The Effects and Effectiveness of Emergency Price Controls During Natural Disasters

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PRELIMINARY DRAFT

Abstract

Anti-price gouging laws, present in most US states, penalize retailers if they make large price increases to disaster supplies during states of emergency. Price caps during periods of high and inelastic demand may worsen or alleviate shortages of essential supplies — they can decrease the incentive to restock supplies by reducing the resale price at which the new inventory sells, or they can increase the incentive to restock supplies by generating more unmet demand at initial inventory levels. We use retailer scanner data and novel trucking data to estimate the effects of US natural disasters on quantities transacted and prices. We estimate that disasters increase demand for a set of 20 essential goods, with increases in mean quantities transacted but also in the probability of having a stockout. Prices increase, with the largest changes concentrated in a minority of retailers, but we see no effects of anti-price gouging laws on the probability of price hikes. On the supply-side, we see limited evidence of marginal cost increases and we estimate a shift in restocking from the disaster period to the week preceding it. Motivated by this evidence, we specify a structural model that will evaluate how stringent enforcement of price caps would affect shortages and consumer surplus during disasters. We construct a non-parametric identification argument that combines an instrument for restocking costs with the observed joint distribution of restocking and quantities transacted to recover latent demand and initial inventory levels.

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1 Introduction

Each year nearly one in three Americans lives in a county affected by a natural disaster, and the vast majority of U.S. residents can expect to experience at least one disaster per decade.[1] These events — which primarily consist of hurricanes, snowstorms, wildfires, and floods — can cause disruptions to electricity, water supplies, and self-reported access to food. While a small number of people evacuate in response to some disasters, the vast majority of affected households experience disasters sheltering in place at home. Yet, despite the widespread presence of disaster risk, many Americans report lacking advance possession of the supplies necessary to withstand such shocks, and especially so among households that also lack cash buffers to manage emergencies (Dinerstein, Lucas, Nath and Rayl, 2025). This underscores the importance of ensuring that policy promotes efficient and equitable market access to emergency supplies during the acute periods when disaster strikes.[2]

In service of ensuring fair access to critical goods, 38 states have implemented anti-price-gouging (APG) laws that prohibit large increases in the retail price of essential supplies during periods of declared emergency. These laws have been the subject of intense public scrutiny and debate. Proponents cite the potential affordability and equity benefits of APG laws, while critics express concerns about their potential to exacerbate shortages. Thus far, however, the discussion about their merits has taken place primarily in newspaper opinion articles and scholarship in philosophy and law, and consists almost entirely of qualitative reasoning.[3] Little evidence documents the dynamics of actual retail sales and prices for key supplies during disasters, or the effects of existing APG laws. Moreover, the debate over these policies lacks grounding in a theoretical framework that formalizes the mechanisms through which a price ceiling could potentially influence welfare in a disaster period setting.

This paper provides comprehensive evidence on the retail market impacts of natural disasters in settings with and without APG laws. We combine scanner data on retail

[1] In the ten years from 2014 to 2023, counties covering 89% of the U.S. population experienced at least one disaster declared by the Federal Emergency Management Agency.

[2] Dinerstein, Lucas, Nath and Rayl (2025) also show that the direct public provision of disaster supplies by FEMA and other agencies is small relative to private market transactions.

[3] See, for example, Brewer (2006), Zwolinski (2008), Snyder (2009), Sorkin (2017), New York Times Editorial Board (2017), Economist (2020).

transactions with novel trucking data, data on natural disaster, and APG laws. We use an event study design to estimate the effects of natural disasters on quantities transacted, stockouts, retail prices, trucking costs, and wholesale prices. We conduct a thorough qualitative review of enforcement of APGs and argue that these laws are generally not enforced. We then predict the welfare consequences of enforcement by specifying a structural model of retailer price-setting and reordering of inventories and propose a novel identification argument.

The event study analysis estimates the effects of natural disasters occurring in the US from 2006-2020 on retail outcomes for 20 essential disaster supplies. We assume the timing and location of federally declared disasters are exogeneous with respect to non-disaster shocks that retailers face. We first examine how quantities transacted change. We find large increases in quantities transacted across the full range of products, but especially large (24%-88%) in durable goods like batteries, bottled water, and flashlights. This increase is consistent with a large increase in demand for these products during disaster periods. But we also estimate an increase in stores selling zero units of a disaster good in a given week. This effect, especially strong for perishables like fresh eggs, bread, and milk, likely reflects an increase in stocking out as baseline inventory levels are not sufficient to meet the increase in demand.

The increase in quantity transacted with a corresponding increase in stockouts might imply large profit incentives for a retailer to raise prices. We estimate that average price increases are small across most goods; our confidence intervals rule out price changes above 1% for most products. This average effect hides considerable heterogeneity, as we see a small set of retailers increasing prices by over 50%. For flashlights, the probability of a 50% price increase goes up by 3.5 percentage points during a disaster. We also see strong effects for fresh eggs. Thus, while our estimates reject widespread price gouging, consistent with existing literature (Gagnon and López-Salido, 2019; Beatty et al., 2021), when we characterize the full distribution we find evidence of some extreme price increases.

Despite these large price increases, we find minimal evidence that APG laws are enforced. We conduct a systematic qualitative analysis of APG law enforcement and find no recorded instances of legal action against retailers under APG laws, despite the prevalence of price increases in our data. We further compare across states with and without APG laws and within the three states that have law changes during our sample and find minimal evidence that pricing behavior during disasters changes with laws.

One reason laws may not bind is that many include exceptions for passing on large cost increases. Using wholesale PromoData prices, we find no effects of natural disasters on the prices of wholesalers located in the county of disaster. We further investigate whether trucking costs, which may be more sensitive to disaster conditions, change with disasters by collecting start-end-point trucking data from Echo Global Logistics for 1% of all trucking in the US. We estimate modest 5-6% disaster-induced increases in the price of a trucking trip. Because wholesale costs comprise 70-80% of grocery store costs, and transportation is a subset of the remaining 20-30%, we conclude that our observed transportation cost shocks should generate small average retail price hikes under competitive pricing regimes (Montgomery 1997). The extreme cases of doubling prices or more are very unlikely the simple result of cost pass-through.

Given the lack of enforcement of current laws, our event study analysis does not estimate the effects binding price caps would have on consumer welfare. Instead, we turn to a structural model of retailer behavior to predict the consequences of enforced laws. We model retailers as setting prices, possibly subject to price caps, and choosing whether to reorder supplies at a fixed cost if existing inventory is insufficient to meet demand. We show that price caps can decrease or increase retailers' incentives to reorder supplies. On the one hand, price caps limit what retailers can charge on new units, which reduces the incentive to incur the ordering cost. On the other hand, price caps lead to more unmet demand at initial inventory levels, which increases the incentive to incur the ordering cost. The direction of the supply response to instituting a price cap depends on initial inventory levels and the reordering fixed cost. We further show that the effects of price caps on consumer surplus are theoretically ambiguous and that the optimal price cap can be non-monotonic in initial inventory levels and reordering costs.

This theoretical ambiguity leads us to estimate the model with our data. Our identification challenge is that predictions depend on factors that are rarely observed in data. For retailer marginal cost and reordering costs, we measure them directly with our data on wholesale and trucking prices, respectively. But demand at equilibrium prices and initial inventories, both crucial to characterizing the effects of laws, remain partially unobserved. For example, initial inventories are only observed when there is a stockout, but our model shows that stockouts are less likely when initial inventory is particularly high (there is enough inventory to meet demand) or low (so much unmet demand leads the retailer to order additional supplies). We construct a non-parametric partial identification argument based on one instrument – shifting the ordering cost and now demand or initial inventories – and partially observing orders, from the trucking data. As ordering costs fall, transacted quantities in our data become more likely to reflect demand (because the retailer reordered supplies) than initial inventory. Linking the cost decrease to the increased reordering probability traces out the joint distribution of demand and inventories. Future drafts will estimate the model and conduct such counterfactual exercises.

Related Literature

A small and growing literature explores pricing during natural disasters from both theoretical and empirical angles. While we suppose price gouging is as defined in US regulation and evaluate it's effects under this definition, Kominers and Dworczak (2025) propose an economic definition of price gouging based: situations in competitive markets where lowering the price from the market-clearing level would increase total welfare. The parameters of existing APG laws can be motivated by such a notion of price gouging since the laws may generate valuable redistribution.

Empirical evidence on prices during natural disasters and emergencies measures price changes and consumer reactions to price hikes. Studying two particularly large events, the 2010 earthquake in Chile and the 2011 earthquake in Japan, Cavallo, Cavallo and Rigobon (2014) find large and persistent drops in the number of products available for sale at supermarkets despite stable prices over several months. In the setting of Florida and Louisiana gasoline markets, Beatty, Lade and Shimshack (2021) find very similar pricing behavior – a large majority of stores are compliant with APG laws and a small right tail of potential violations. Using store level price and quantity indices of all products, Gagnon and López-Salido (2019) find little to no evidence of price increases during episodes of high demand, including snowstorms and hurricanes in the US. Holz, Durán and Laguna-Müggenburg (2022) uses an experimental setting to argue that retailers may face reputational costs from increasing prices during emergencies.

We develop a model of retail supply during natural disasters. The prevalence of stockouts means that our model fits in a latent choice and consideration set literature (Goeree, 2008). In the absence of direct measures of choice sets (Dinerstein, Einav, Levin and

Sundaresan, 2018), researchers have appealed to a variety of identification arguments (Abaluck and Adams-Prassl, 2021). In a broad class of latent choice set models, Agarwal and Somaini (2022) show that identification requires an instrument for utility and an instrument for availability. We instead construct an argument based on one instrument and observing endogenous actions to increase availability (orders). Our model provides the structure relating orders to the latent variables.

Finally, our work is part of a broader literature on the economics of natural disasters, reviewed by Botzen, Deschenes and Sanders (2019). We use this literature in particular to motivate our investigation of the distributional consequences of retail pricing, supply, and demand. Closely related to our work, Beatty, Shimshack and Volpe (2019) study sales of batteries, flashlights, and bottled water before and during hurricanes, finding that pre-landfall sales are higher in coastal, wealthier, and whiter areas, while ex post purchases are higher in predominantly Black, lower income, and less educated areas.

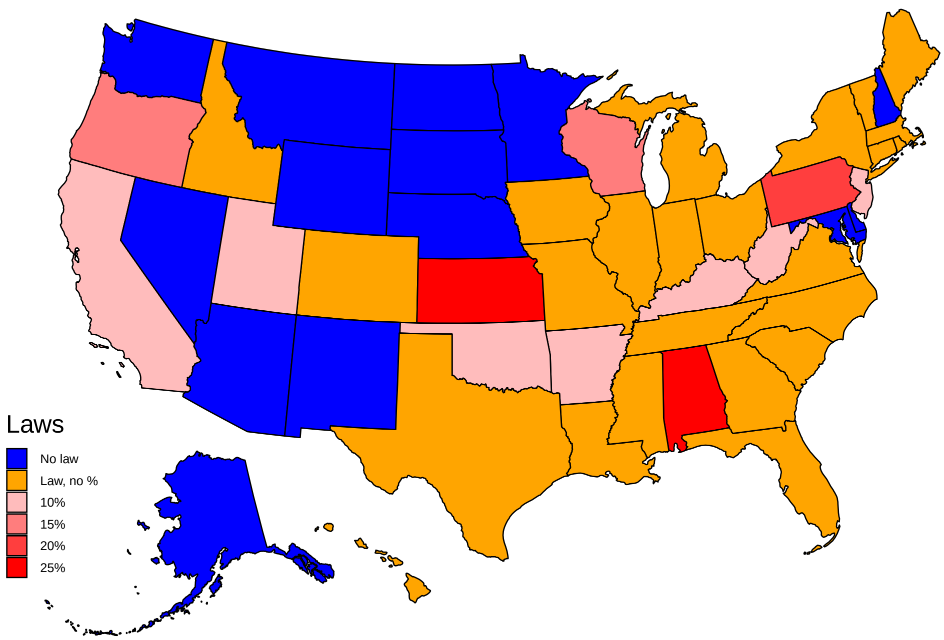
2 Empirical Approach

2.1 Background on Anti-Price-Gouging Laws

We start by compiling a comprehensive list of state-level anti-price-gouging (APG) laws in the U.S. Thirty-eight US states have laws restricting large price increases on essential goods like food, bottled water, first aid supplies, and fuel during natural disasters. During the COVID-19 pandemic, many of the remaining 12 states took temporary action prohibiting retail price increases. Our focus is on price regulations that apply to supplies essential to cope with local and shorter-term natural disasters, as opposed to worldwide and longer-term shocks, so we focus analysis on the pre-COVID period. Four of these pre-COVID laws apply only to fuel, leaving 34 states with laws pertaining to the disaster supplies in the data we describe below.

Anti-price-gouging laws are broadly similar in language and scope, and go into effect when state governors declare a state of emergency, as is common during natural disasters. They do, however, vary substantially across states in both how they define violations and the penalties applied to violators. Out of the 34 states with pre-pandemic laws, 12 specify a magnitude of prohibited price increase, which ranges from 10 to 25 percent. The other 22 states use qualitative language to describe what constitutes price gouging, using words such as "unconscionable" to define a violation. In addition, 20 of the states with APG laws explicitly exempt price increases that can be justified by an increase in costs, thereby implicitly defining price gouging as an increase in markups rather than just an increase in prices. Illegal price gouging is a civil offense punishable with fines in all states with laws, with 9 of the state laws also including the further possibility of facing criminal charges and prison time.

Figure 1: State Level Anti-Price-Gouging Laws as of 2019



Notes: Map displays the anti-price gouging laws across the US as of 2019. States in blue have no APG law. States in orange have an APG law which defines price gouging qualitatively. States in shades of red have APG laws with quantitative definitions of price gouging. The percentage represents the threshold of price increase above which retailers violate the law.

