In both cases, post-hurricane margin losses remain significant.

Price and Margin Event Study Samples. Figures B.12 and B.13 explore the sensitivity of our price and margin event study results to the sample restrictions discussed in Appendix A. We show results using three alternative sample restrictions. The first panel (a) shows results for a sample where we impose the same 75% reporting requirement, but we do so by year. Thus, if a station has consistent reporting in 2004 but not 2005, it is in our sample in the former but not the latter year. Panel (b) shows results imposing the 75% reporting requirement in 2004 and carrying that sample forward for all years. Panel (c) presents results when we do not impose any sample restrictions and include all retail prices from OPIS. All results are very similar to our preferred estimates.

Lagged Adjustment. Our difference-in-differences model estimates long-run wholesale and bulk cost pass-through rates around 80% instead of the expected full cost pass-through. Meanwhile, the distributed lag model (Figure 3) shows full wholesale-to-retail cost pass-through after 30 days. To resolve this, we re-estimate equations (1) and (2) with distributed lag wholesale cost terms instead of the level of wholesale costs. Table B.5 presents corresponding results to Table 2 Panel B. Results are largely similar to our main findings, especially in our preferred station- and rack-fixed effects models. We no longer find a small negative retail margin after landfall. Instead, we estimate a 1.6 cent/gallon increase in the margin that is statistically insignificant. Wholesale-to-retail pass-through rates increase to (slightly over) 100%, and bulk-to-wholesale pass-through rates increase to 90%. Figure C.6 presents corresponding event-study results confirming that retail margins are unaffected by hurricanes. While the results here suggest that the distributed lag form may be more appropriate, we prefer to maintain the contemporaneous wholesale cost control model for a few reasons. Most importantly, including thirty lags necessarily drops June from our model given that we only observe prices during hurricane months. Hurricanes Alberto and Arlene are not included in our estimates as a result. Second, the distributed lag model requires absorbing a large number of degrees of freedom.

Extended Event Window. Figure B.14 shows an extended event study. We extend our window around landfall stations to include 21 days before and after landfall. For comparison purposes, we present all results relative to the fourteenth day before landfall. We find largely similar results, and observe that both the positive price results and negative margins after hurricanes are temporary, recovering back to pre-hurricane levels by the third week after landfall.

Extended Cost-Pass-Through. Figure B.15 shows an extended cost pass-through graph, allowing for up to 45 days for a $1 wholesale cost shock to affect retail prices. To estimate the landfall station pass-through equation, we extended the time window to 45 days pre- and post-hurricane. As in our main pass-through figure, we compare cost pass-through at these stations to cost pass-through for all stations in all periods. While pass-through slightly exceeds unity after 30 days, it generally flattens at the 30-day mark and bounces around for landfall stations.

4. Two-Way Fixed Effects Model Considerations

The difference-in-differences (DiD) model serves as an instructive first stage of our analysis. We present an intuitive framework from which we describe the impacts of hurricanes on gasoline prices and margins at stations and wholesale terminals directly impacted by hurricanes relative to stations that were not impacted by hurricanes. We examine impacts for the period leading up to, during, and after landfall at these 'treated' stations.

Several features of our setting may violate the assumptions required to consistently estimate our treatment effects of interest using the DiD model. Most important is a clear violation of the stable unit treatment values assumption (SUTVA). Hurricanes impact regional and national energy markets, causing clear spatial interference – hurricanes impact the operation and prices of control stations. As a result, we rely on the event-study methodology to draw causal inference of the average impact of hurricanes on retail and wholesale gasoline margins. We limit our sample to just stations in landfall areas and exploit *within station* changes in prices and margins to estimate the causal impact of hurricanes on these outcomes. Further, for each hurricane, we standardize the first landfall date to be the same, limiting the scope for spatial spillovers.[1]

A second concern relates to a growing literature showing shortcomings of two-way fixed effects DiD (TWFE) models. The literature considers settings where treatment is staggered over time, treatment impacts multiple groups, and treatment effects may be heterogeneous across groups or time (Goodman-Bacon 2019a; Marcus and Sant'Anna 2020; Sun and Abraham 2020; Callaway and Sant'Anna 2020; de Chaisemartin and D'Haultfoeuille, 2020). In these settings, TWFE models estimate a weighted average of causal effects of treatment, though the weights may not be convex. In particular, the weights assigned to each causal effect can be unintuitive and even negative (de Chaisemartin and D'Haultfoeuille, 2020). These concerns may invalidate the resulting treatment effect estimated by the TWFE DiD model.

Many of these concerns do not apply to our setting. Much of the previously cited literature focuses on settings where different localities adopt a policy at different points in time. After the policy is adopted, it

[1] This approach does not rule out all potential SUTVA violations. One concern could be *temporal contamination* from hurricanes that come in quick succession. For example, many stations were hit by both hurricanes Arlene and Dennis. These stations show up as observations both before and after landfall. Excluding these overlapping observations and estimating the event study for non-overlapping stations does not impact our results.

remains in effect for the remainder of the sample. Weights are determined by the sample size of each treated unit and how long treatment has been effective. Unlike these situations, treatment turns 'on' and 'off' (fourteen days before and after landfall) in our setting. There is little overlap in treatment, except for hurricanes that come in quick succession. Further, our event study design is not subject to these concerns and is, in fact, a recommended solution to these issues (Goodman-Bacon, 2019b).

Nonetheless, here we present an alternative difference-in-differences strategy that accounts for some of these concerns. We follow Cengiz et al. (2019) and estimate a 'stacked' event-by-event difference-in-differences model with 'clean' controls.

To do this, we first create event-specific panels of gas stations. Each event *h* dataset includes stations treated by hurricane *h* and 'clean' control stations for a 90-day panel by event time (t=-45, …, 45). Clean control stations are defined as stations that were not in any hurricane landfall area from 2004 to 2008. We ensure that all 'treated' stations include stations impacted only by the hurricane of interest during the 90-day panel. This ensures that previously treated stations do not serve as controls for stations impacted by future storms.[2] We then stack all of the event-specific datasets to calculate an average effect across the 14 storms as:

$$Y_{ist} = \beta_1 1[Before]_{st} + \beta_2 1[Landfall]_{st} + \beta_3 1[After]_{st} + \alpha_i + \delta_m + \pi_y + \varphi_w + X'_{ist}\Gamma + \epsilon_{ist} \quad (B.1)$$

Equation B.1 is an alternative to equation (1) since it uses a stricter criterion for admissible control and treatment groups and compares control in treatment in standardized 'event-time' windows.

Table B.6 presents the stacked difference-in-differences results. Results are broadly similar to our main difference-in-differences results, particularly for our preferred model with station or wholesale terminal fixed effects. When we do not include upstream price controls, we find positive retail price impacts of hurricanes in the state fixed effects model, but not in the station fixed effects model. When we include wholesale price controls, we find a positive margin impact of hurricanes in all periods. However, the result reverts to our main findings when we account for station-specific unobservables in Panel B, column (2). In this column, we again find no impact of hurricanes on margins in the pre-hurricane period, potentially positive impact on margins during hurricanes, and negative margin impacts post-hurricane.

Despite the cleaner interpretation of the difference-in-differences results, equation B.1 does not address outstanding SUTVA concerns. As a result, we continue to rely on our event study estimates when drawing conclusions.

[2] Stations impacted by hurricane Arlene are the one exception to this filtering mechanism. All stations impacted by Arlene were later impacted by Dennis. For consistency, we leave all Arlene stations in the sample, but remove the stations impacted by Arlene in the Dennis event-study design. Storms at the beginning and end of our sample may not have a full 45-day pre- and post-window.

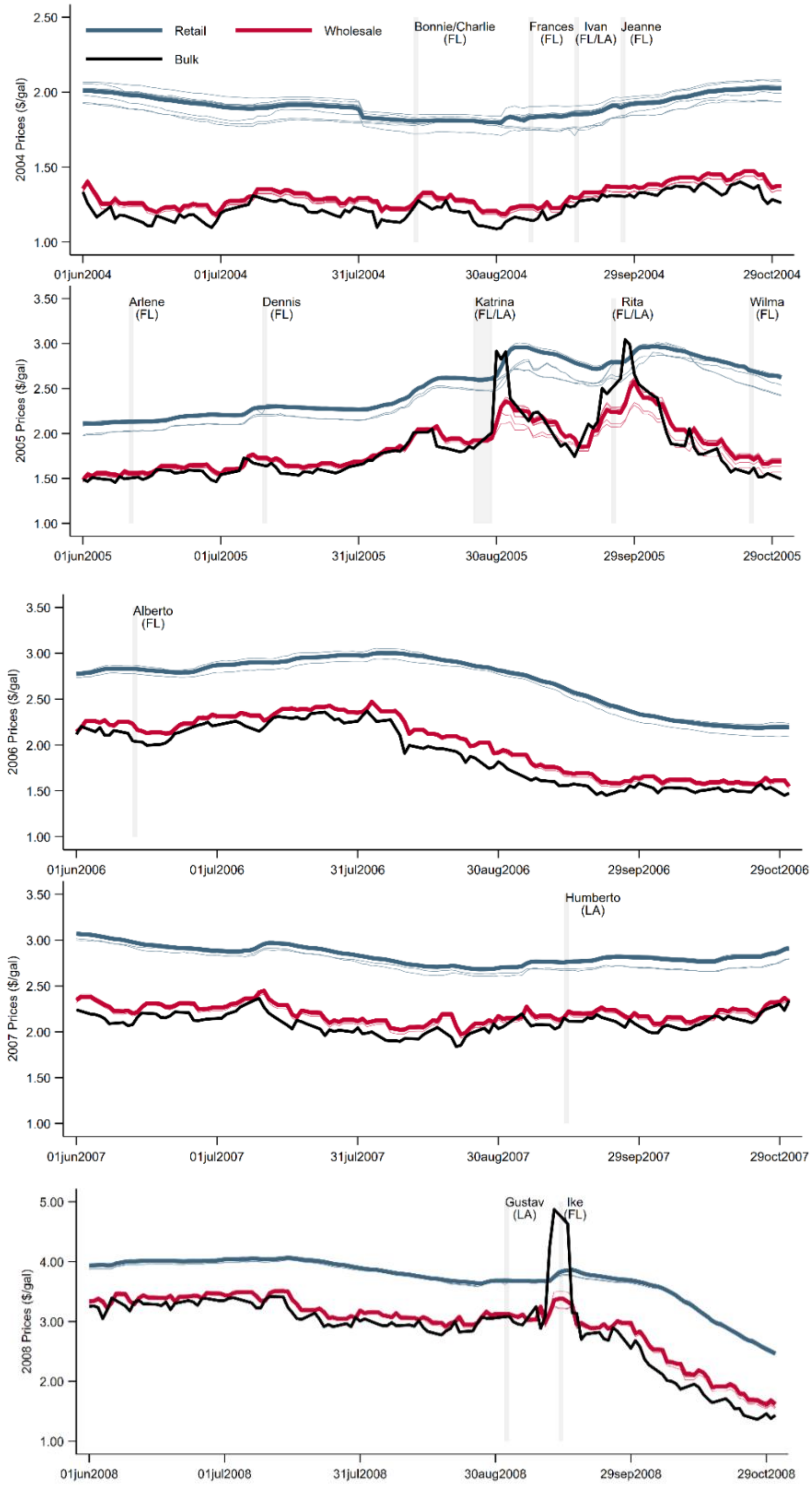


Figure B.1: Average retail, wholesale, and bulk prices.