Figure 1 maps the laws and their specified thresholds for disallowed price increases. Laws are especially common in eastern seaboard hurricane states, and states without laws are especially clustered in the west and the northern Great Plains region. Many states also passed their APG laws in the period after large natural disasters, underscoring the types of selection that complicate the interpretation of comparisons across states with and without laws. Further, critically for the empirical estimates in this paper, all but three states — Hawaii, Oregon, and Rhode Island — passed their laws prior to 2006 when the data on retail prices and quantities that we describe below is first available.

2.2 Data

Natural Disasters

We compile a list of all federal disaster declarations from FEMA for the years 2006-2022. This data records events where one or more states makes a request for federal disaster assistance.[4] Notably, the Stafford Act of 1988 that governs this process generally requires that the state governor declares a state of emergency to apply for federal aid, which is also the standard trigger for state anti-price-gouging laws to go into effect.

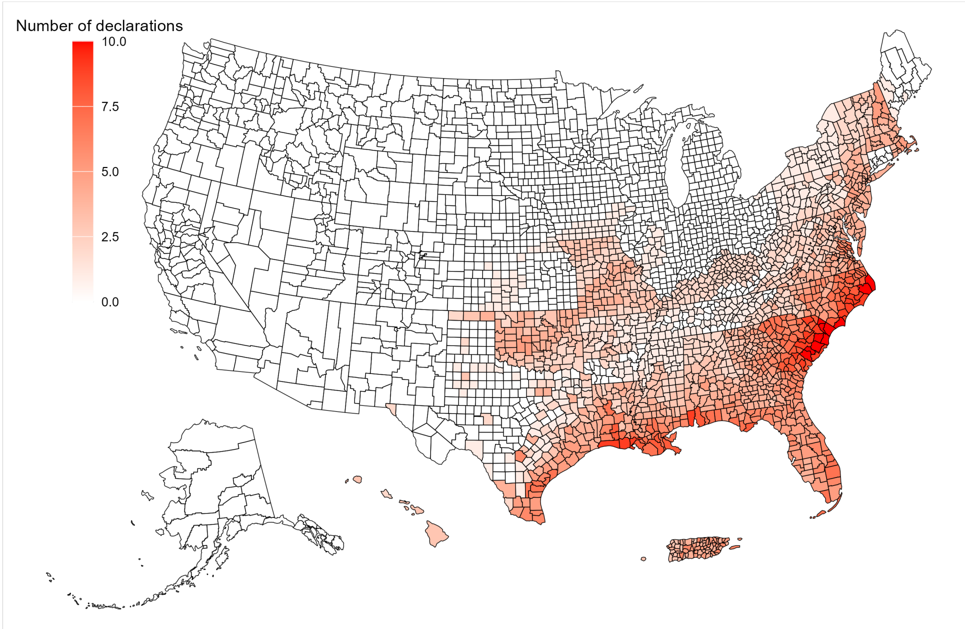
The data records all possible categories of disasters including hurricanes, severe thunderstorms, snowstorms, tornadoes, earthquakes, wildfires, and floods, as well as a very small number of manmade events such as terrorist attacks and chemical accidents. We focus the analysis on hurricanes, severe storms, and severe snow and ice storms, which we label as "key" disasters. These are the events for which the market for disaster supplies is most relevant, as households typically have an opportunity to prepare in advance for a period of time that will require sheltering in place at home, often in the presence of disruptions to infrastructure supplying electricity and water. In contrast, earthquakes and tornadoes occur suddenly without warning and end within minutes, and fires and floods typically require evacuation from affected areas. For completeness, we control for these other disaster types in all specifications and show results for them separately in the appendix.

Figure 2 shows a count of these key disasters across states from 2006-2019. In total, there are 183 state by disaster declarations for the included disaster types during the sample period. Some states have as few as 1 disaster-level storms, while the range extends to a maximum of 16 in Florida. The map also shows that neighboring states can have very different frequencies of disaster declarations — e.g. Georgia has more than twice the number of disasters as South Carolina. This illustrates that the threshold for choosing to declare a disaster and triggers a price-gouging law may differ across states, in addition to the laws themselves.

In order to isolate the effects of particularly severe events, we also collect information

[4] In rare cases, such as terrorist attacks, the President can also unilaterally declare a disaster without a state request.

Figure 2: U.S. Counties by Total Key Disaster Count (2006-2019)



Notes: Map displays the count of disaster declarations in each county between 2006 and 2019. Included disaster types are hurricanes, severe storms, and severe snow and ice storms.

on the top 10 most severe hurricanes that occurred in the U.S. during our sample period. We define severity both by total estimated damages and by the number of deaths, each of which leads to the same set of top 10 costliest events. The hurricanes in this sample are Florence, Gustav, Harvey, Ike, Irma, Isaac, Laura, Matthew, Michael, and Sandy.[5] Estimates for the top ten hurricanes will be included in a subsequent draft of the paper.

Retail Sales and Prices

We use retail scanner data from NielsenIQ for the period from 2006 to 2019. We observe weekly prices and units sold per item for over 60,000 stores nationwide for over 140,000 Universal Product Codes (UPCs) across twenty product categories. We choose products based on the items recommended by FEMA as disaster supplies. In the main text, we show results for nine product categories: three types of durable disaster supplies (flashlights, batteries, and bottled water); three types of storable food (canned beans, canned soup, and canned fruit); and three types of perishable staples (fresh milk, fresh eggs, and baked bread). In the appendix, we also show results for a range of related products: toilet

[5]Hurricanes Marco and Laura occurred in so close proximity and time that they were not differentiated in the data.

tissue, paper towels, matches, first aid treatments, cellular phones, diapers, baby wipes, pasta, dry beans, and peas, lentils, and corn.

For each product category, we calculate weekly prices at the store-week-product level by dividing the sale amount by the number of units, following the literature. We aggregate from the UPC to the category level weighting each UPC by its *annual*, rather than weekly, quantity sold. This measure of average prices holds constant the composition of varieties sold within a product category, so that measured prices capture weekly changes within UPC codes rather than shifts in consumption across UPCs that may be induced by a disaster. Altogether, there are over 700 million store-by-week observations across the 20 product categories. The identity of each retailer in the data is anonymous, but the NielsenIQ data does contain a chain identifier that links establishments operated by the same firm.

Consumer Panel

We also use NielsenIQ's longitudinal scanner data of shopping trips at the household level. This data contains household demographics, the date of each shopping trip that ended in at least one purchase, and the products, quantities, and prices of each purchase on each trip. The store codes in the consumer panel data match those in the retail scanner data such that it is possible to match individual households to the establishments at which they shop.

Wholesale Prices

We use weekly wholesale price data for 12 U.S. grocery providers from the years 2006-2012 from National Promotion Reports, LLC ("PromoData").[6] We follow the same procedure as with the retail scanner to aggregate from the UPC to the product category level.

Trucking Shipments

To further investigate the cost shocks facing retailers during disasters, we use data on trucking shipments from 2017-2021 obtained from Echo Global Logistics, a supply chain and transportation management firm that brokers trades between shippers and carriers. The data contains detailed trip-level information for a sample that approaches 1% of all U.S. trucking by the end of the period. For each trip, the data records the origin and

[6] We no longer have access to this data through our current contract, but present results estimated under a prior data use agreement.

destination location, the number of miles traveled, the start and end time of the trip, whether arrival was considered to be delayed, and the price of the transaction. The data also records the type of truck being used, which indicates most notably whether or not the products were refrigerated. Altogether, this data allows us to observe how disasters affect the total number of trips, the total price, the cost of each mile traveled, the duration of each trip, the price per minute, and the composition of origin locations serving a disaster region.

2.3 Event Study Approach

Our primary empirical analysis uses an event study design to measure the effects of disasters on retail prices and quantities from the NielsenIQ scanner data. To construct the data for this analysis, we match the retail sales data to disasters at the county-by-week level. County is the highest resolution location information available in the NielsenIQ data, and also the geographic scale at which disaster declarations are assigned. In terms of timing, FEMA records the exact date of disaster declarations, but the retail scanner data is only collected at a weekly level. Further, the exact week in the NielsenIQ data during which the disaster occurs cannot be assigned with perfect precision. While NielsenIQ lists data for weeks measured from Sunday to Saturday, the actual days constituting a reporting week vary unobservably across establishments. For instance, some stores count sales on a Tuesday to Monday basis, in which case the weekly sales data listed on Saturday will be for the week that ended four days prior. Thus, the true potential window of sales in the data is over a 13-day period. We assign disaster onset to the week comprised of the seven days prior to NielsenIQ's Saturday reporting date, but note that some post-disaster sales will be assigned to the week prior to the disaster in the data.

With the merged disaster and scanner data, we estimate a panel regression for outcome y in store j and week t:

$$y_{jt} = \sum_{\tau=-4}^{8} \beta_\tau 1\{\text{disaster}_{jd}, t = d + \tau\} + \theta_j + \alpha_t + X_{jt} + \varepsilon_{jt}. \tag{1}$$

Store-week level indicator $\mathbb{1}\{\text{disaster}_{jd}, t = d + \tau\}$ indicates a store-week belonging to the 13-week period surrounding a disaster which arrives in week d in store j's county. Coefficients β_τ measure the difference in outcome y during disaster weeks relative to non-disaster weeks. Controls X_{jt} are store-week indicators capturing the presence of other types of disasters non included in our set of "key" disaster types.

The week in which $\tau = 0$ is when the disaster occurs, subject to the measurement error described above. We measure pre-trends for four weeks prior to the disaster and the post-disaster effects for eight weeks afterwards. Note that there is no clear definition of when a disaster period "ends" as market disruptions may last well beyond the dates during which the physical hazards remain in effect. The specification controls for store and week fixed effects that control for fixed characteristics of a given establishment and seasonality and time trends at the national level. Thus, the implied control group is comprised of all store-week observations that are not within the sample window. We show robustness of the main results to region-by-week fixed effects, but note that some major disasters — particularly the top-10 hurricanes — affect most parts of a broad region, leaving no clear unaffected control group in those specifications.

In some specifications below, we examine heterogeneity by interacting the main disaster effects, β_τ, with characteristics of a given store or location. We also use similar specifications to Equation 1 to estimate the effects of disasters on truck shipments to disaster regions. We describe the details of the approach to that portion of the analysis more in Section 4.2.

3 Event Study Analysis

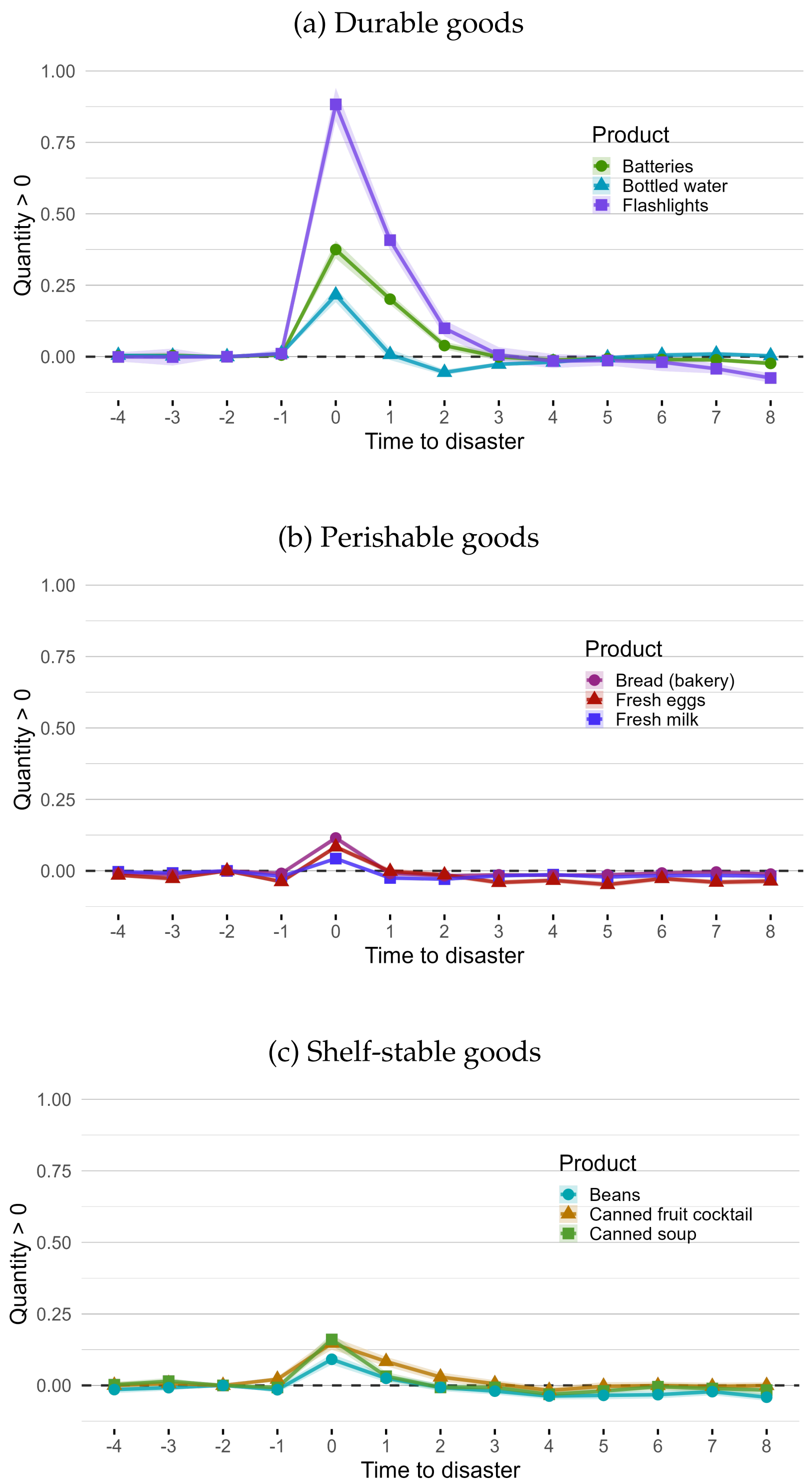
3.1 The Effects of Natural Disasters on the Market for Critical Supplies

We estimate the specification from Equation 1 on a series of dependent variables to show the effects of natural disasters on the retail market for disaster supplies. We summarize the key takeaways from each component of this analysis in bold.