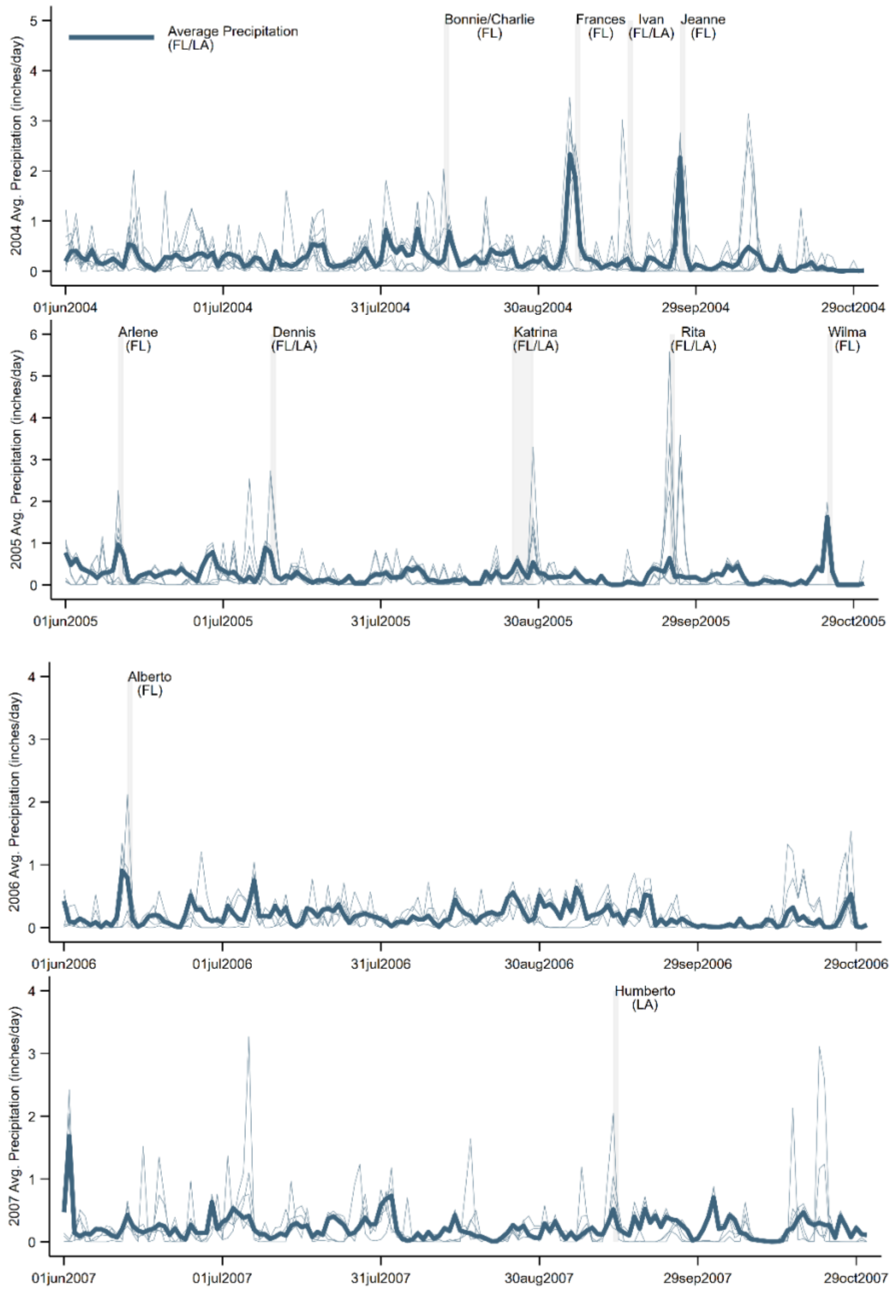


Figure B.2: Average precipitation.

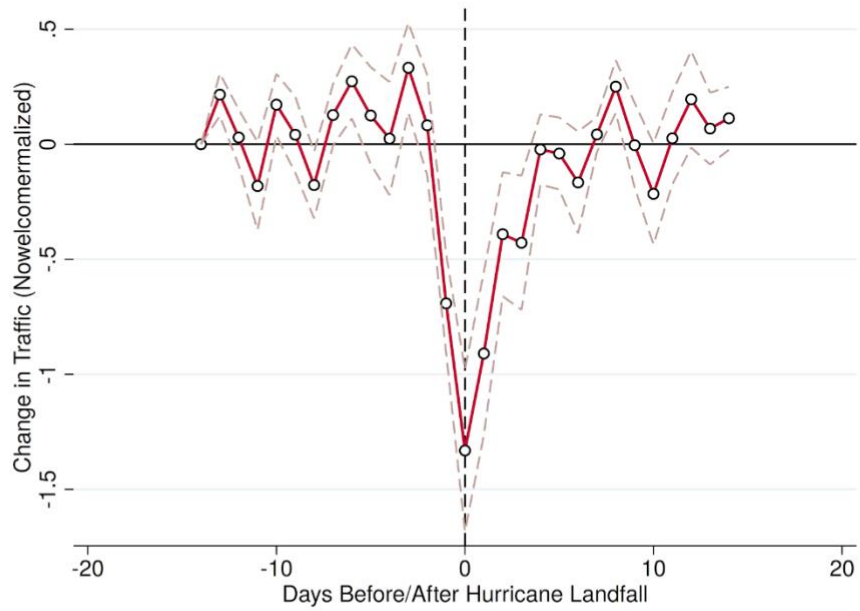


Figure B.3: Normalized Traffic Levels Event Study

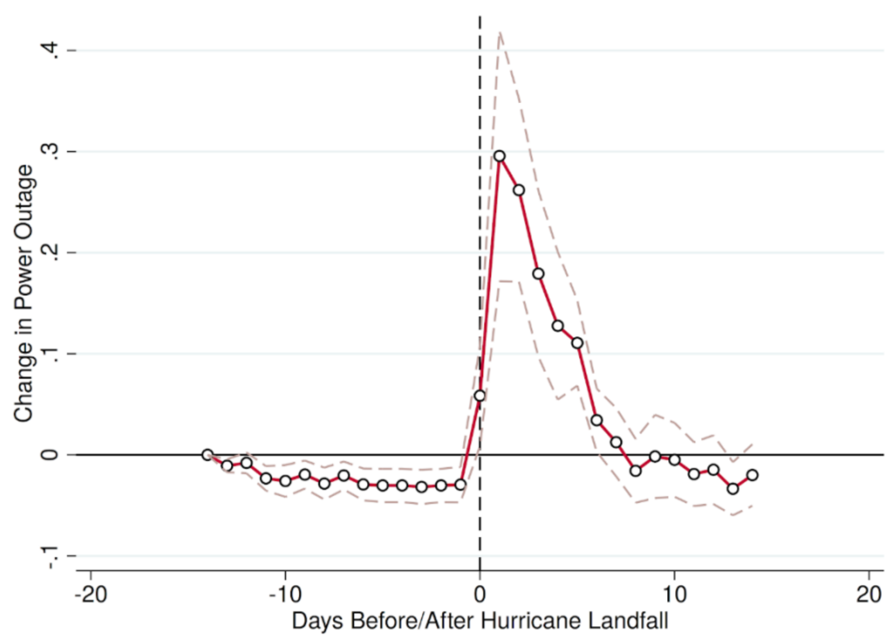
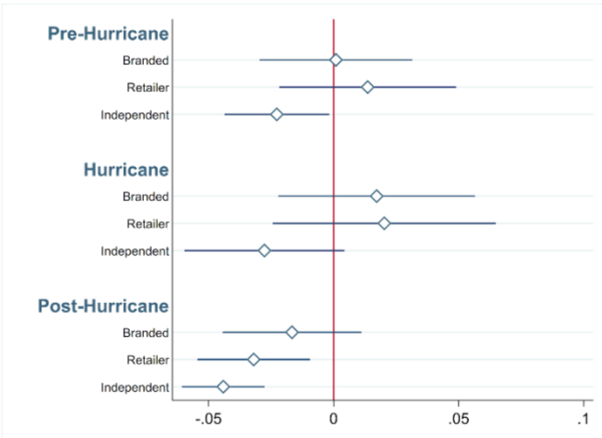
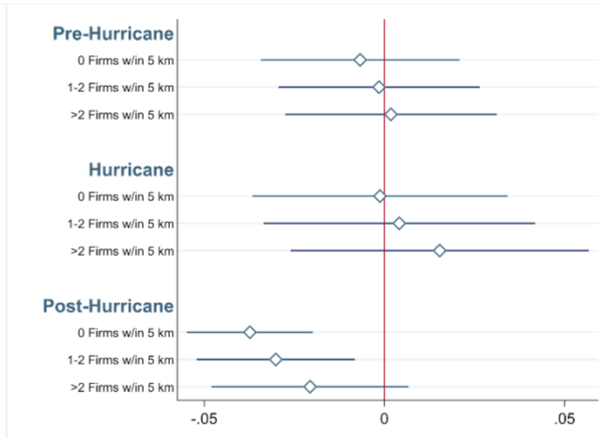


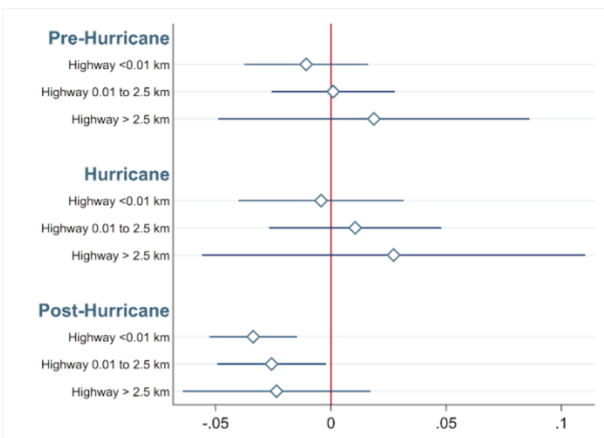
Figure B.4: Power Outage Event Study



(a) Station Ownership

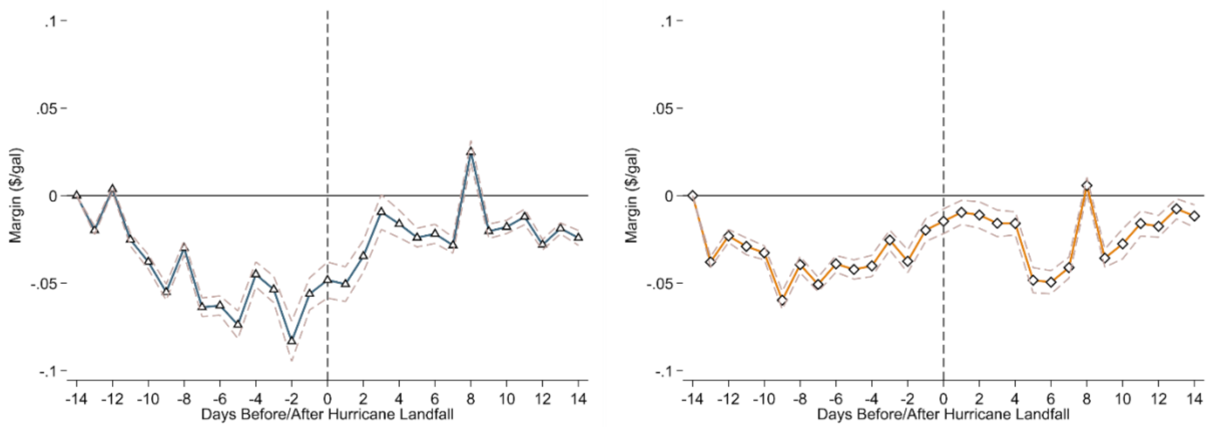


(b) Local Competition



(c) Distance to Highways

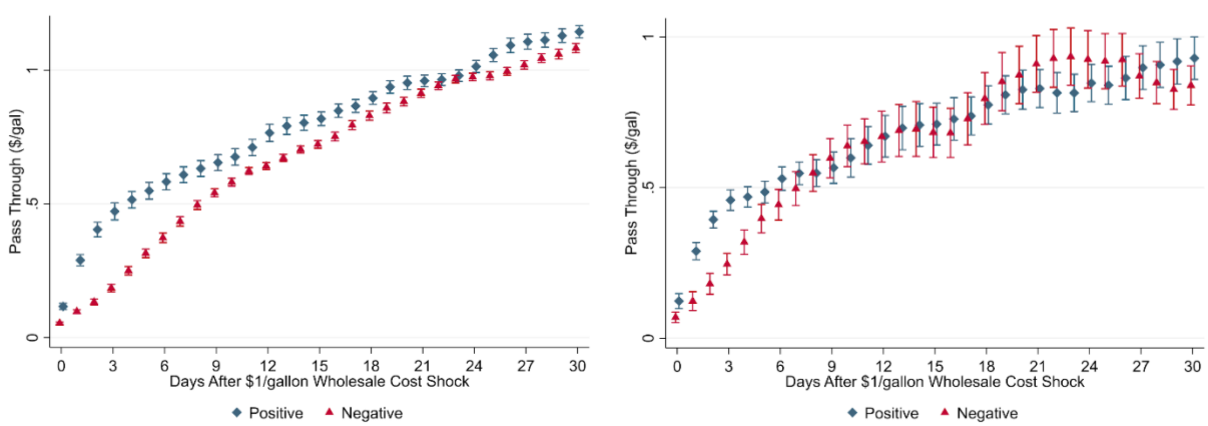
Figure B.5: Treatment Effect Heterogeneity (Difference-in-Differences)



(a) All Hurricanes

(b) All Hurricanes except Katrina, Rita, and Ike

Figure B.6: Wholesale Rack Margins



(a) All Stations, All Periods

(b) Landfall Stations, Hurricane Window

Figure B.7: Asymmetric Cost Pass-Through

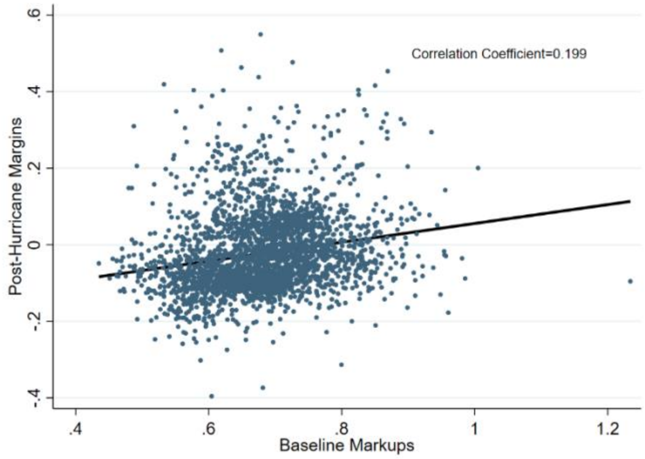
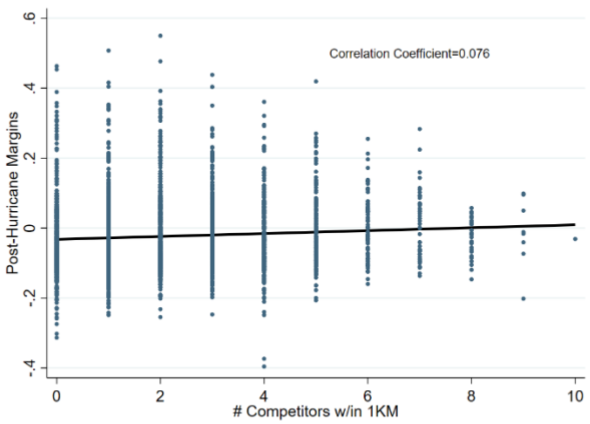
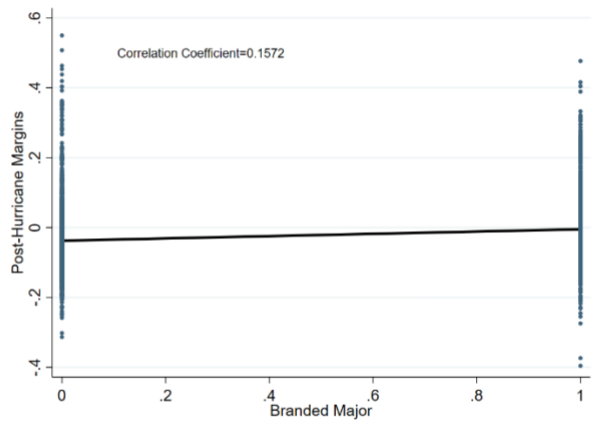