Quantities: Retailers experience a large increase in contemporaneous sales of a wide range of disaster supplies.

Figure 3 shows the effects of our key natural disasters — hurricanes, severe snowstorms, and severe thunderstorms — on the log of quantity of sales of the main set of products. To separate the effects of shortages from the increase in demand for disaster supplies, we condition on the quantity sold being greater than zero in this specification. The results show large increases in sales for all product categories displayed in the graphs.

Figure 3: Effects of Disasters on Quantity Sold (Conditional on $Q > 0$)



Notes: Figure plots the estimates of coefficients $\hat{\beta}_\tau$ from Equation 1, with a y variable of quantities transacted. Corresponding standard errors are in the shaded areas. Estimates reflect the change in the quantities transacted during the 12 weeks around natural disasters relative to non-disaster periods. The estimating sample conditions on store-weeks with non-zero sales to eliminate measuring sales during weeks with potential stockouts or store closures.

The largest spikes are for the durable goods: flashlights, batteries, and bottled water see excess sales of 88%, 37%, and 24% respectively in the week a disaster hits, relative to the predicted value from store and week fixed effects. Sales of these durable products also remain elevated the week following a disaster, consistent with the threat of lasting power outages reported in Dinerstein, Lucas, Nath and Rayl (2025).

Sales of perishable and non-perishable foods also show substantial, though smaller, increases in the week of a disaster. Bread, eggs, milk, beans, canned fruit, and canned soup all 5-15% increases in sales during a disaster week, with some effects in the subsequent week for the canned goods. The concentration of sales increases in the precise week of a disaster is perhaps surprising given the measurement complications regarding timing described in Section 2.3. For most disaster supplies we consider, and especially for fresh foods, the disaster-induced increase in sales is highly concentrated during the week in which the disaster occurs.

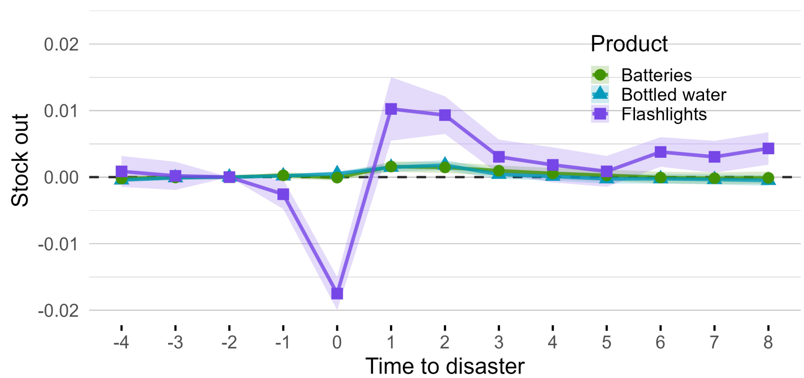
Stockouts: A small share of stores record zero sales for some goods over entire weeks following a natural disaster, suggesting that they experience lengthy stockouts.

Figure 4 shows the effects of disasters on zero sales at the weekly level. Note that this measure constitutes only a coarse measure of shortages. If supply is temporarily interrupted at a given location but available at the beginning or end of a given week, the data will not record a zero. To make the measure more accurately targeted at shortages, we exclude stores from this analysis that record a zero in at least 10% of all weeks, to avoid including small stores with infrequent sales of some products for which a week of zero sales is not likely to be associated with a supply disruption. Despite this sample restriction, we still find that the frequency of zero sales is lower than expected during a disaster week for flashlights, which have the largest increase in sales of the products in our data and may be sold infrequently in many locations.

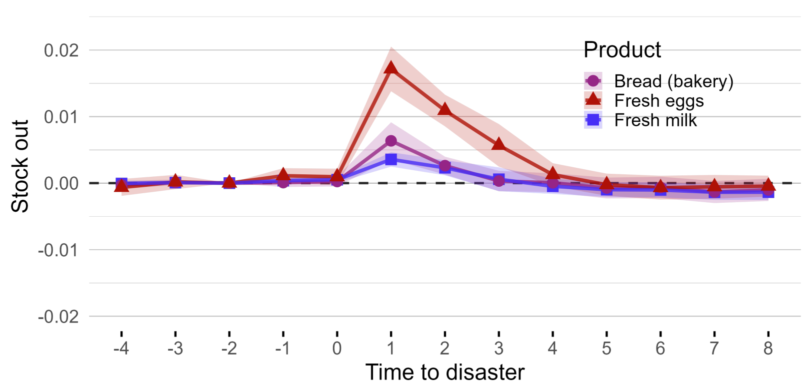
Overall, the results in Figure 4 show a meaningful frequency in excess weekly zero sales observations in the first and second week following a disaster for some products. Fresh eggs are the product most affected by shortages, with close to 1 in 50 stores experiencing a full week of zero sales the week following a disaster, and some prevalence of weekly zeros lasting for three post-disaster weeks. The incidence of zero weekly zero sales is about half as high for bread, and slightly lower than that for fresh milk. The rest

Figure 4: Effects of Disasters on Zero Weekly Sales Observations

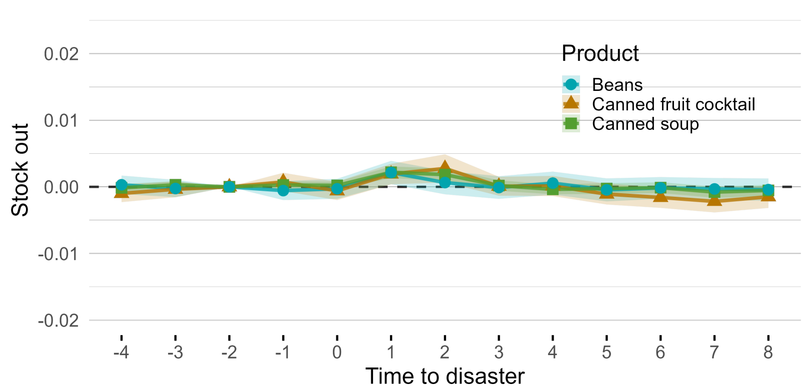
(a) Durable goods



(b) Perishable goods



(c) Shelf-stable goods



Notes: Figure plots the estimates of coefficients $\hat{\beta}_\tau$ from Equation 1, with a y variable of an indicator for a store making 0 transactions of a product category in the week. Corresponding standard errors are in the shaded areas. Estimates reflect the change in the probability of a store making zero sales in a week during the 12 weeks around natural disasters, relative to non-disaster periods.

of the products show in the graphs — beans, canned fruit, canned soup, batteries, and flashlights — all have statistically significant increases in zero sales the week after a disaster as well, though the magnitudes are close to ten times smaller than for fresh eggs. Flashlights are an unusual case, with a decline in zero sales during disaster weeks, and then an increase affecting about 1 in 100 stores in the two weeks following a disaster.

While we cannot observe stockouts at the daily or hourly level that may affect many consumers, we interpret the results in Figure 4 as evidence that shortages play a meaningful role in the post-disaster period. A weekly stockout is an extreme case in this context, and their measurable prevalence along with anecdotal evidence from news reports suggests that shorter-term stockouts may pose a serious consideration for consumers in post-disaster periods. It is also perhaps notable that the rate of weekly shortages are by far the highest for fresh food products for which inventories cannot be stored for prolonged periods and need to be re-ordered more frequently.

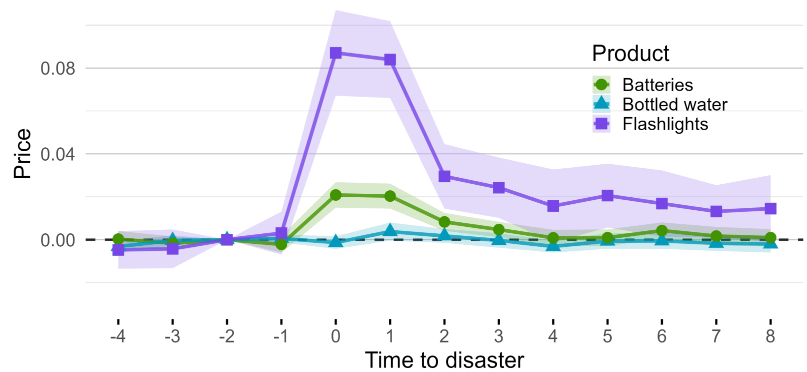
Prices: The average change in retail prices during disaster periods is small to negligible for most disaster supplies, but a small share of retailers raise prices on some disaster supplies dramatically.

Figure 5 shows the effects of disasters on the log of average retail prices by product. The price of flashlights rises by over 8% in the week of a disaster and the week following, and remains elevated by 2-3% points throughout the eight week post-disaster period. The average price of batteries is about 2% higher in weeks 0 and 1. Other than these two products, however, there is little evidence of price changes. The average price increase is not statistically different from zero for any of the other products except for canned soup, which sees about a 1% price increase. For the remaining products, the confidence intervals can rule out even a 1% change in average prices during the week of the disaster.

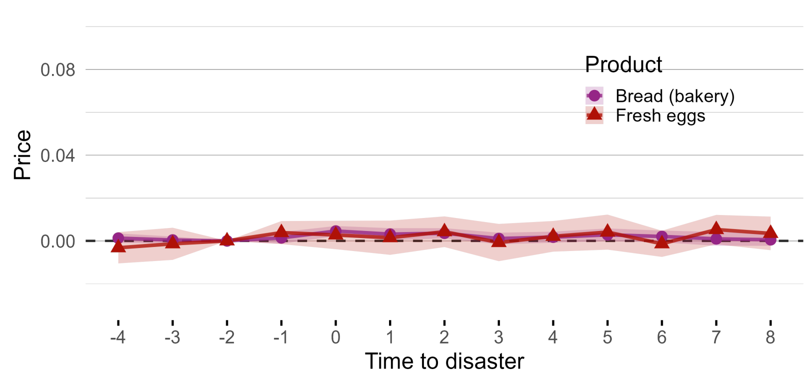
The small change in average price masks substantial heterogeneity, however. Figure 6 shows the effects of disasters on the probability of a 50% price increase relative to any of the previous twelve weeks, as compared to non-disaster periods. For flashlights, more than 1 in 30 stores raises prices by at least 50% during the week of a disaster, with diminishing shares continuing to do so in the subsequent weeks. The rate of such large price increases is also elevated to varying degrees in the weeks following disasters for bottled water, canned soup, bread, batteries, and especially fresh eggs. As with flashlights, about

Figure 5: Effects of Disasters on Retail Prices

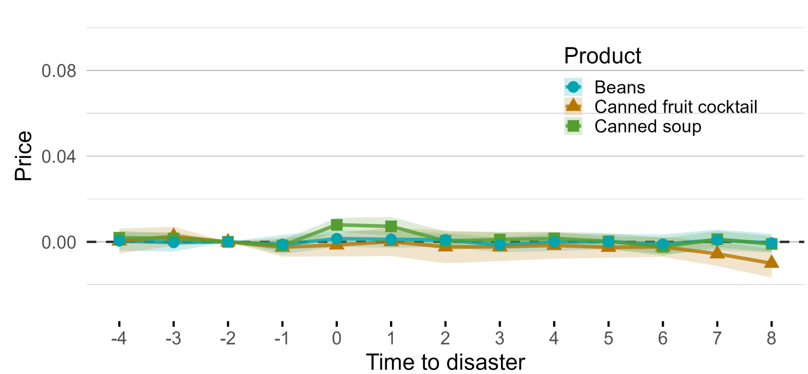
(a) Durable goods



(b) Perishable goods



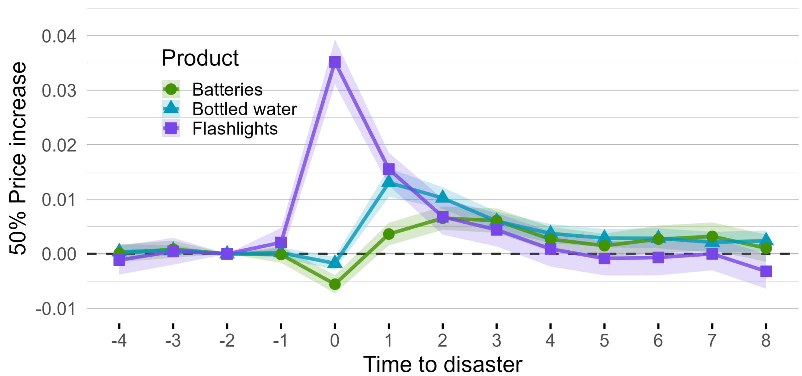
(c) Shelf-stable goods



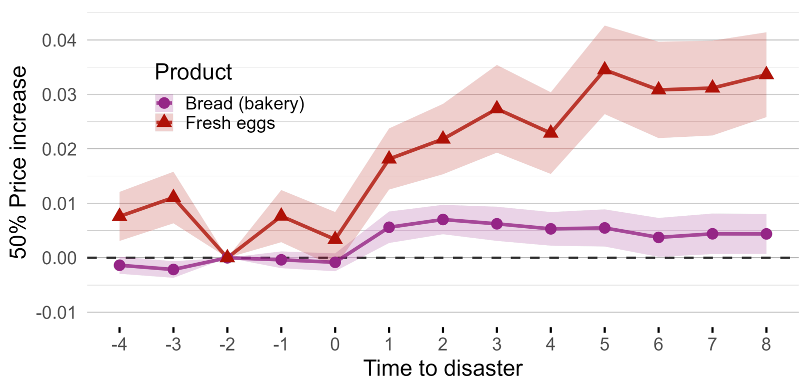
Notes: Figure plots the estimates of coefficients $\hat{\beta}_\tau$ from Equation 1, with a y variable of product category price. Corresponding standard errors are in the shaded areas. Estimates reflect the change in the produce category price in a week during the 12 weeks around natural disasters, relative to non-disaster periods. Product category prices are a weighted average of UPC prices within the product category, weighted by long-run market shares.