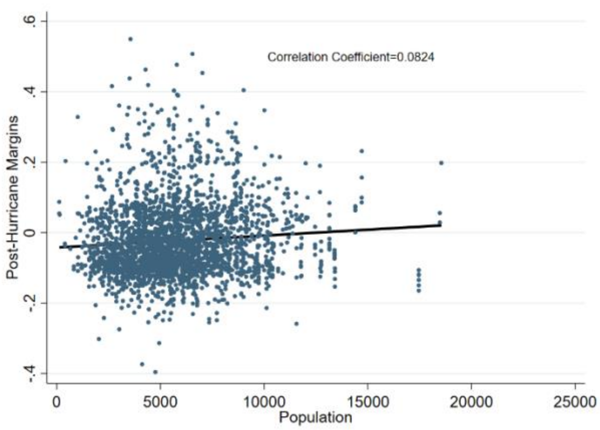
Figure B.8: Station Post-Hurricane Markups and Baseline Markups



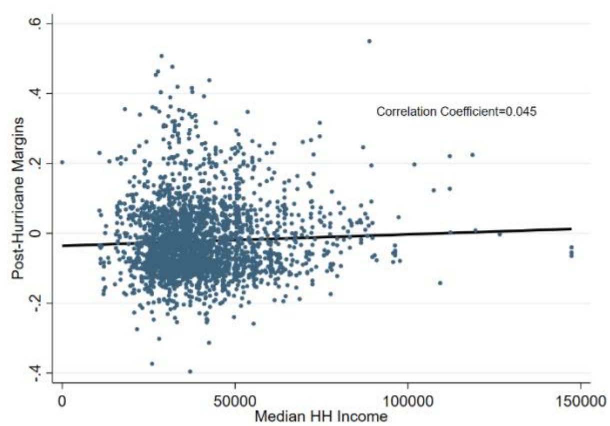
(a) Local Competition



(b) Branded vs. Unbranded Stations



(c) Census Tract Population



(d) Census Tract Median Household Income

Figure B.9: Station Post-Hurricane Markups Local Supply and Demand Conditions

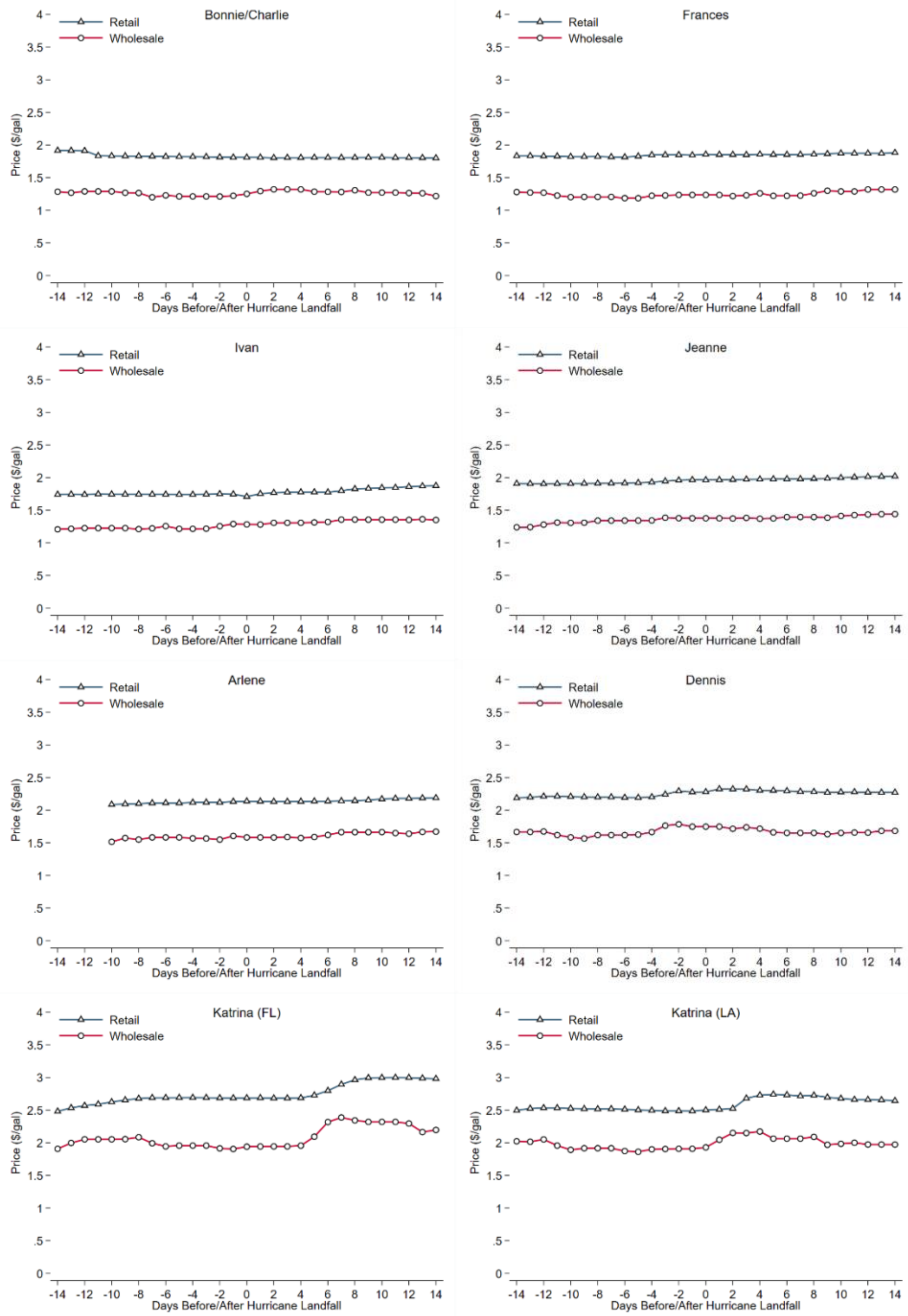


Figure B.10: Hurricane Event Studies: Retail and Wholesale Prices

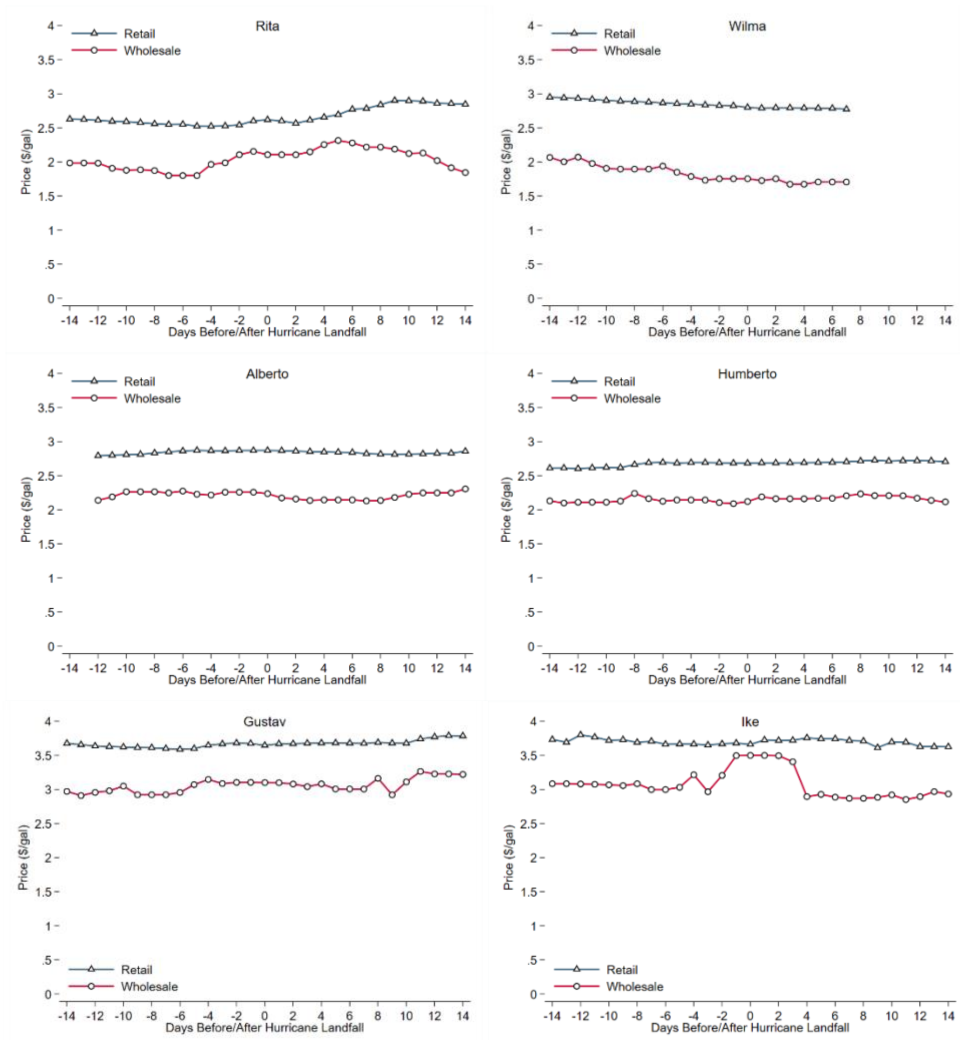
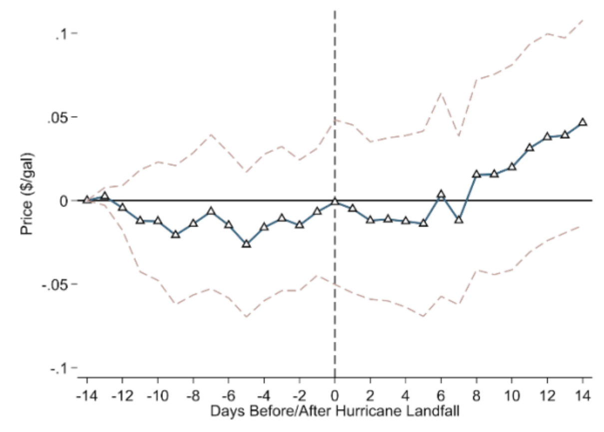
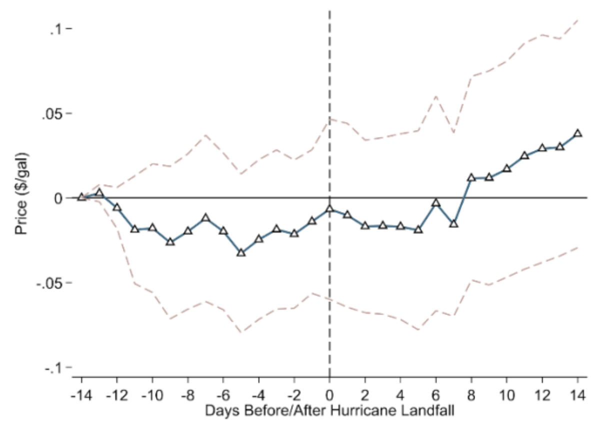


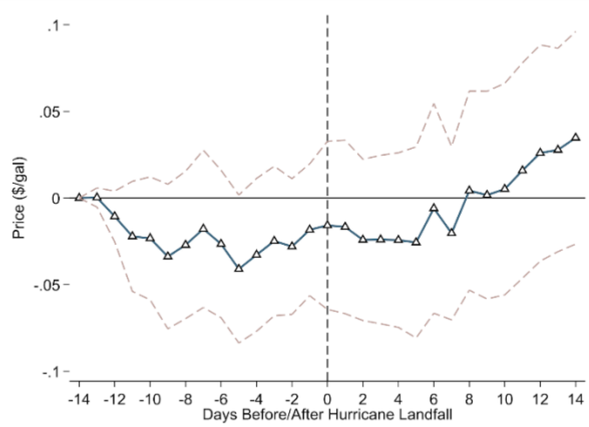
Figure B.10 (cont.): Hurricane Event Studies: Retail and Wholesale Prices



(a) 75% Reporting Sample

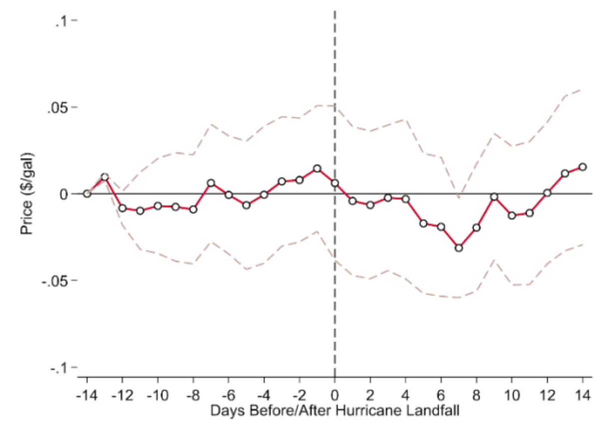


(b) 2004 75% Reporting Sample

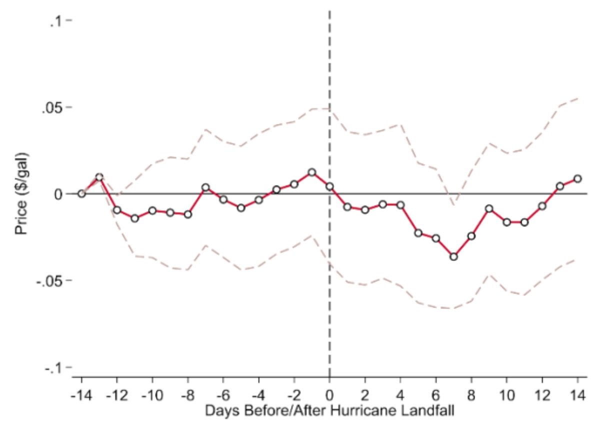


(c) All OPIS stations

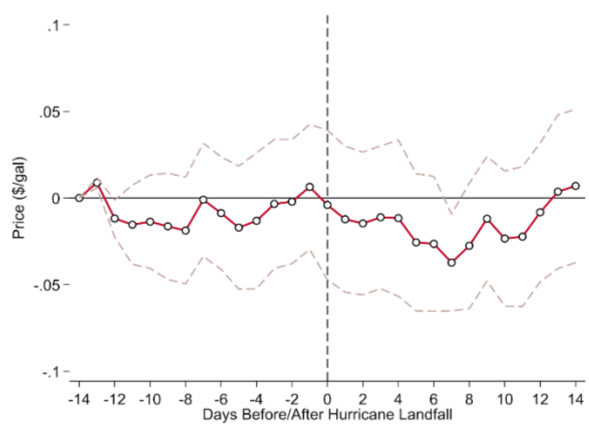
Figure B.11: Price Event Study Sample Sensitivity