Figure 6: Effects of Disasters on Probability of Large (50%) Price Increases

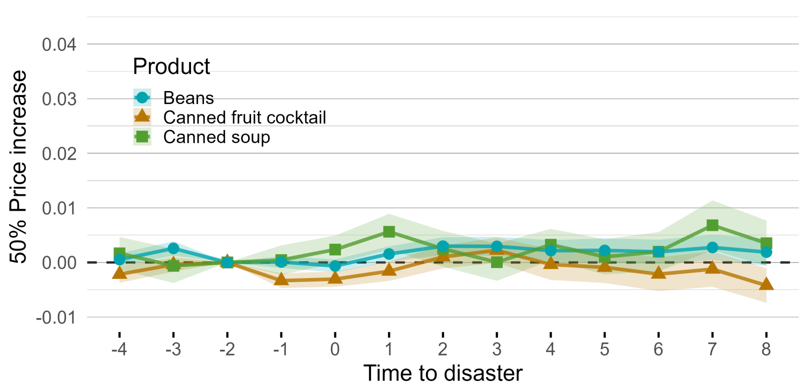
(a) Durable goods



(b) Perishable goods



(c) Shelf-stable goods



Notes: Figure plots the estimates of coefficients $\hat{\beta}_\tau$ from Equation 1, with a y variable of an indicator for a store increasing the product category price by 50% over any price in the previous 12 weeks. Corresponding standard errors are in the shaded areas. Estimates reflect the change in the probability of a 50% price increase during the 12 weeks around natural disasters, relative to non-disaster periods.

1 in 30 stores are raising the price of eggs by at least 50% several weeks after a disaster. Interestingly, most of these products do not see an excess of large price increases during the week of a disaster, but rather in the weeks that follow when quantities generally return to normal levels (Figure 3). In the Appendix, we will show that the incidence of even larger price increases of 100% to 500% occurs for some products such as eggs, bread, milk, first aid treatments, and bottled water. Altogether, the event study estimates show that average prices do not respond much to natural disasters, but a small proportion of retailers react by raising prices dramatically.

3.2 Heterogeneity in natural disaster effects

We examine a variety of dimensions of heterogeneity of the results presented in Section 3.1. In future drafts of the paper we will present each of these results in the Appendix. For now we summarize them qualitatively.

First, we examine the types of regions in which sales and prices respond the most to natural disasters by interacting the event time coefficients from Equation 1 with county-level demographic information from the U.S. Census and American Community Survey. We find little evidence that large price changes occur disproportionately in counties with any particular demographic group, and moderate evidence that sales of disaster supplies — especially durables like batteries and flashlights — respond more in counties with a greater share of minority residents, consistent with what Beatty, Shimshack and Volpe (2019) show for ex post purchases.

Second, we consider the characteristics of retailers that engage in large price changes during disaster. We test for evidence of three hypotheses in the data. First, we examine whether independently owned establishments or those that are part of smaller chains have more freedom to raise prices in response to local shocks than those in larger chains. Second, we consider whether stores are more likely to raise prices on private-label products produced by the same firm for which they may have more control over pricing. Third, we assess whether stores with more nearby competitors may be less likely to raise prices due to the competition they face. We find no compelling evidence for any of these dimensions of heterogeneity.

We do, however, find that establishments within the same chain appear to follow similar disaster period pricing policies. While there is no correlation between chain size and the likelihood of disaster period price increases, the (anonymized) chain identifier is a strong predictor of whether a given establishment will raise prices during a disaster. This is consistent with broader evidence of common chain-wide pricing behavior (DellaVigna and Gentzkow, 2019), with a particular application to changes in prices during distinct disaster periods that hit different establishment within a given chain at different times. This evidence leads us to examine whether the predicted price increase at a given establishment based on the chain-level disaster period average is associated with the quantity sold at those establishments. We find a strong positive relationship between predicted prices and sales for batteries and flashlights, and little association for other products. For flashlights and batteries, every 1% increase in the predicted price change during the week of a disaster raises sales in the subsequent week by 3% and 5.5%, respectively.

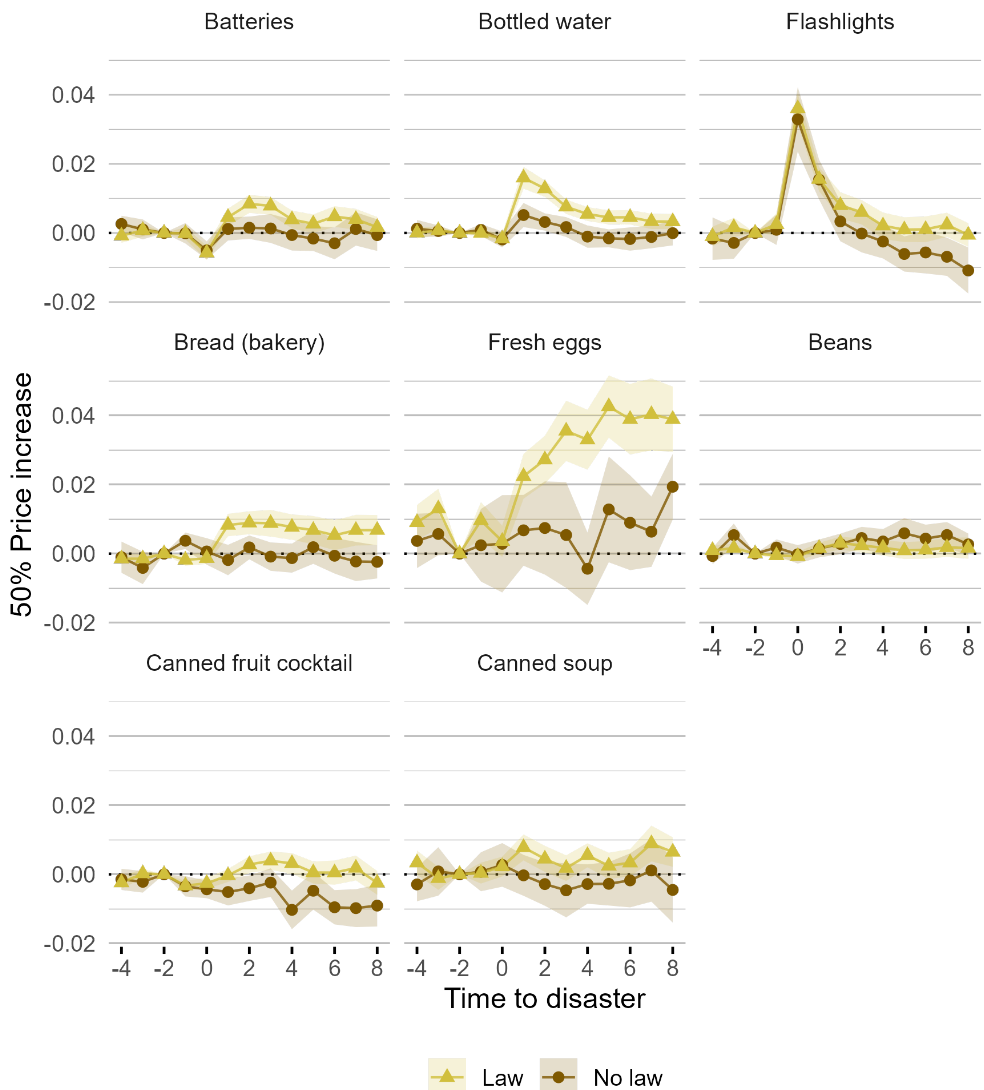
4 Empirical Evidence on Anti-Price Gouging Laws

4.1 Effects of Existing APG Laws

We next consider the effects of existing APG laws. As discussed in Section 2.1, there are a variety of challenges to identifying the effects of these laws. States appear to select into passage of APG laws based on natural disaster exposure, seem to differ in their proclivity to declare an emergency during a given disaster event, and nearly all passed their laws prior to the sample period in the data. In light of this, we present four pieces of separate suggestive evidence on the effects of APG laws that we interpret collectively to assess their effects: a qualitative review of penalties imposed under existing laws, a cross-sectional comparison of pricing behavior across states with and without laws, plausibly causal evidence on the impact of new laws in Hawaii, Oregon, and Rhode Island, and a count of price increases that exceed the specific threshold in the law. Altogether, this collage of evidence strongly suggests that existing APG laws do not bind for retailers selling the disaster supplies in our sample.

Our first component of evidence is a qualitative review of existing enforcement of APG laws. In general, states rely on consumer complaints to alert them of possible violations, and have the resources to investigate only a small subset of such tips (Bae, 2011). For example, in the two weeks after landfall of Hurricane Ian in the fall of 2022, the Florida Attorney's General Office received over 1,300 complaints of potential price gouging and

Figure 7: Probability of Large (50%) Price Increases in States With and Without APG Laws



Notes: Figure plots the estimates of coefficients $\hat{\beta}_\tau$ (dark brown) and $\hat{\beta}_\tau + \hat{\delta}_\tau$ (light brown) from Equation 2, with a y variable of an indicator for a store making a 50% price increase over prices in the previous 12 weeks. Corresponding standard errors are in the shaded areas. Estimates reflect the change in the probability of a store making large price increases during the 12 weeks around natural disasters, relative to non-disaster periods, separately for states with and without APG laws.

followed through with rebates for 100 customers (Rohrer, 2022). Other occasional reports document settlements involving compensation to a small number of consumers, such as for six gas stations after Hurricane Harvey (Paxton, 2019). However, a systematic LexisNexis keyword search for all litigation pursued under anti-price-gouging laws reveals no prosecutions of retail goods sellers prior to 2020. While a small number of cases (8 in total) involving gas stations went to court, the legal records do not show a single instance of a retailer selling disaster supplies of the type in our data facing official penalties, fines, or criminal charges under APG laws.

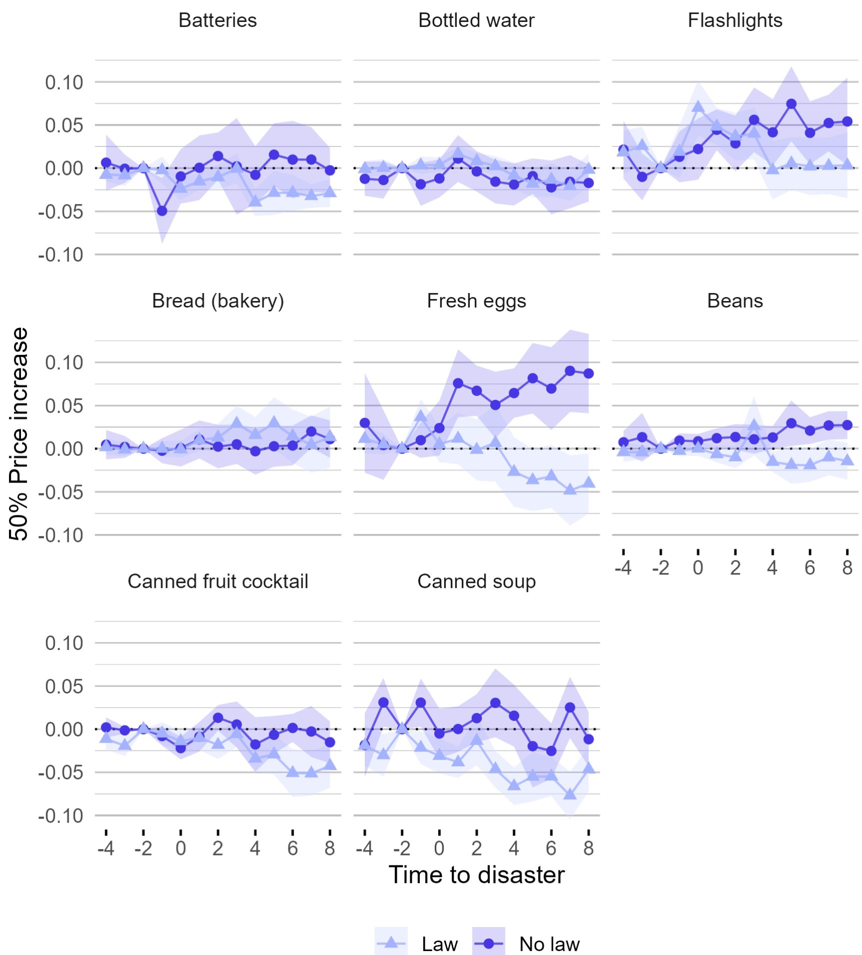
The second component of evidence about price-gouging laws is a cross-sectional comparison of the propensity of stores to enact large price increases during disasters in states with and without laws. In particular, we estimate a version of Equation 1 in which we allow the coefficients on the disaster period indicators to differ between states with and without APG laws currently in effect. The dependent variable is the probability of a 50% price increase relative to any price in the previous twelve weeks. The specification is as follows, where disaster$_{jd}$ is abbreviated as D_{jd}:

$$y_{jt} = \sum_{\tau=-4}^{8} \beta_j \mathbb{1}\{D_{jd}, t = d + \tau\} + \sum_{\tau=-4}^{8} \delta_\tau \mathbb{1}\{D_{jd}, t = \tau\} \mathbb{1}\{\text{Law}_{jd}\} + \lambda_s + \theta_i + \alpha_t + X_{jt} + \varepsilon_{jt} \tag{2}$$

Variation in laws is primarily cross-sectional based on the presence of laws across states. A small subset of law variation is temporal, arising from the three states which passed laws during our sample period. We begin with evidence combining both sources of variation, and then isolate temporal variation by restricting to the three states.