(a) 75% Reporting Sample



(b) 2004 75% Reporting Sample



(c) All OPIS stations

Figure B.12: Margin Event Study Sample Sensitivity

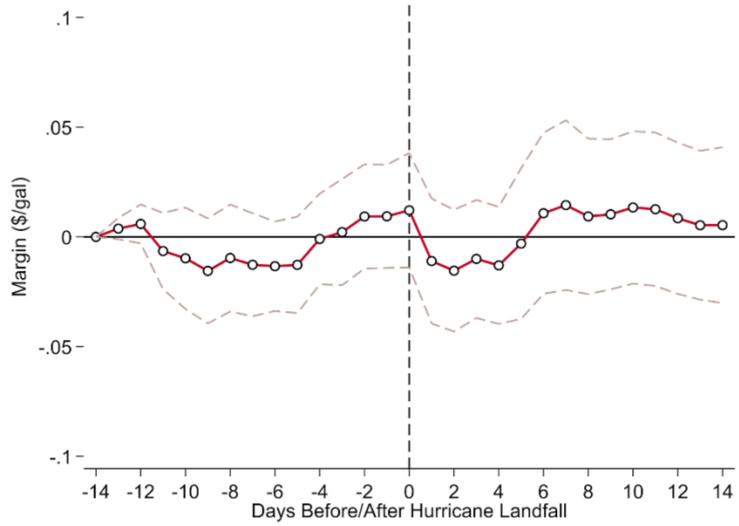
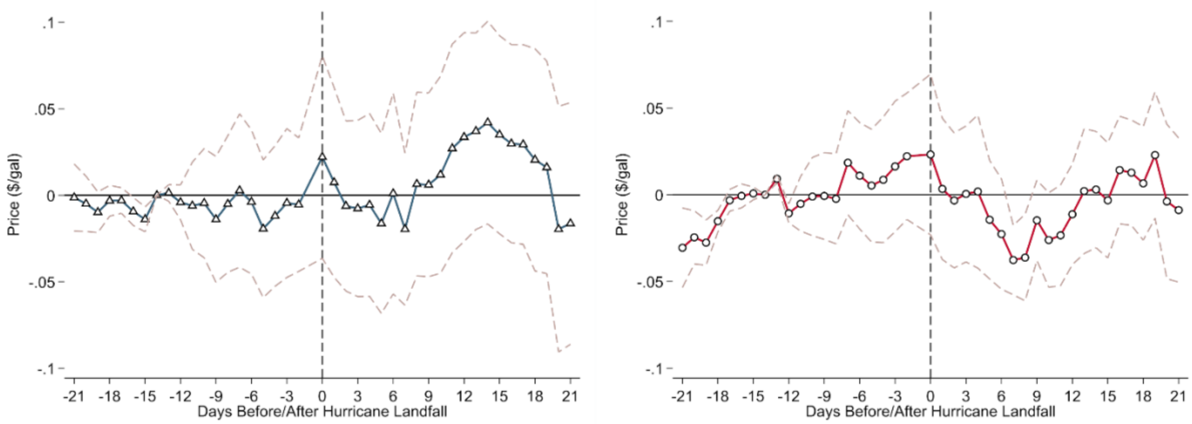


Figure B.13: Margins Event Study with Distributed Lag Wholesale Controls.



(a): Price Impacts *(b): Margin Impacts*

Figure B.14: 21-Day Event Studies

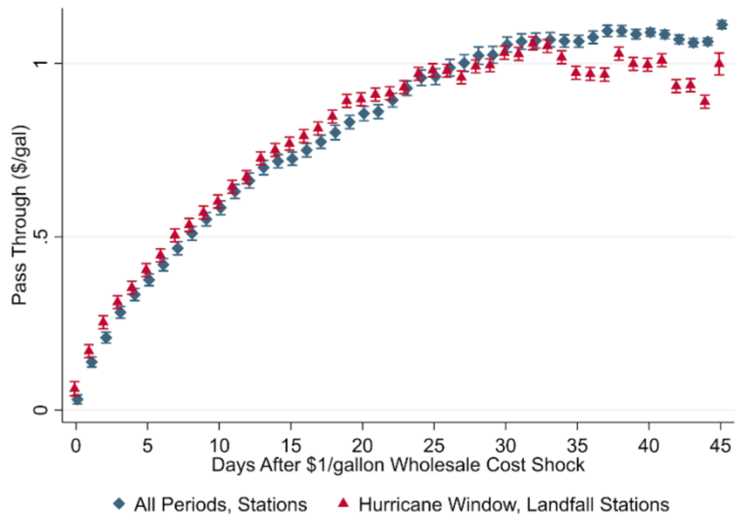


Figure B.15: Extended Wholesale Cost Pass-Through

Table B.1: Station Post-Hurricane Markups and Supply/Demand Factors

	(1)	(2)	(3)	(4)
Branded Major	0.031***	0.007***	0.025***	0.004
	(0.004)	(0.003)	(0.005)	(0.003)
Competitors w/in 1KM	0.004***	0.000	0.004***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Population (1000 people)	0.003***	-0.000	0.004***	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)
Median Income ($10K)	0.002*	0.003***	0.011***	0.001
	(0.001)	(0.001)	(0.002)	(0.001)
Retail Major			-0.000	-0.005
			(0.005)	(0.004)
KM to Rack			0.000***	0.001***
			(0.000)	(0.000)
KM to Highway			0.001	-0.000
			(0.002)	(0.001)
Non-White Population (%)			0.137***	-0.019**
			(0.010)	(0.008)
Associates Degree or Higher (%)			-0.001	0.002
			(0.018)	(0.014)
Unemployment (%)			-0.010	0.012
			(0.158)	(0.071)
Housing (1000 units)			-0.003	0.002
			(0.003)	(0.002)
Vacant Housing (%)			0.001***	-0.000
			(0.000)	(0.000)
Observations	2770	2770	2770	2770
State FE	Yes	No	Yes	No
County FE	No	Yes	No	Yes

Notes: The dependent variable is the estimated station-specific post-hurricane markup. All demographic data are at the Census tract level and are from the 2010 Census. *, **, and *** denote significance at the 10%, 5%, and 1% level.