Figure 7 displays the results from the cross-state comparison. The yellow lines show the estimates for β from Equation 2 — the uninteracted effects in states with no laws. The brown lines show the sum of β and δ for the effect in states with the laws. The graph shows that, if anything, retailers in states with laws are *more* likely to impose large price increases during disaster weeks. For flashlights, the product with the most frequent large price increases, the coefficients are very similar in states with and without laws. For bottled water, bread, batteries, and especially fresh eggs, retailers in states with laws are substantially more likely to raise prices by at least 50%. For flashlights and eggs, about one in every 25 retailers is raising prices by at least 50% during disasters in states with APG laws, relative to the baseline frequency of large price changes in non-disaster periods. Recall that 12 of the 34 states in the sample specify a threshold of disallowed price increases, all of which are less than or equal to 25%. We will return to this point below.

Figure 8: Probability of Large (50%) Price Increases Before and After Law Passage in Hawaii, Oregon, & Rhode Island



Notes: Figure plots the estimates of coefficients $\hat{\beta}_\tau$ (dark blue) and $\hat{\beta}_\tau + \hat{\delta}_\tau$ (light blue) from Equation 2, with a y variable of an indicator for a store making 0 transactions of a product category in the week, estimated for Hawaii, Oregon and Rhode Island. Variation in APG laws is temporal since these three states passed new laws during our sample period. Estimates reflect the change in the probability of a store making zero sales in a week during the 12 weeks around natural disasters, relative to non-disaster periods.

The third component of evidence re-estimates Equation 2 for the subsample of states with time-series variation in laws within the sample: Hawaii, Oregon, and Rhode Island. For these three states, a comparison of retailer pricing behavior before and after the passage of laws can be interpreted as a difference-in-difference estimate of the effects of APG laws on large price increases. Estimates reflect the causal effect of APG laws on price increases during disasters to the extent that the timing of law-passage is unrelated to unobserved changes in retail pricing behavior during natural disasters. Figure 8 displays the results. The dark blue lines show the estimates of β, which are the effects of disasters on 50% price increases in these three states prior to the passage of laws. The light blue lines show the estimates of $\beta + \delta$, which are the effects in HI, OR, and RI after the passage of laws. In general, the graphs do not show meaningful evidence of the effects of APG laws. While the incidence of large price increases on fresh eggs falls after the passage of the law, the opposite is true for price increases on flashlights in the week of a disaster. We caution that these results are for only three states, none of which experiences frequent disasters, but interpret them as further suggestive evidence that existing APG laws do not meaningfully affect pricing decisions.

The fourth component of evidence tallies possible violations of APG laws in the data. We consider a store to have potentially violated an APG law if any price in weeks 0-3 of a disaster is greater than the $\tau = -4$ pre-disaster price by an amount that exceeds the threshold specified in that state as illegal. Column 1 of Table 1 displays the results. Overall, we document 253,134 instances between 2006-2019 in which a store's disaster period price increase exceeds the allowed amount in the 12 states that list specific thresholds, with fresh eggs, canned soup, batteries, flashlights, and first aid treatments leading the count. Column 2 of Table 1 displays the share of *all* store by week observations for which an APG law is violated. At least 20% of store-weeks technically violate an APG law for the full set of included products. For batteries, flashlights, and first aid treatments, the share of store-weeks in violation of a law is over 40%, 50%, and 60%, respectively.

The raw data count of price increases may reflect underlying price volatility rather than disaster-specific pricing behavior. Stores may offer temporary discounts or vary prices in response to temporal shifts or seasonality in demand. To correct for this, Column 3 of Table 1 uses the coefficients and residuals from estimating Equation 1 for the effects of disasters on prices in the 12 states that specify a disallowed threshold in their APG laws. A store enters the count of being violation when the coefficient for price increase plus that store's residual ($\hat{\beta}_\tau + \hat{\varepsilon}_{jt}$) exceeds the given threshold in that state. This approach corrects for the baseline level of price volatility by removing estimated store and week

Table 1: Possible Violations of Anti-Price-Gouging Laws

Product	Raw count	Raw data share	Regression share
Cellular Phone	12,683	0.297	0.279
First Aid Treatments	30,065	0.614	0.408
Flashlights	25,470	0.513	0.402
Batteries	20,658	0.406	0.170
Paper Towels	14,771	0.294	0.131
Matches	12,832	0.313	0.238
Toilet Tissue	13,098	0.261	0.107
Fresh Eggs	18,454	0.390	-
Bakery Bread	10,200	0.218	0.120
Fresh Milk	15,311	0.310	0.196
Bottled Water	13,303	0.261	0.170
Peas, Lentils, and Corn	6,786	0.318	0.123
Dry Beans	6,808	0.250	0.086
Canned Soup	19,108	0.382	0.187
Canned Fruit Cocktail	8,463	0.201	0.116
Canned Beans	10,721	0.226	0.157
Pasta	14,403	0.298	-

Notes: This table displays three measures of possible APG law violations in states with explicit legal thresholds. The raw count measure counts store-by-disaster instances where there was an increase in the price of a product exceeding the legal threshold in the three weeks after a natural disaster (compared to the price four weeks prior to the disaster). The raw data share presents the number as a share of the total store-by-disaster instances in the universe of state-years with a legal price gouging threshold. The regression share identifies analogous violations by adding coefficients from the key disaster log price event studies to their residuals ($\hat{\beta}_\tau + \hat{\varepsilon}_{jt}$)and comparing them to the legal threshold in the three weeks after a disaster. It divides resulting violations by the same denominator as the raw data share.
Legal thresholds: WV, UT, OK, CA, KY, NJ, and AR prohibit price increases over 10%. OR, WI prohibit increases over 15%, PA over 20%, and AL, KS over 25%.

fixed effects, and counts only the excess frequency of price increases during disasters. Column 3 shows that the share of store-week observations in violation of the laws remains high, though it is smaller than the proportion in Column 2 that does not correct for the frequency of non-disaster price increases. For flashlights and first aid treatments, over 40% of store-weeks are raising prices by more than the specified amount as compared with the rate in a non-disaster period. For other products, the adjusted share of violations is less than half as large as the raw count of violations, suggesting that price changes of the specified magnitudes are fairly common even in non-disaster periods.

Overall, the circumstantial evidence suggests that APG laws are frequently violated with no consequence to retailers. Table 1 shows that a large share of disaster store-week observations in the sample — even a majority for some products — are technically in violation of their state's APG laws. This is perhaps surprising given the evidence in Section 3.1 that average prices move little for many of these product categories. The evidence in Table 1 suggests that one contributing factor may be that the price thresholds specified by existing APG laws are small relative to the underlying volatility in prices even in non-disaster periods. If retailers often move by prices by more than 10-25%, it may be difficult for regulators to make the case that such changes that occur in non-disaster periods truly constitute price-gouging even if they technically violate the text of the law. As a further requirement for the enforcement of APG laws, most states offer exceptions for stores that can demonstrate a price increase was justified by an increase in their costs. We turn to an empirical examination of the potential changes in input costs facing retailers during natural disasters next.

4.2 Effects of Natural Disasters on Retailer Costs

Wholesale Prices: Disasters have no measurable effects on wholesale prices

Figure 9 shows results from estimating the Equation 1 on wholesale prices. As with the retail price event study, we construct product category average prices at a weekly frequency and assign disasters to the county in which a wholesaler operates. The results show no distinguishable effects on wholesale prices for any of the nine main products. The Appendix shows similar results for the other product categories.

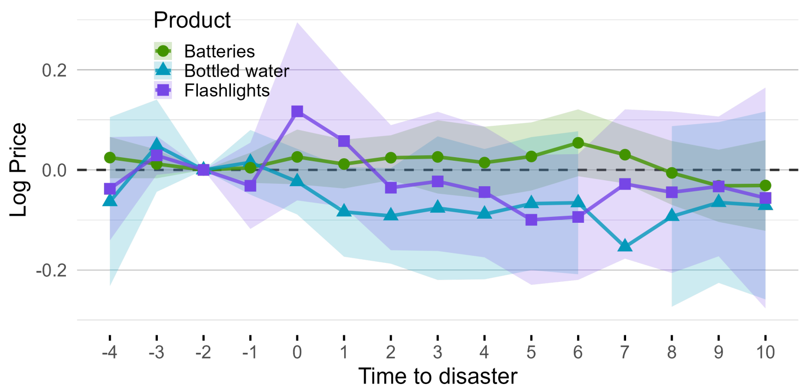
We interpret these results as evidence that disasters do not cause retailers to face cost shocks in their procurement of wholesale goods. This may be because wholesalers aggregate across geographically diversified suppliers beyond the disaster region, or because prices tend to be long-term contracted.

Transportation: Disasters have modest effects on trucking costs

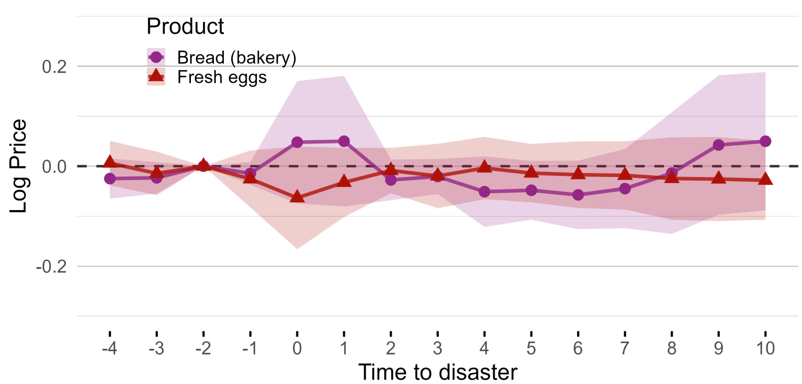
We use data from Echo Global Logistics, a trucking brokerage firm, to estimate the effects of disasters on trucking shipments and prices. We use the event study specification from Equation 1, but consider only two treatment periods: the ten days before a disaster and the ten days following a disaster, inclusive of the day the disaster was declared. We use information from the data on the starting point, end point, price, and duration of each trip to construct a number of variables that decompose the components of each trip's cost.

Figure 9: Effects of disasters on wholesale prices

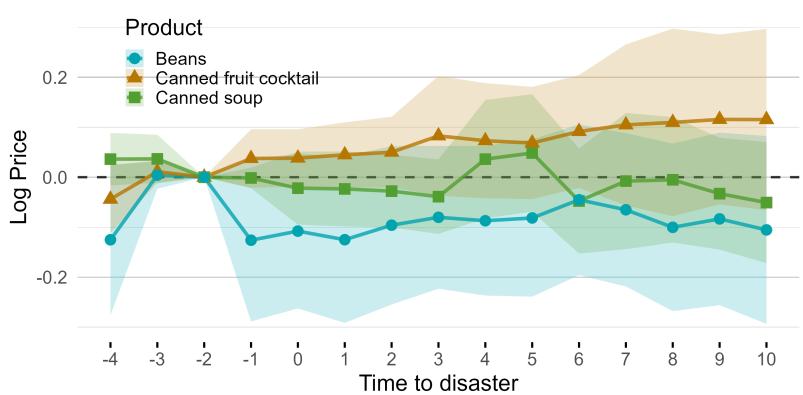
(a) Durable goods



(b) Perishable goods



(c) Shelf-stable goods



Notes: Figure plots estimates $\hat{\beta}_\tau$ associated with wholesale prices in product categories. Corresponding standard errors are in shaded areas. Estimates reflect changes in wholesale prices during the 12 weeks around disasters relative to non-disaster periods.

Table 2: Effects of Natural Disasters on Trucking Shipments

	(1) Price/Mile	(2) Price/Mile	(3) Hours/Mile	(4) Price/Hour	(5) Delayed	(6) Price/Trip	(7) Num Trips/Day	(8) Miles/Trip	(9) Hours/Trip
Pre Disaster	-0.00867	-0.00256	0.00361	1.919	-1.073	31.58**	0.199***	11.66*	2.597
	(0.00749)	(0.00703)	(0.00257)	(3.409)	(5.177)	(10.16)	(0.0346)	(4.893)	(1.367)
During / Post Disaster	0.0537***	0.0695***	0.00192	0.626	-2.865	112.0***	-0.245***	30.25***	4.144**
	(0.00740)	(0.00696)	(0.00254)	(3.369)	(5.119)	(10.05)	(0.0362)	(4.838)	(1.351)
Miles per Trip		-0.000524***							
		(0.00000178)							
Cons	2.204***	2.632***	0.0634***	47.63***	6.102***	1739.0***	4.790***	816.2***	49.61***
	(0.000642)	(0.00157)	(0.000221)	(0.292)	(0.444)	(0.872)	(0.00315)	(0.420)	(0.117)
N	658817	658817	658642	657955	658816	658817	4678911	658817	658642

Notes: Table reports estimates of $\hat{\beta}_\tau$ from Equation 1, but with disaster time periods measured as the ten days before a disaster and the ten days following a disaster. Estimates reflect the changes in trucking shipment outcomes during the 10 days before and 10 days after disasters, relative to non-disaster periods. Trips are considered treated by natural disasters if they end in a county experiencing a declared disaster.

In each case, the dependent variable in the regression refers to trips that *end* in the disaster county, as we are interested in shipments that affect available supply in disaster regions.

Table 2 displays the results. Column 7 shows a shift in shipping transactions from the post-disaster period to the pre-disaster period. The disaster window sees about 5% fewer trips than would be expected in a 10 day non-disaster period, and the pre-disaster period sees about 4% more. This is consistent with retailers adding a buffer of supply in response to a forecasted impending disaster, but having reduced access or reduced willingness to order new shipments in the immediate aftermath of the event.

Column 6 of Table 2 shows that the price of each trip is about 6.4% higher than average in the post-disaster window, and about 1.8% higher in the pre-disaster window. In the pre-disaster window, an increase in demand for shipments could play a role in raising the equilibrium price. In the post-disaster window, prices may also be affected by disruptions to supply or a safety premium for truckers driving in risky road conditions. Column 8 shows that the average trip distance is 3.6% longer in the post-disaster window, and 1.3% longer in the pre-disaster window, consistent with a potential need to access more irregular suppliers from further away.

Columns 1 and 2 show that the price per mile traveled is also about 2.5% higher in the post-disaster window, even controlling for trip distance (which can affect price per mile, as shorter trips are typically more expensive per mile traveled). This is consistent with the idea of a safety premium for contending with hazardous conditions. All of these effects are highly statistically significant. Perhaps surprisingly, however, Columns 3 and 4 show that trips do not take much longer per mile or cost more per minute in the disaster period, as the effects on hours per mile and price per hour are modest in size and not statistically different from zero. This suggests that the physical barriers to trucking may not be especially severe on average in the ten day window following a disaster.

Altogether, the estimates of disasters on trucking suggest that the cost of shipments rises by about 6-7% in the post disaster period, driven by a combination of sourcing from more distant suppliers and paying a higher price per mile traveled. We also find no discernible effects of disasters on wholesale prices. Given that prior estimates suggest that the marginal cost faced by supermarkets consists of 70-80% wholesale costs and 20-30% all other costs (Montgomery, 1997), we interpret this collection of results as suggesting that retailers face only modest cost shocks during disasters. We interpret the welfare and policy implications of this evidence further in the sections to follow.

5 Model

The lack of enforcement of APG laws leads us to consider how market outcomes would change were the laws enforced. As retailers would likely change their behaviors in response to a price cap, we specify a model to predict counterfactual outcomes.

We start with a basic model to develop intuition for how retailers might respond. We then show comparative statics that highlight how price caps can increase or decrease supply and consumer surplus, before specifying a more complete empirical model that we will later estimate.

5.1 Basic Model

We consider a single-product store, j, in period t, when a disaster suddenly hits. At the moment of the disaster, j has I_{jt} units of inventory and residual demand $D_{jt}(p_{jt})$ as a function of its price. The retailer has two types of costs: constant marginal cost c_{jt} from selling an additional unit from inventory and cost K_{jt} from ordering (a very large amount of) new inventory.

Let $O_{jt} \in \{0,1\}$ be whether j places an order for new inventory. In the absence of a price cap, j chooses its price and ordering decision to maximize static profits:

$$\begin{aligned}\max_{p_{jt}, O_{jt} \in \{0,1\}} & O_{jt} \left((p_{jt} - c_{jt}) D_{jt}(p_{jt}) - K_{jt} \right) \\ & + (1 - O_{jt}) \left((p_{jt} - c_{jt}) min(D_{jt}(p_{jt}), I_{jt}) \right) .\end{aligned} \tag{3}$$

The first term represents profits when a new order is made. The ordering cost K_{jt} is paid and then variable profits are maximized where the retailer is not at risk of stocking out. The second term represents profits without a new order. The retailers sells $D_{jt}(p_{jt})$ units, unless inventory is not enough to cover residual demand, in which case the retailer sells I_{jt} units.

Let $\tilde{p}^*_{jt}$ be j's profit-maximizing price were I_{jt} infinite. If $D_{jt}(\tilde{p}^*_{jt}) \leq I_{jt}$, then inventory is not binding, $p^*_{jt} = \tilde{p}^*_{jt}$, and no order is placed ($O^*_{jt} = 0$).

If inventory is binding ($D_{jt}(\tilde{p}^*_{jt}) > I_{jt}$), then the retailer may take one of two paths. First, it may decide to increase inventory by making a new order ($O_{jt} = 1$), in which case it would relax the inventory constraint and charge $p^*_{jt} = \tilde{p}^*_{jt}$. Second, it may decide not to reorder ($O_{jt} = 0$) and sell up to its inventory, I_{jt}. Then it would set its price to sell exactly I_{jt} units. Let $D^{-1}_{jt}(I_{jt})$ be j's inverse residual demand, evaluated at the inventory level. Then the profit-maximizing price would be $p^*_{jt} = D^{-1}_{jt}(I_{jt}) > \tilde{p}^*_{jt}$.

The retailer will take the reordering path when the reordering costs are lower than the incremental variable profits from relaxing the inventory constraint:

$$K_{jt} < (D_{jt}(\tilde{p}^*_{jt}) - I_{jt})(\tilde{p}^*_{jt} - c_{jt}) - I_{jt}(D^{-1}_{jt}(I_{jt}) - \tilde{p}^*_{jt}), \tag{4}$$

where the first term on the right-hand-side is the increased profits from making more sales and the second term is the decreased profits from setting a lower price on the sales from the initial inventory.[7]

Now suppose a price cap $\bar{p}_{jt}$ is instituted and enforced and that the cap is binding ($\bar{p}_{jt} < \tilde{p}^*_{jt}$). The retailer's problem is the same except instead of choosing $\tilde{p}^*_{jt}$, when inventory is not binding or a new order is placed, or $D^{-1}_{jt}(I_{jt})$, when inventory is binding and no new order is placed, the retailer will price at $\bar{p}_{jt}$. The retailer will then place a new

[7]We can extend the model to allow the retailer to set two prices: one on the initial inventory and one on newly ordered inventory. The qualitative conclusions below are all unchanged.

order when:

$$K_{jt} < (D_{jt}(\bar{p}_{jt}) - I_{jt})(\bar{p}_{jt} - c_{jt}). \tag{5}$$

Compared to the condition without a price cap, the condition with a price cap only depends on the incremental sales from reordering, as the initial inventory will be sold at $\bar{p}_{jt}$ regardless of whether a new order is placed.

The model highlights why a price cap could lead to more or less reordering. A higher price, in the absence of a price cap, increases the profit per reordered unit sold. But a constrained lower price moves the retailer further along the demand curve and increases the number of reordered units that will be sold. We next analyze a toy numerical example that verifies this tradeoff and characterizes the consumer surplus-maximizing price cap.

5.2 Comparative Statics and Optimal Price Caps

Consider the case of linear residual demand: $D_{jt}(p_{jt}) = 50 - 3p_{jt}$. We will start with a marginal cost of 2 and assess how price caps affect reordering and consumer surplus, depending on inventory and ordering costs.

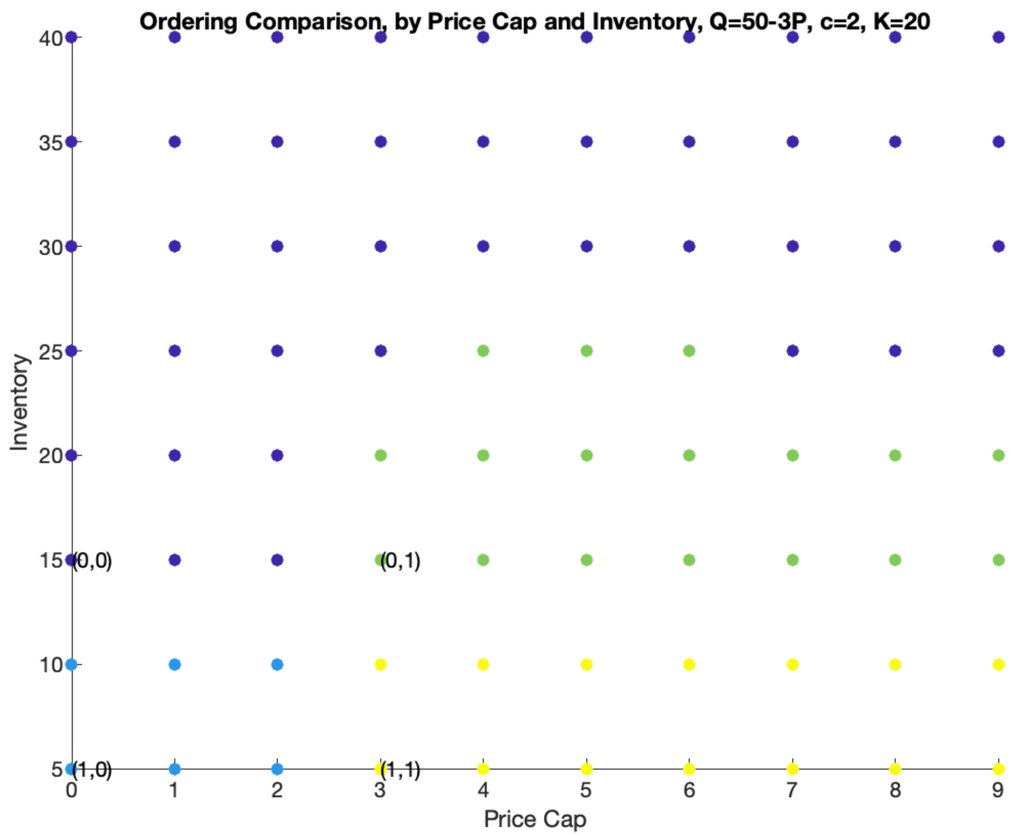
First, we fix the ordering cost at 20 and examine how outcomes vary with inventory. In Figure 10, for each level of price cap (x-axis) and inventory (y-axis), we solve whether the retailer will reorder in the absence of a price cap (first argument) and whether the retailer will reorder under the price cap (second argument). The colors show the four possible scenarios of reordering under each regime.

We see that all four scenarios appear in the figure. Thus, price caps may increase reordering, maintain reordering, decrease reordering, or maintain no reordering. Increases are most likely at medium inventory levels, when the lower prices mean more consumers would buy out of the reordered inventory. Decreases are most likely when price caps are particularly low and initial inventory levels are low.

Consumer surplus depends not just on the supply (reordering) response but also on the price consumers transact at. In Appendix Figure 1, we show that when price caps and inventories are both low, the price cap decreases consumer surplus. But for higher price caps or inventories, the price cap increases consumer surplus. Thus, whether a price cap benefits consumers is theoretically ambiguous.

These results show that the level of the price cap matters for the supply response

Figure 10: Ordering Comparison by Price Cap and Inventory

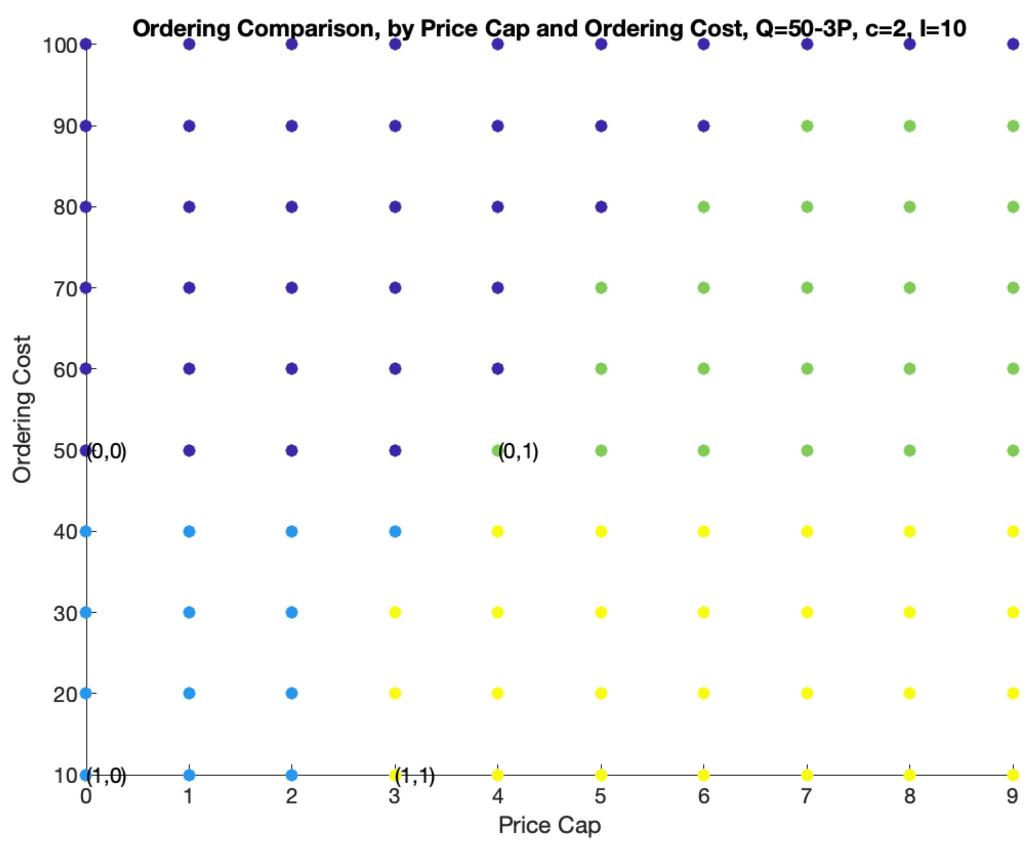


Notes: The figure compares ordering decisions based on the price cap and inventory. The first entry is whether the unconstrained retailer orders. The second entry is whether the retailer with a price cap orders.

and consumer surplus. In Appendix Figure 2, we show how the consumer surplus-maximizing ("optimal") price cap varies with inventory. We see that the optimal price cap is non-monotonic in inventory levels. At low inventories, a moderate price cap is optimal, as the low inventory means the retailer will reorder supplies with or without a price cap and the cap thus reduces the retailer's pricing market power. Even for the case where inventory is 15, the optimal price cap is 3 even though the cap moves the retailer from reordering to not. Here, the pricing market power reduction overcomes the decrease in transactions.

If we instead let ordering costs vary, fixing inventory at 10, we see similar patterns. Figure 11 shows that price caps can lead to more, less, or not change in reordering, with reordering rates predictably falling in ordering costs.

Figure 11: Ordering Comparison by Price Cap and Ordering Cost



Notes: The figure compares ordering decisions based on the price cap and ordering cost. The first entry is whether the unconstrained retailer orders. The second entry is whether the retailer with a price cap orders.

Consumer surplus can again increase or decrease with a price cap (Appendix Figure 3), where surplus tends to be higher with price caps as ordering costs increase (because ordering is less likely and reducing pricing market power is more valuable), though not in all cases. In particular, at a price cap of 3, the price cap leads to higher consumer surplus at a medium ordering cost of 40 but not for higher or lower ordering costs.

Finally, the optimal price cap is mostly increasing in the ordering cost (Appendix Figure 4), as giving retailers higher prices makes them willing to incur the high ordering cost. But eventually ordering costs are sufficiently high that the retailer opts against ordering and a lower price cap is optimal.

In sum, our toy model confirms that price caps can decrease or increase the supply response and consumer surplus. The optimal price cap is non-monotonic in inventories and ordering costs. Making predictions about the sign of counterfactual outcomes thus requires an empirical model, which we turn to now.

5.3 Empirical Model

Our basic model considered a single retailer's problem. We now specify a complete model. There are M markets active over T periods. A period is a three-week block, which we choose to reflect the event study results that most disaster effects are felt within three weeks and we seek to model the full disaster period rather than the dynamics within the period. I_m consumers live in market m and there are J_m retailers. Each retailer sells the same product.

In some market-periods, there will be a natural disaster. Let $d = d(m,t)$ indicate whether a market-period has a disaster. We will not specify an arrival process for disasters because we will assume no anticipation.[8]

We start as if all retailers are local monopolists, with $J_m = 1$ for all m. Thus, residual demand is equivalent to market demand. We label latent demand j in week t as $D_{jt}(p_{jt})$. Future drafts may extend the model to incorporate competition across retailers and we write the identification section below for the more general case.

Retailer j has marginal cost c_{jt} of selling the product, provided it is in inventory. Let I_{jt} be j's inventory for week t. During non-disaster periods, inventory follows an exogenous stochastic process. We assume that $I_{jt} > D_{jt}(p_{jt})$, which is consistent with stockouts being

[8] As we show in Dinerstein et al. (2025), retailer profits are highly concentrated in non-disaster periods and thus we assume that retailers do not optimize for even the chance that a disaster might hit.

very rare in non-disaster periods.

During disaster periods, I_{jt} is what j would have in t were it not to adjust its plans given a disaster arrived (i.e., inventory is the inventory at the beginning of the period plus any new orders that are already scheduled and will occur without any endogenous adjustments). The retailer may endogenously increase inventory by making a new order. Let the cost to ordering (a very large amount of) new inventory be K_{jt} and O_{jt} be an indicator for whether j orders new inventory in t.

Retailer j has a stock-out in disaster period t if and only if $D_{jt}(p_{jt}) \geq I_{jt}$ and $O_{jt} = 0$, where $D_{jt}(p_{jt})$ is j's residual latent demand.

Retailers choose prices and orders to maximize profits. Equation 3 shows the retailer's problem. Future drafts will incorporate heterogeneity in pricing flexibility based on chain uniform pricing policies (DellaVigna and Gentzkow, 2019).

6 Model Identification and Estimation

We next discuss the requirements for identifying model parameters. Future drafts will then estimate the empirical model and conduct counterfactual analysis.

6.1 Identification

We start with an argument for how we would identify parameters of a non-parametric model. While we will estimate a parametric model, we find that the argument in the non-parametric case is the most helpful for understanding how variation and realized outcomes link to model parameters.

We assume that marginal and order costs are observed. The main marginal cost is the wholesale price and the main order cost is the cost of the trucking trip, each of which we observe in data.

The main identification challenge is that unobserved stockouts make consumers' choice sets latent. We see transacted quantities in the data, but do not always know whether the quantity transacted reflects demand at that price or reflects inventory levels. Latent choice sets create econometric challenges where, in a broad class of models, identification of utility and availability requires both an instrument for availability and an instrument for utility (Agarwal and Somaini, 2022).[9] We will be able to forgo the utility instrument

[9]Stockouts complicate the search for an instrument that shifts utility and not availability because avail-

because we have data on reorders, which are related to both utility and availability. The structure of our economic model will characterize this relationship.

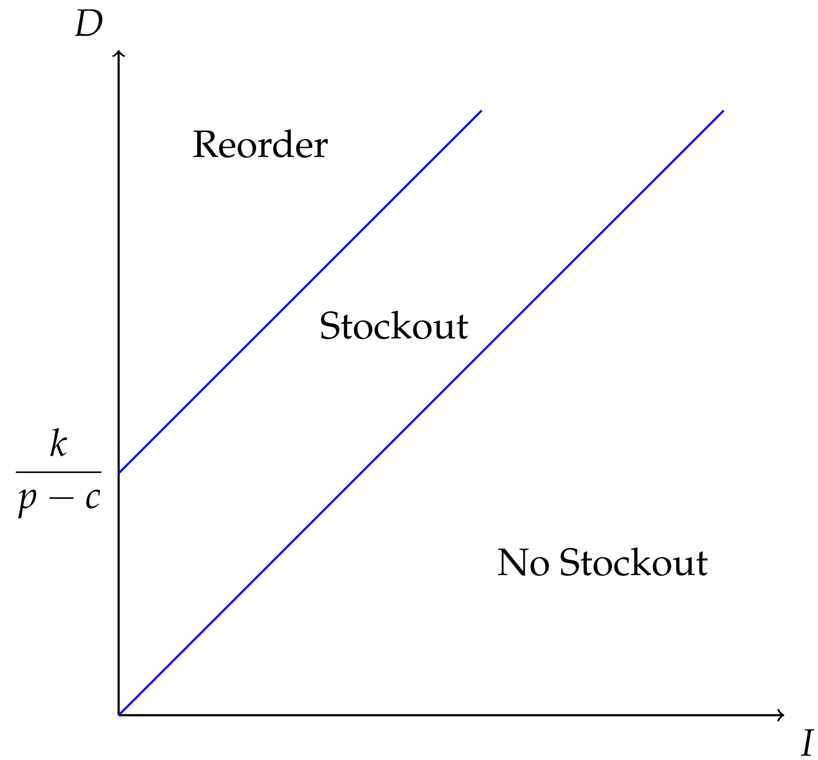
6.11. Identification with Exogenous Prices and Observed Orders

Consider the joint distribution of latent (residual) demand and inventory, given price and ordering costs: $F_{(D,I|p,K)}$. Assume costs c and K are observed. Demand and inventory are each latent. Instead of observing these latent variables, in the data we observe realized transactions D^* and orders O. The identification question is whether knowing $F_{(D^*,O)|p,K}$ lets us identify $F_{(D,I)|p,K}$.

Our joint model of orders and stockouts provides structure that relates observed outcomes to latent variables. We provide intuition in Figure 12, for a retailer not facing price caps. If inventories are higher than latent demand – the bottom right triangle of the figure – then there is no stockout. If inventories are lower than latent demand but not too much lower (the middle strip in the figure), then a stockout occurs because the retailer does not have enough inventory but also does not find it profitable to reorder. If inventories are well below latent demand (the upper left triangle in the figure), then the retailer reorders more inventory. The dividing lines between these regions are implied by the model. The lower line is the 45-degree line. The line separating stockouts from reorders is parallel to the 45-degree line, where its intercept is $\frac{k}{p-c}$, as implied by optimal reordering in the model.

ability is a direct function of aggregate demand.

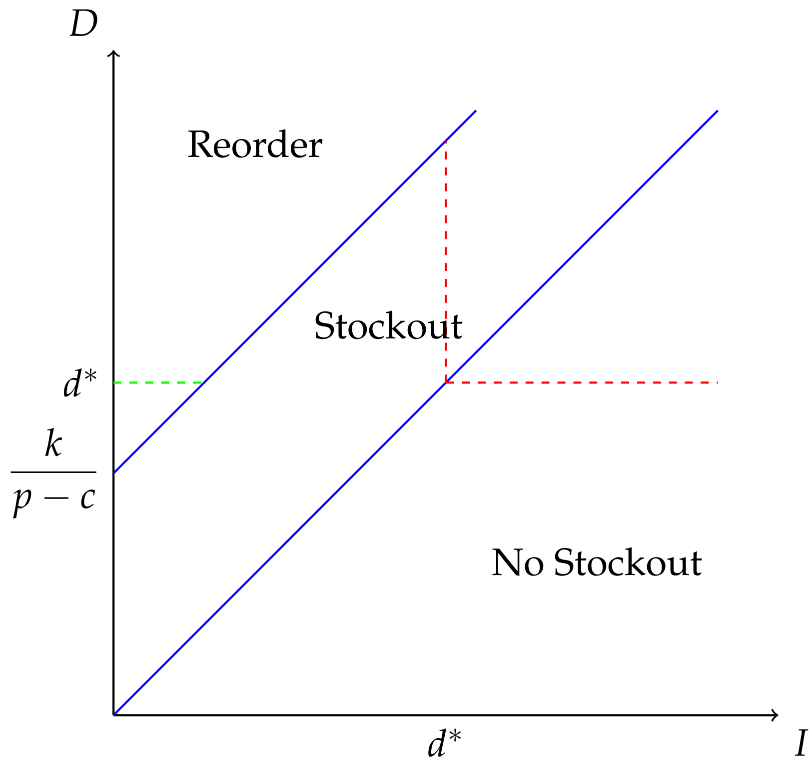
Figure 12: Identification Argument: Reordering and Stockout



Notes: Figure displays the regions in inventory-demand space during which stockouts and/or reordering occur under the empirical model.

We see neither latent demand nor inventories in the data, so we cannot place data points on this figure. Instead, we observe transactions, D^*, and orders, O. Consider d^*, a specific value of D^*. In Figure 13, we show in the dashed lines the various latent demand and inventory points that could lead to d^* transactions. The two horizontal segments represent situations where all desired transactions take place – i.e., latent demand equals realized transactions – with the green left part representing a situation where the retailer reordered and the red right part representing a situation where the retailer had sufficient inventory when the disaster hit. The vertical segment represents a stockout, where realized transactions coincide with the inventory level, and latent demand exceeds inventory up to the point where the retailer would have reordered. We know whether latent demand and inventory lie on the green versus red line based on whether we see a reorder, $o = 1$, but otherwise we cannot point identify where we are on the lines.

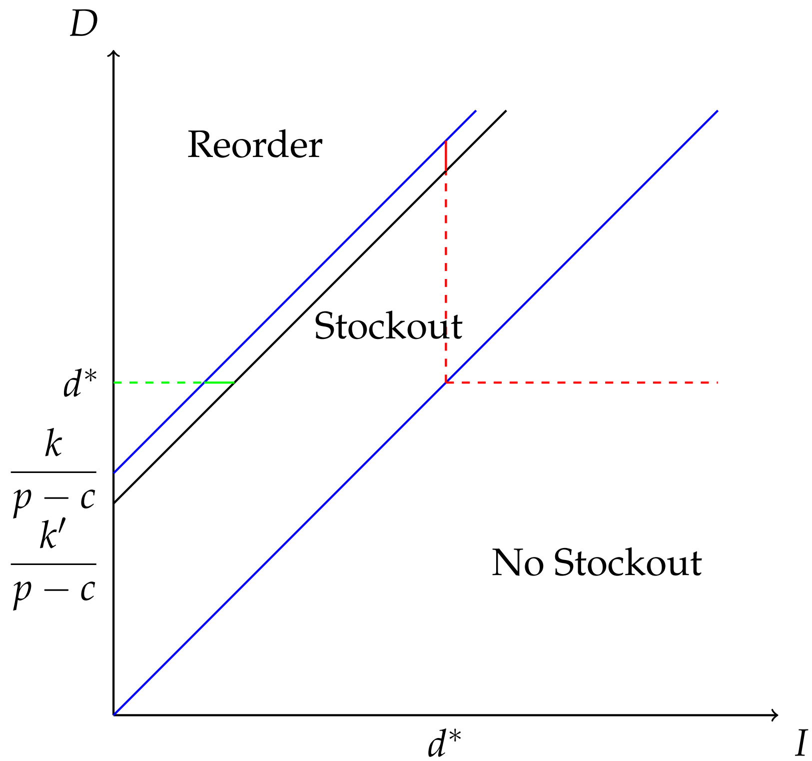
Figure 13: Identification Argument: Observed Transactions



Notes: Figure displays how observations of orders and transactions map onto latent values of inventory and demand.

We now assume we also have exogenous variation in K from some instrument that does not vary with D or I. Thus, $F_{(D,I)|p,k} = F_{(D,I)|p,k'}$ for various k, k'. We draw one such k' in Figure 14, where we have a slight decrease in ordering costs. This makes reordering slightly more likely, and converts some situations where there would be a stockout into reorders. If we compare the relative frequency of observing d^* transactions with ordering costs k' versus k, then the difference corresponds to the mass of latent demand and inventory that is on the green solid line net of the mass on the red solid line. If we compare the relative frequency of observing d^* transactions separately by whether a reorder was made, then we can separate the mass on the green solid line from the mass on the red solid line.

Figure 14: Identification Argument: Variation in Order Costs



Notes: Figure shows how exogeneous variation in ordering cost K can be used to identify the latent joint distribution of demand and inventory.

For this same reduction in ordering costs, we can repeat this visual argument for every observed value of quantity transacted (for a given exogenous price) to identify the part of the distribution that lies between the "reordering line" under k and the reordering line under k'. Exogenous variation in K that shifts ordering costs from 0 to infinity then shifts the location of these lines further and allows us to identify the part of $F_{(D,I)|p,K}$ where $D > I$.

This argument does not let us learn about the "No Stockout" area because here ordering costs do not matter. We have some extra information because we know that if there is no stockout, observed transactions match latent demand. We thus identify the conditional marginal distribution of D: $F_{D|p,K,D<I}$. We are thus partially identified. To point identify the nonparametric joint distribution, we would also need an instrument for inventory levels.

6.12. Endogenous Prices and Other Extensions

Endogenous prices pose two challenges. First, prices may be correlated with demand unobservables such that cross-sectional comparisons of demand at different price points do not capture the causal effect of a price change on demand, holding all else equal. Our argument above leads to identification of latent demand at each specific price. We can then identify the causal effect by using an instrument for price.

Second, as our model highlights, retailers might choose prices endogenously with respect to inventory levels and whether they are reordering. In this endogenous case, our identification argument holds for $p = \tilde{p^{*''}}$, the price the retailer would charge with infinite inventory. The challenge is that this price is not necessarily observed in the data if there is a stockout, when we predict retailers will choose $D^{-1}(I)$. We note that with enough variation in the ordering cost instrument, we can identify $F_{(D,I)|p,K,D>I}$ by solely focusing on data points when orders are made. As Figure 14 shows, identifying mass on the solid green lines will cover the full upper triangle of the space.

The other extension reflects the sampling of our ordering data. We only see a sample of orders. Provided we know the sampling rule (including the rate), we can convert our identification argument to only probabilistically observed O. The argument still goes through, though estimation will be noisier.

6.13. Instrument Choices

We propose using gas prices and chain-level price rules as instruments for ordering costs and prices. Our trucking data show that gas prices are key inputs into the price per mile. Thus, disasters that hit during high gas price periods may lead to fewer deliveries. Because the trucks are often leaving from different geographic areas, we plan to use gas prices in adjacent areas, but not in the disaster area.

For pricing, some chains do not vary prices across stores (DellaVigna and Gentzkow, 2019), such that local prices are less likely to be endogenous.

Future drafts will develop these instruments in more detail.

6.2 Estimation

In estimation, we will use more information from our data than we included in the identification argument. In the event study analysis, we examined the effect on whether a retailer made 0 sales in a week. While our model aggregates the disaster weeks into a single period, we know that a retailer that made 0 sales in a week was stocked out. Thus, we partially observe (selected) stockouts.

Future drafts will describe estimation in more detail and include estimates.

7 Counterfactuals

Future drafts will incorporate estimates of counterfactual outcomes.

8 Conclusion

This paper examines the existing and potential efficacy of emergency price controls during natural disasters. Thus far, we show event study estimates of retail market dynamics during disasters. Quantities transacted rise sharply for a wide range of products, particularly durable goods. Some of these goods, especially perishable foods like eggs and milks, experience prolonged shortages at some retailers in the weeks following a disaster. Average prices for emergency supplies more little during disaster periods, but some retailers raise prices by very large amounts, such as over 50%.

We also show in the data that anti-price-gouging laws appear to be violated with substantial frequency, and that existing laws do not seem to have been widely enforced. Given this, we consider the effects of a hypothetical well-enforced policy using a structural model of retailer ordering and pricing decisions. In future drafts of the paper, we will estimate this model and quantify the effects of counterfactual policy scenarios.

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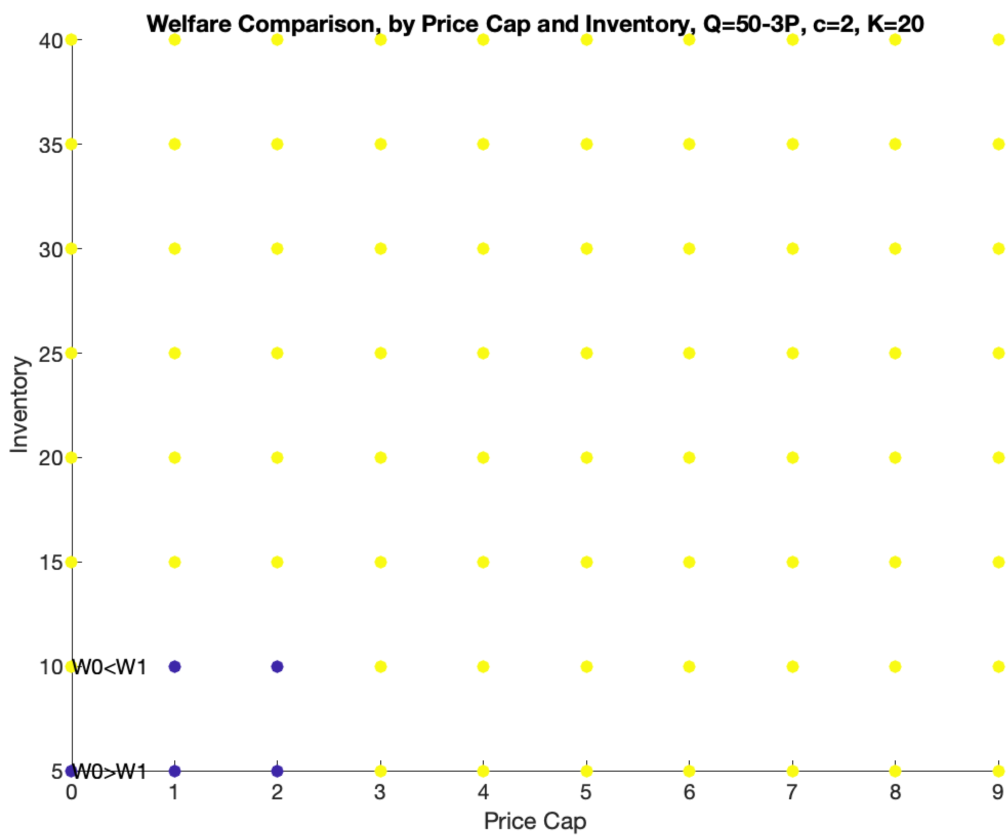
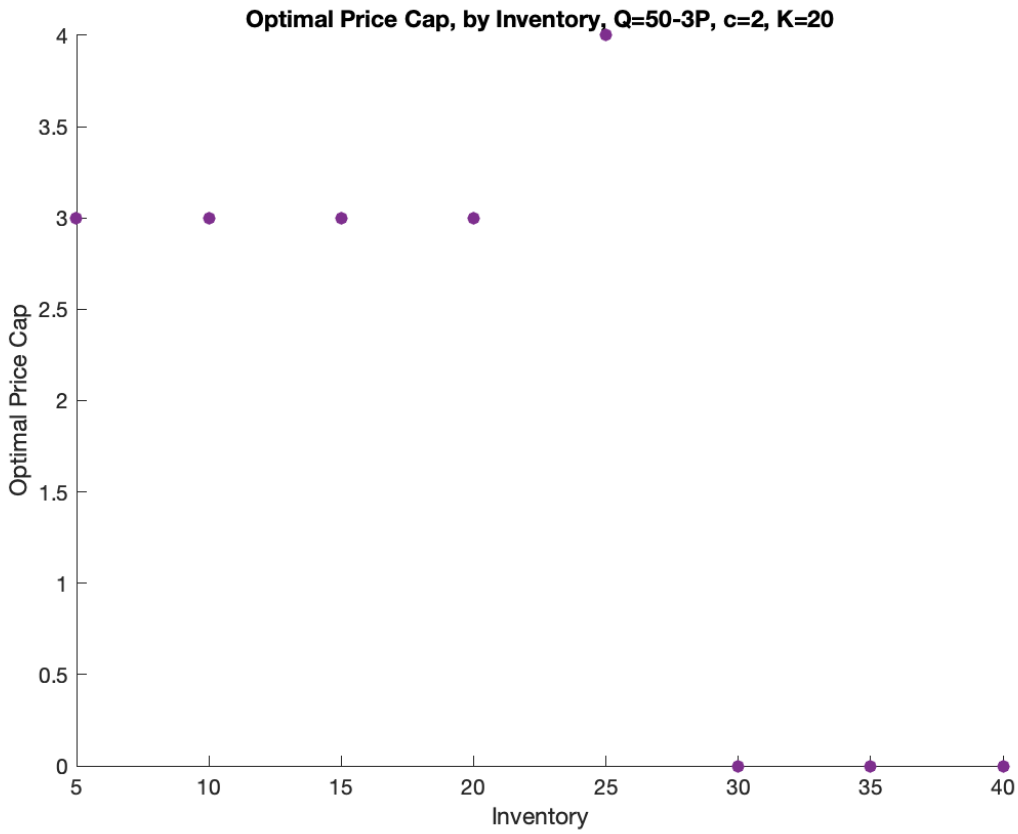
9 Appendix Figures

Figure 1: Consumer Surplus Comparison by Price Cap and Inventory



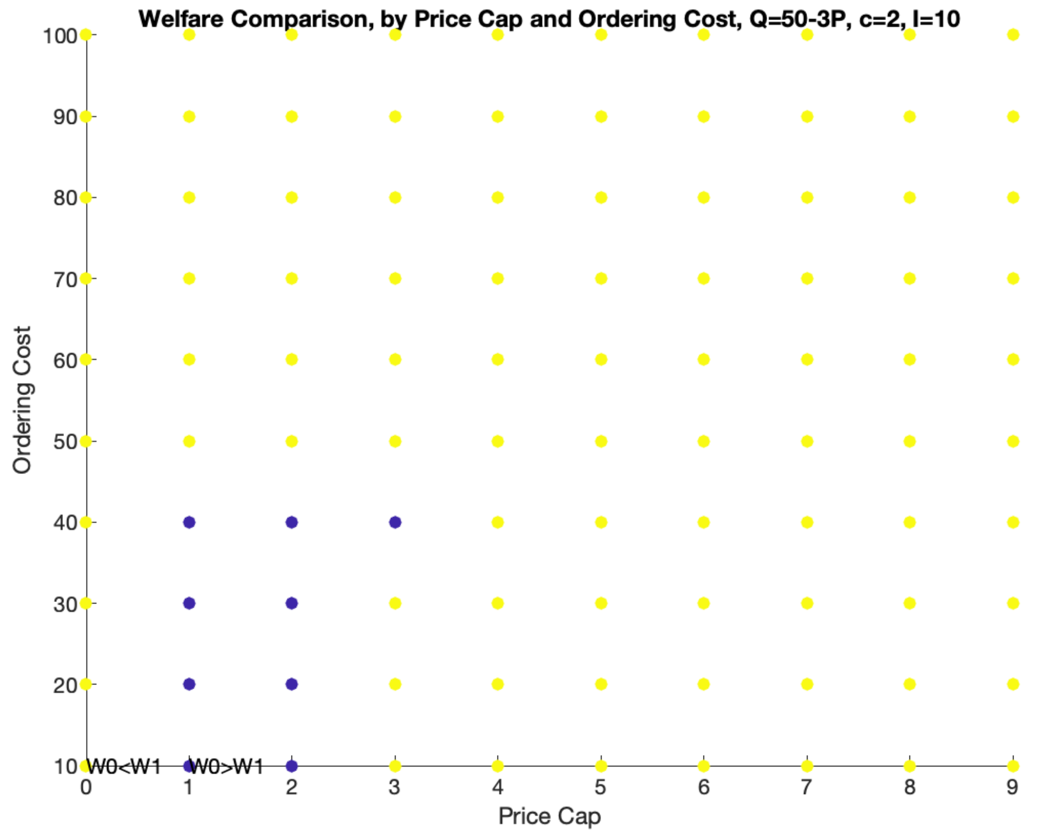
Notes: The figure compares consumer surplus based on the price cap and inventory. $W0 < W1$ means consumer surplus is higher under a price cap.

Figure 2: Optimal Price Cap by Inventory



Notes: The figure shows the optimal (consumer surplus maximizing) price cap. In all cases, the optimal price cap is not infinite. In cases where the price cap is below marginal cost (2), consumer surplus decreases monotonically in the price cap.

Figure 3: Consumer Surplus Comparison by Price Cap and Ordering Cost

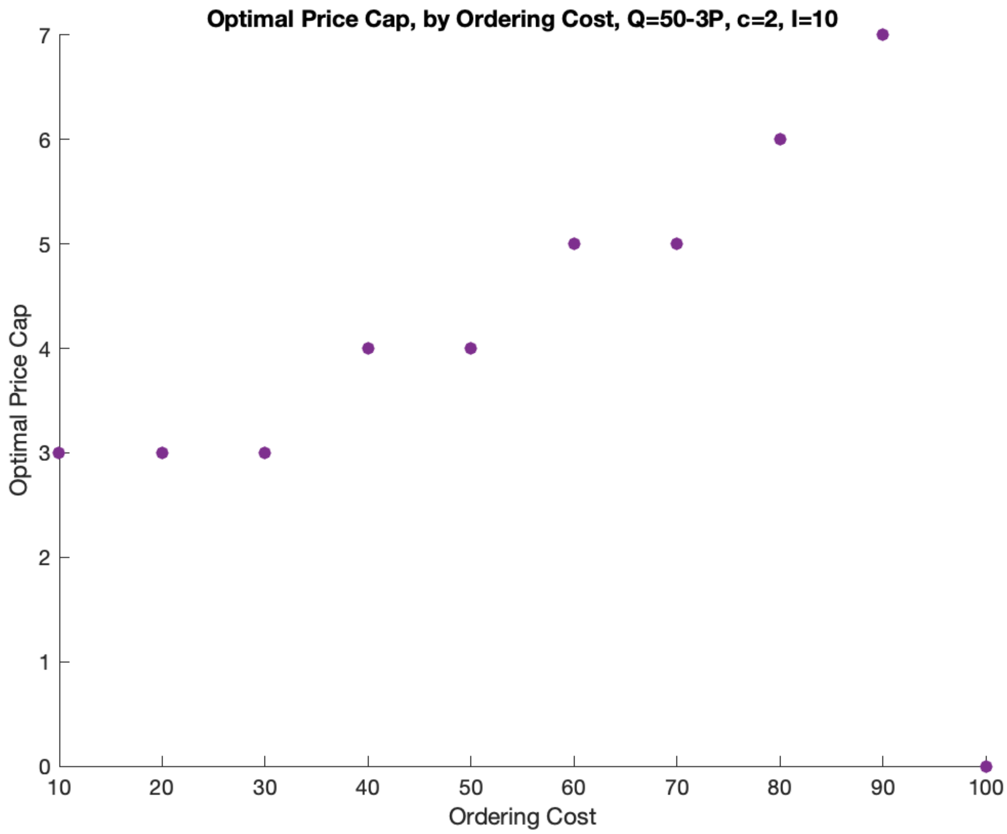


Notes: The figure compares consumer surplus based on the price cap and ordering cost. $W0 < W1$ means consumer surplus is higher under a price cap.

Depending on the level of the price cap and inventory, a price cap can lead to more or less ordering and higher or lower consumer surplus.

What is the optimal price cap, by ordering cost?

Figure 4: Optimal Price Cap by Ordering Cost



Notes: The figure shows the optimal (consumer surplus maximizing) price cap. In all cases, the optimal price cap is not infinite. In cases where the price cap is below marginal cost (2), consumer surplus decreases monotonically in the price cap.