Table B.2: Average Effect of Hurricanes on Retail and Wholesale Prices and Margins Balanced Panel 1

	(1)	(2)	(3)	(4)
Dep. Var	Retail	Retail	Wholesale	Wholesale
Panel A: No upstream price controls				
Pre-Hurricane	0.053*	0.026	0.027	0.032
	(0.031)	(0.030)	(0.031)	(0.032)
Hurricane	0.060**	0.033	0.040*	0.045*
	(0.029)	(0.028)	(0.021)	(0.023)
Post-Hurricane	0.101**	0.076**	0.129*	0.133**
	(0.040)	(0.037)	(0.061)	(0.063)
Observations	1267615	1267615	1298205	1298205
Stations/Racks	1697	1697	18	18
Panel B: Upstream price controls				
Pre-Hurricane	0.031*	-0.000	0.029*	0.027*
	(0.016)	(0.015)	(0.016)	(0.015)
Hurricane	0.041*	0.010	0.012	0.010
	(0.022)	(0.019)	(0.017)	(0.016)
Post-Hurricane	-0.001	-0.030**	-0.035	-0.036
	(0.015)	(0.012)	(0.027)	(0.026)
Wholesale Price	0.784***	0.785***		
	(0.007)	(0.007)		
Bulk Price			0.721***	0.720***
			(0.005)	(0.004)
Observations	3,196,641	3,196,641	3,574,845	3,574,845
Stations/Racks	4,673	4,673	18	18
State FE	Yes	No	Yes	No
Station/Rack FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Month-of-Year FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes

Table B.3: Average Effect of Hurricanes on Retail and Wholesale Prices and Margins Balanced Panel 2

	(1)	(2)	(3)	(4)
Dep. Var	Retail	Retail	Wholesale	Wholesale
Panel A: No upstream price controls				
Pre-Hurricane	0.044	0.019	0.019	0.022
	(0.034)	(0.033)	(0.034)	(0.036)
Hurricane	0.049	0.023	0.031	0.035
	(0.031)	(0.030)	(0.024)	(0.027)
Post-Hurricane	0.084**	0.061	0.110*	0.112*
	(0.041)	(0.040)	(0.059)	(0.061)
Observations	1981000	1981000	2144295	2144295
Stations/Racks	2803	2803	18	18
Panel B: Upstream price controls				
Pre-Hurricane	0.027*	-0.001	0.026	0.024
	(0.016)	(0.014)	(0.017)	(0.016)
Hurricane	0.035*	0.007	0.014	0.012
	(0.021)	(0.019)	(0.016)	(0.015)
Post-Hurricane	-0.005	-0.030**	-0.030	-0.031
	(0.016)	(0.013)	(0.024)	(0.023)
Wholesale Price	0.785***	0.786***		
	(0.006)	(0.006)		
Bulk Price			0.719***	0.719***
			(0.005)	(0.005)
Observations	1981000	1981000	2144295	2144295
Stations/Racks	2803	2803	18	18
State FE	Yes	No	Yes	No
Station/Rack FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Month-of-Year FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes

Table B.4: Average Effect of Hurricanes on Retail and Wholesale Prices and Margins: Adjusted Standard Errors

	(1)	(2)	(3)	(4)
Dep. Var	Retail	Retail	Wholesale	Wholesale
Panel A: No upstream price controls				
Pre-Hurricane	0.042	0.022	0.017	0.02
	-0.03	-0.029	-0.037	-0.04
	[0.056]	[0.055]	[0.054]	[0.055]
Hurricane	0.05	0.028	0.035	0.038
	-0.03	-0.028	-0.027	-0.03
	[0.053]	[0.051]	[0.044]	[0.045]
Post-Hurricane	0.089**	0.069**	0.114*	0.116*
	-0.035	-0.034	-0.064	-0.066
	[0.048]	[0.046]	[0.066]	[0.069]
Observations	3,196,641	3,196,641	3,574,845	3,574,845
Stations/Racks	4,673	4,673	18	18
Panel B: Upstream price controls				
Pre-Hurricane	0.022*	-0.001	0.028	0.025
	-0.012	-0.01	-0.016	-0.016
	[0.027]	[0.025]	[0.019]	[0.019]
Hurricane	0.033**	0.007	0.009	0.007
	-0.018	-0.015	-0.018	-0.018
	[0.037]	[0.032]	[0.021]	[0.020]
Post-Hurricane	-0.006	-0.028***	-0.033	-0.036
	-0.012	-0.01	-0.026	-0.025
	[0.018]	[0.014]	[0.021]	[0.022]
Wholesale Price	0.785***	0.786***		
	-0.005	-0.004		
	[0.049]	[0.050]		
Bulk Price			0.719***	0.719***
			-0.005	-0.004
			[0.095]	[0.095]
Observations	3,196,641	3,196,641	3,574,845	3,574,845
Stations/Racks	4,673	4,673	18	18
State FE	Yes	No	Yes	No
Station/Rack FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Month-of-Year FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes

Table B.5: Average Effect of Hurricanes on Retail and Wholesale Margins - Lagged Wholesale and Bulk Controls

	(1)	(2)	(3)	(4)
Dep. Var	Retail	Retail	Wholesale	Wholesale
Pre-Hurricane	0.024***	-0.003	0.013	0.011
	(0.009)	(0.006)	(0.018)	(0.018)
Hurricane	0.031***	0.003	0.009	0.007
	(0.010)	(0.007)	(0.013)	(0.014)
Post-Hurricane	0.040***	0.016	-0.010	-0.011
	(0.015)	(0.012)	(0.023)	(0.022)
Wholesale Price (30 days)	1.091***	1.091***		
	(0.005)	(0.005)		
Bulk Price			0.910***	0.910***
			(0.005)	(0.005)
Observations	2556192	2556192	2867745	2867745
Stations/Racks	4663	4663	18	18
State FE	Yes	No	Yes	No
Station/Rack FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Month-of-Year FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes

Table B.6: Average Effect of Hurricanes on Retail and Wholesale Prices and Margins – Stacked Event-by-Event Estimates

	(1)	(2)	(3)	(4)
Dep. Var	Retail	Retail	Wholesale	Wholesale
Panel A: No upstream price controls				
Pre-Hurricane	0.042***	-0.020***	-0.021	-0.028*
	(0.012)	(0.006)	(0.014)	(0.016)
Hurricane	0.047***	-0.013*	-0.038*	-0.044*
	(0.010)	(0.007)	(0.018)	(0.022)
Post-Hurricane	0.064***	0.007	0.056*	0.050*
	(0.017)	(0.015)	(0.030)	(0.027)
Observations	3,194,016	3,194,016	3,194,016	3,194,016
Panel B: Upstream price controls				
Pre-Hurricane	0.057***	0.006	0.015**	0.010**
	(0.011)	(0.008)	(0.007)	(0.004)
Hurricane	0.074***	0.024*	-0.000	-0.005
	(0.015)	(0.013)	(0.009)	(0.007)
Post-Hurricane	0.025**	-0.022**	-0.039*	-0.043*
	(0.012)	(0.009)	(0.021)	(0.021)
Wholesale Price	0.703***	0.700***		
	(0.011)	(0.011)		
Bulk Price			0.561***	0.560***
			(0.011)	(0.011)
Observations	3,194,016	3,194,016	3,194,016	3,194,016
State FE	Yes	No	Yes	No
Station/Rack FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Month-of-Year FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes