DISCUSSION PAPER SERIES

No. 10725

SMALL PRICE RESPONSES TO LARGE DEMAND SHOCKS

Etienne Gagnon and David López-Salido

MONETARY ECONOMICS AND FLUCTUATIONS

Centre for Economic Policy Research

ISSN 0265-8003

SMALL PRICE RESPONSES TO LARGE DEMAND SHOCKS

Etienne Gagnon and David López-Salido

Discussion Paper No. 10725
July 2015
Submitted 08 July 2015

Centre for Economic Policy Research
77 Bastwick Street, London EC1V 3PZ, UK
Tel: (44 20) 7183 8801
www.cepr.org

SMALL PRICE RESPONSES TO LARGE DEMAND SHOCKS[†]

Abstract

We study the pricing response of U.S. supermarkets to large demand shocks triggered by labor conflicts, mass population relocation, and shopping sprees around major snowstorms and hurricanes. We find that these large swings in demand have, at best, modest effects on the level of retail prices, consistent with flat short- to medium-term supply curves. This finding holds even when shocks are highly persistent and even though stores adjust prices frequently. We also uncover evidence that retailers with radically different demand shocks nonetheless seek to match their local competitors' pricing movements and recourse to sales and promotions.

JEL Classification: E30 and L11
Keywords: demand shocks, inflation, labor conflicts, mass population displacement, sales and severe weather events

Etienne Gagnon etienne.gagnon@frb.gov
Federal Reserve Board

David López-Salido david.j.lopez-salido@frb.gov
Federal Reserve Board and CEPR

[†] The views in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the Board of Governors of the Federal Reserve System or any other person associated with the Federal Reserve System. We are grateful to Information Resources Inc. (IRI) for providing the scanner data. All estimates and analyses in this paper based on IRI data are by the authors and not by IRI. We thank Jason Sockin for providing superb research assistance, and Laura Ardila, Andrew B. Giffin, and Teyanna Munyan for further assistance with the data. We also thank Charlie Gilbert for assisting us with the weekly seasonal filtering procedure.

1 Introduction

The effect of broad-based movements in demand on prices and quantities remains a lingering question in economics. A key reason is that prices and quantities are simultaneously determined; analyses based on correlations between these two variables may thus suffer from biases in coefficients and not be used to support causal claims. In this paper, we document a number of instances in which large demand shocks to retailers can be readily identified and show that, in those instances, the level of their prices reacts proportionally little. In particular, we study the response of supermarkets to labor conflicts, mass population relocation, and shopping sprees caused by severe weather events, which are shocks that either profoundly reallocate customers across retailers or boost shoppers' willingness to consume. As we shall argue, we see the occurrence of our shocks as exogenous to retailers' activities, allowing us to use them as instruments to assess the causal effect of broad-based variation in demand on prices.

We perform our analysis using a large dataset of weekly scanner data made available to researchers by Information Resources Inc. (IRI). This dataset contains price and quantity information from 2001 through 2011 on personal care, housekeeping, and food products, and is well suited for our endeavor for several reasons. First, it covers 50 U.S. metropolitan markets, making possible the study of shocks that have a large impact on demand at a regional level but a limited influence on national aggregates. Second, its exceptionally large size—about 4 million individual observations per week after our trimming—permits the computation of reasonably accurate statistics at the product category–market level; this feat would not be achievable using the considerably smaller micro datasets collected for the computation of the official CPI and national account statistics. Third, it contains the universe of items sold by stores within each product category, allowing us to track the evolution of spending on these categories. In conjunction with the price data, the spending data permit us to derive real quantity measures (on a same-store basis) using a methodology similar to that employed for the national accounts. Fourth, given the weekly frequency of the data, we can analyze shocks that have a short life-span. Fifth, the dataset contains data from multiple retail chains and stores, which allows us to contrast the pricing strategies of competiting retailers facing differing shocks to demand.

Our most crucial task is to identify events that can broadly move demand and are otherwise exogenous to supply factors. We first consider two labor conflicts that both began in October 2003. The conflict in St. Louis, MO, was relatively short (less than a month) while that in Southern California dragged on nearly five months to become the longest supermarket strike and lockout in U.S. history. Because many shoppers would rather take

their business elsewhere than cross picket lines, strikes and lockouts can dramatically reallocate store traffic within a market. Indeed, many stores in our sample experienced revenue collapses of 50 percent or more during these conflicts, while several others belonging to unaffected retailers enjoyed correspondingly large increases. Whereas these conflicts hindered the ability of affected stores to supply goods, unaffected retailers faced no supply disruptions, making the rise in their demand akin to an exogenous demand shock. Moreover, at the end of these conflicts, supply disruptions vanished but store traffic did not always revert back to pre-conflict levels, providing another opportunity to study the effects of variation in the level of demand.

We next consider the mass population displacement brought about by Hurricane Katrina, the most expensive natural disaster in U.S. history. The hurricane displaced about 1 million persons, many of whom took years to resettle. Most displaced households moved in with relatives and friends, boosting population density and store traffic in neighborhoods that were relatively spared by the storm. Stores with undisrupted activities in our New Orleans, Louisiana, and Mississippi subsamples experienced a persistent rise in sales volumes of about 20 percent, on average, in the wake of the hurricane.

Finally, we consider shopping sprees around major snowstorms and hurricanes not associated with mass population displacement. Like Hurricane Katrina, their occurrence is unquestionably exogenous to retail activities. But contrary to Hurricane Katrina, the typical effects of these storms are short-lived and operate primarily through a rise in the demand of existing shoppers rather than through a reshuffling of the consumer base. Storms that result in the closing of schools and workplaces force households to consume a larger fraction of their meals at home, thus boosting the demand for food items. Similarly, the demand for personal care and housekeeping products rises around these events as households engage in more home production or might take advantage of their trip to the supermarket to purchase items other than food.

Our key finding is that large swings in demand have, at best, a modest effect on the level of retail prices, consistent with the economy's equilibrium moving along flat short- to medium-term supply curves (or, equivalently, consistent with high price elasticities of retail supply). This finding holds even in the case of our most persistent shocks—namely the Southern California labor conflict and Hurricane Katrina—for which stores adjusted the price of most items at least a couple of times before demand reverted back to more normal levels; this fact suggests that price stickiness alone cannot account for the lack of a price response. The muted price response also seems inconsistent with the marginal cost of retailers being sensitive to the level of demand, say because of fixed factors of production as posited by Burstein and Hellwig (2007). Relatedly, to the extent that our stores' marginal costs were

mostly unaffected by the shocks (a reasonable conjecture given the localized nature of our shocks, the preponderance of national brands in our sample, and the low value added of retail activities), our modest price responses do not point to retailers making large alterations to their markups in response to our swings in demand.

The behavior of sales volumes during and after our labor conflicts is also consistent with stickiness in consumer preferences for particular points of purchase, as emphasized by economic theories in which stores treat the consumer base as an asset. At the end of the Southern California conflict, affected stores immediately recouped 4 out of every 5 dollars in lost business. In the case of the shorter St. Louis conflict, which featured similarly large swings in quantities, sales volumes snapped back to pre-strike levels as soon as the conflict ended. Taken together, these observations indicate that preferences for particular stores are not eroded by shopping elsewhere for a few weeks, but longer displacements can lead to persistent changes.

Our evidence further illustrates that retailers care about matching their local competitors' pricing movements and recourse to sales and promotions. Perhaps most remarkably, stores on strike during the Southern California labor conflict put almost the same number of items on promotional sale as unaffected stores despite having limited staffing numbers, supporting the view that engaging in price discrimination is an essential activity of retailers. Once that conflict was over, stores that had suffered an erosion of their customer base boosted the number of sales and promotions to regain market share; this strategy was mimicked by their local competitors.

Our third set of demand shocks—shopping sprees triggered by major snowstorms and hurricanes—has no apparent effect on prices and other aspects of our retailers' pricing strategies. This finding is perhaps unsurprising because these shocks may be both too difficult to forecast and too transitory to leave retailers sufficient time to adjust prices. Nonetheless, they invite us to qualify earlier findings that retail prices tend to fall around transitory peaks in demand. Warner and Barsky (1995), MacDonald (2000), Chevalier, Kashyap, and Rossi (2003) report that the price of household appliances and a number of food items tends to decline around major holidays. This evidence is sometimes seen as suggestive of countercyclical markups at the macroeconomic level. Our conjecture is that the high predictability of demand peaks associated with holidays and the passing of seasons allows retailers and upstream producers ample time to adjust their marketing, inventory, and production strategies accordingly, making these events somewhat uninformative about how retailers might respond to unexpected demand shocks.

To our knowledge, our focus on broad-based demand shocks—that is, shocks that affect retailers' entire product offering—is novel in the empirical literature that uses large datasets

of individual data to bridge micro price behavior and aggregate price dynamics. Perhaps one exception is Coibion, Gorodnichenko, and Hong (2015), who look at the link between local labor market conditions and pricing. Otherwise, previous work has considered primarily a number of supply-related factors such as changes in sales taxes (see, for example, Dhyne *et al.* (2005) and the references therein, Karadi and Reiff (2012), and Gagnon, López-Salido, and Vincent (2013)), imported good price shocks (for example, Gopinath and Itskhoki (2010)), and commodity price shocks (for example, Nakamura (2008) and Hong and Li (2013)). This literature has typically found moderate pass-through rates that contrast with the muted price responses to our shocks. Our use of natural disasters to identify exogenous changes in demand is also somewhat unusual as the literature has typically been interested in their disruptive effects on supply. In a related paper, Cavallo, Cavallo, and Rigobon (2013) use online price data to show that a 2010 earthquake in Chile and the 2011 Sendai earthquake and tsunami in Japan both led to widespread product unavailability but not to higher prices.

The paper is organized as follows. Section 2 details the construction of our dataset and presents key features of retail activities in normal times. Section 3 analyzes customer base displacement due to labor conflicts. Section 4 investigates shifts in demand due to major weather events, starting with the year-long population displacement caused by Hurricane Katrina and proceeding with transitory boosts to demand related to major snowstorms and hurricanes. Section 5 discusses what factors might contribute to retailers' muted price responses to our large demand shocks. Section 6 concludes.

2 Dataset and some salient pricing facts in normal times

Our price and quantity analysis is performed using weekly scanner data made available to researchers for a nominal fee by IRI. The content of the dataset is detailed in Kruger and Pagni (2012) and Bronnenberg, Kruger, and Mela (2008), so we shall give only a brief exposition. The data come from a large sample of over $1,500$ U.S. supermarket stores belonging to a variety of retail chains and operating in 50 U.S. markets. An observation corresponds to the information about an item (that is, a barcode sold in a particular store) in a given week. The number of observations is exceptionally large at about 300 million per year. Available item information includes the number of units sold and total revenue during the week. As is customary with scanner data, we derive a unit price by dividing total revenue by the number of units sold. Although the data are limited to 29 food, housekeeping, and personal care product categories, they have the appealing feature of encompassing all item

transactions within those product categories.[1]

To make the micro data suitable for our purposes, we drop items with suspiciously large price adjustments and apply various filters to extract regular price series and compute various moments of temporary sales. Our regular price filter is based on that proposed by Nakamura and Steinsson (2008).[2] We then construct price and quantity indexes for each IRI market–product category combination using a methodology that closely matches that employed by the Bureau of Labor Statistics (BLS) and the Bureau of Economic Analysis (BEA) for the U.S. CPI and the U.S. National Income and Product Accounts, respectively. Our sample-wide statistics aggregate product-category statistics by weekly revenues. We relegate these technical details to our online appendix.

Before proceeding with our key findings, we briefly describe the general behavior of retail prices in the dataset to provide us with some benchmark against which to compare pricing behavior in the presence of large demand shocks. Our online appendix provides a more detailed presentation of salient pricing facts in normal times. The key message is threefold: retail prices are adjusted frequently, price adjustments are primarily associated with sales and promotions, and the bulk of barcode-level price variation is common across stores belonging to the same retail chain.

In particular, on average, 30.4 percent of items in our sample experience a price change every week.[3] On a monthly basis, 42.3 percent of items experience a price change, a figure somewhat above corresponding estimates for the broader U.S. CPI, which range from 26 percent in Bils and Klenow (2004) and Nakamura and Steinsson (2008), to 36 percent in Klenow and Kryvtsov (2008). Regular price changes identified by our filter are less common, at 11.2 percent per week and 23.7 percent per month. Moreover, price behavior is consistent with pricing decisions originating principally at the retail chain level. To establish this fact, we define a weekly "chain price" as the modal price at which a barcode is sold across stores belonging to the same retail chain within a market. Overall, 71.2 percent of store prices equal their corresponding chain price. In addition, the probability of an item experiencing

[1] The IRI dataset also has information on cigarettes and photographic supplies; we exclude these product categories because of regulatory restrictions on pricing and their gradual obsolescence, respectively.

[2] The identification of regular prices is subject to some degree of imprecision because of high-frequency movements in store-reported weekly average transaction prices. As our online appendix discusses, we experimented with a number of procedures to separate low- and high-frequency price movements. These procedures include a reference price filter, a V-shape sales filter, and the use of a 5-percent-or-more price reduction flag created by IRI. Typically, filters that produce relatively infrequent regular price adjustments are associated with relatively frequent but shallow sales. Conversely, filters that produce relatively infrequent sales are associated with frequent positive and negative regular price adjustments. Although no filtering method seems fully satisfactory, our conclusion that large demand shocks have little effect on prices is robust to whether we allocate high-frequency price variation primarily to sales or to regular prices movements.

[3] Our sample-wide statistics aggregate product-category statistics by weekly revenues.

a price change jumps to over 80 percent when we condition on a change in the item's chain price. Together, these facts provide clear indications that item prices are coordinated to some degree across stores belonging to the same retail chain.

3 Labor conflicts as exogenous demand shocks

Labor conflicts can have substantial effects on store traffic because many shoppers would rather take their business elsewhere than cross employee picket lines. Thus, affected stores can lose most of their demand whereas unaffected stores may experience a jump in sales volume. This section looks at such rearrangements of the customer base during two major labor conflicts in the supermarket industry. The key finding is that pricing strategies at stores whose demand rose largely mimicked those at stores whose demand declined. This finding is consistent with the effects of radically different demand shocks on pricing being trumped by a desire to keep up with pricing movements by local competitors.

Because labor conflicts are man-made and affect staffing levels, it is appropriate to take a moment to discuss their exogeneity with respect to pricing strategies and their likely effects on stores' ability to supply goods. For stores whose employees are not on strike or locked-out, there were no disruption to staffing numbers. The observed rise in their sales volumes was thus akin to an exogenous demand shock, supporting our use of the strike as an instrument. The normalization of staffing levels at the end of our conflicts provides other opportunities to study the effects of variation in demand in the absence of constraints on supply because store traffic did not always revert back to pre-crisis levels.

In the case of stores whose employees were on strike or locked-out, the ability to meet any given level of demand was certainly hindered by low staffing levels over the duration of the conflicts. As we will see shortly, the conflict was also accompanied by a large and arguably more important adverse shock to demand as many shoppers opted not to cross picket lines and took their business elsewhere. Strikes and lockouts are often due to disagreements over employee compensation, which enters a grocers' marginal cost and should thus influence prices. However, we know from financial statements of publicly-traded U.S. supermarket chains that compensation accounts for a relatively small share of overall supermarket revenues. For instance, the largest two publicly-traded U.S. retail chains with activities concentrated in supermarket products, Kroger and Safeway, reported goods acquisition costs equivalent to 77.5 percent of their combined revenues in 2012. By contrast, operating and administrative expenses, which include employee compensation as well as spending on store management, utilities, local advertising, etc., accounted for only 18.3 percent of revenues.[4]

[4] This publicly-available information is provided without any implication for these retailers' inclusion in

If these figures are representative of the cost structure of establishments in our sample, then the difference in bargaining positions between employers and employees is unlikely to have exceeded a small fraction of overall costs. For this reason, we see labor disputes as ultimately creating only limited uncertainty regarding prices paid by consumers. That said, even small changes to employee compensation can have a large impact on store profitability because food retailing is a low-margin, high-volume businesses (operating profits at Kroger and Safeway averaged only 2.6 percent of their combined revenues in 2012).

3.1 2003-2004 Southern California supermarket strike and lockout

On October 11, 2003, about 70,000 unionized supermarket workers in Southern California either went on strike or were put on lockout due to a disagreement with several retail chains over benefits and compensation. The dispute ended nearly five months later, on February 29, 2004, when workers voted in favor of a negotiated contract, resolving what had become the longest supermarket strike in U.S. history. Stores whose employees were on strike or on lockout remained in operation throughout the conflict, in part as retail chains reallocated managerial resources to the aisles and pursued alternative arrangements to ensure continued product availability. News organizations reporting on the conflict indicated that shelves were generally fully stocked but that few shoppers were strolling the aisles.[5] For this reason, we believe that supply constraints were not the primary driver of the large drop in quantities observed at stores on strike, although supply constraints certainly would have made it challenging if not impossible to satisfy pre-conflict sales volumes.

Our dataset contains establishments that experienced marked drops in revenues during the strike as well as establishments that experienced marked increases. As noted earlier, the identity of stores and retail chains is censored in the sample and, in compliance with our terms of usage, we make no attempt to uncover them. To further preserve the anonymity of establishments and retail chains involved, we pool all data from the Los Angeles and San Diego markets and look at two broad groups of stores: those whose revenues dropped more than 10 percent (generically labeled "on strike") and those whose revenues rose more than 10 percent (generically labeled "not on strike") during the strike relative to the corresponding period of the previous year. Because our identification strategy uses only observed movements in revenues but no retail chain information, the two groups may not perfectly

the sample. The terms of our contract with IRI prevent us from reporting any information that could lead to the identification of particular retail chains or establishments.

[5] For example, the New York Times reported on October 14 that "store shelves appeared fully stocked but the aisles were largely empty of shoppers." As an explanation, the article cited a striking inventory control clerk contending that "picketing had turned away about half of the market's customers" while "the other half [was] getting a couple things."

overlap with the sets of stores that were actually affected and unaffected by the strike. That said, persistent movements in weekly revenues of 10 percent or more are exceptional in the dataset, and the large movements that we attribute to stores being on strike or benefiting from the strike accord with the chronology of the conflict. Some stores also saw revenues change by less than 10 percent; we ignore them to focus on stores subject to large demand shocks. We finally compute separate statistics for each group to contrast the situation of stores with large positive and large negative shocks to demand.

3.1.1 Overall impact on quantities and prices

Salient features of the strike's impact on retailing activities are presented in figure 1. As the top panel shows, stores whose employees were on strike experienced, on average, a staggering 50 percent drop in sales volumes over the conflict's duration. Some stores even saw sustained declines in quantities close to 80 percent. The drop was somewhat more pronounced in the first few weeks, suggesting that some shoppers who had initially declined to cross picket lines or to limit their purchases quickly returned. This modest rebound aside, the drop in revenues at establishments on strike was large and sustained for the duration of the labor conflict. In sharp contrast, establishments in our group of stores that benefited from the strike witnessed large increases in revenues—over 30 percent, on average, with some stores even seeing their revenues more than double. Contrary to stores on strike, those that benefitted continued to have their regular employees present to serve consumers, replenish the shelves, and move products from warehouses to stores. In the absence of disruptions to their ability to supply products, the sudden rise in demand for these stores is thus unambiguously interpretable as a demand shock.

The top panel of figure 1 further suggests that a majority of consumers displaced by the strike returned to their previous shopping location at the end of the conflict: Sales volumes immediately rebounded at stores that had been on strike and fell sharply at stores that had benefited from the strike. The normalization was incomplete, however. In the year that followed the end of the conflict, sales volumes at stores that had been on strike were still 10 percent below sales volumes before the strike, whereas stores that had benefited from the strike retained some of the customers displaced by the strike. This fact could be consistent with theories that emphasize consumer loyalty to brand, stores, or retail chains.[6] The immediate return of sales volumes toward their pre-conflict levels suggests that consumer preferences for points of purchase may persist even after consumers have switched stores for

[6] Most such models are specified in terms of loyalty to specific items or brands rather than to establishments or retail chains as seems appropriate in our context. These models include switching costs (for example, Klemperer (1995) and Kleshchelski and Vincent (2009)) and deep habits (Ravn, Schmitt-Grohé, and Uribe (2006)).

nearly five months. It is also possible that other factors were at play. For instance, stores with significantly higher sales volumes during the strike may have been able to offer fresher produce as a result. Such benefits may, in turn, have helped them retain consumers at the end of the conflict.

As the middle panel shows, price movements during the conflict, at only a couple of percentage points, were more than an order of magnitude smaller than swings in sales volumes, suggestive of flat supply curves. For example, if we attribute the observed rise in prices at stores not on strike entirely to a positive demand shock, then the estimates in table 1 imply a supply elasticity equal to $\log(1.021)/\log(1.336) = 0.07$. This modest estimate falls slightly if we control for inflation over the strike period by measuring the demand shock's effect as the rise in excess of that for the price index of the full IRI sample.[7] Such small responses appear consistent with surveys of price-setting practices in which firms report being relatively unresponsive to positive shocks to demand conditions (see Fabiani *et al.* (2006)). These supply elasticity estimates for the retail sector are markedly lower than the 0.18 figure reported by Shea (1993) for the U.S. manufacturing sector. One explanation for the difference could be that Shea's (1993) estimate applies to a one-year horizon whereas the Southern California strike ended after five months, leaving less time for prices to adjust to higher demand. However, our evidence for the longer post-strike period, which features persistently large differences in demand and no supply disruptions at both groups of stores, also points to little if any price response.[8]

It is also apparent that price movements were similar between stores on strike and stores not on strike before, during, and after the conflict. After rising a couple percent in the spring and summer of 2003, prices declined a little in the months prior to the strike, then rose over 3 percent in the first half of the conflict before retracing half of that rise in the second half. Overall, divergences in price movements between the two groups of stores were short-lived and not exceptionally large in comparison to other relative price movements over our sample period. After the strike, constraints on stores' ability to adjust prices and to supply goods vanished. Fair pricing motives that could have made retailers reluctant to boost prices amid exceptionally high demand would have greatly eased now that customers could shop freely at all stores. Despite some persistent differences in the quantity indexes, our price indexes show little economically meaningful divergence between the two groups of stores.

[7] We could further measure the supply elasticity by comparing relative price movements to relative quantity movements between stores on strike and stores not on strike, with the caveat that stores on strike may have suffered from supply disruptions in addition to a relative demand shock. The resulting elasticity is essentially zero at $\log(1.336/0.512)/\log(1.021/1.013) = 0.01$.

[8] When we measure the medium-run elasticity of supply using relative movements in prices and quantities from the period before to the period after the strike, we obtain a mildly negative slope estimate, $\log(1.009/1.012)/\log(1.036/0.904) = -0.02$.

To provide a more direct comparison of the level of prices between stores that saw their demand soar and stores that saw their demand collapse, we next look at the cost of purchasing identical baskets of goods. We consider three such baskets. The first basket (the "fixed" basket) consists of all barcodes continuously available at both groups of stores over the two-year period displayed in figure 1, that is, over a period starting 26 weeks before and ending 78 weeks after the beginning of the conflict. The number of units purchased for each barcode is set to its weekly average across all stores over that two-year period. This fixed-quantity basket is reasonably representative of overall purchases in Southern California, accounting for nearly 70 percent of total revenues at the time. At the product category level, the coverage of the basket ranges from 27 percent of total revenues for razors to 95 percent for peanut butter. The second basket (the "on-strike" basket) sets the number of units to actual purchases at stores on strike, again using only barcodes that are continuously available at both groups of stores. Similarly, the third basket (the "not-on-strike" basket) sets the number of units to those actually sold at stores not on strike. Contrary to the fixed basket, the composition of the on-strike and not-on-strike baskets thus varies from week to week according to shoppers' actual purchases at stores on strike and stores not on strike, respectively.

The lower panel of figure 1 reports the cost of purchasing each of the three baskets at the mean transaction price observed at stores on strike relative to that at stores not on strike. (See our online appendix for the formulas.) All three ratios tell the same story: The cost of purchasing any of our baskets at stores on strike relative to stores not on strike hovered near its pre-strike level for the duration of the conflict. A slight increase in the relative price of the baskets at stores that were on strike is apparent several months after the end of the conflict. We are reluctant to attribute this rise in the ratios to a price response to persistently lower demand given the historical variability of the sample.

We note that shoppers at stores on strike could have purchased identical baskets of goods for an equal or even lower price at stores that were not on strike at any point over our two-year period, a finding that suggest some insensitivity to the level of prices on the part of consumers. This conclusion comes with a number of caveats. Because the identity of stores and retail chains is censored, we cannot control for differences in factors such as income and sales taxes that may affect the level of prices across areas. Also, our baskets comprise solely barcodes that are simultaneously available in both groups of stores; by ignoring roughly 30 percent of store revenues in our product categories, we may be overlooking the effect of private labels on the effective costs of a typical basket. Finally, retailers sell items in product categories that are not covered by the IRI sample.

3.1.2 Price adjustments, discounts, and the labor conflict

Promotional discounting is a central feature of supermarket retailing (see, for example, Pesendorfer (2002)). Stores in our sample could have adjusted prices paid by consumers in a number of ways, notably by changing the regular price or by altering the frequency and depth of promotional sales. To shed light on these channels, figure 2 presents some key statistics pertaining to our stores' broader retailing strategy before, during, and after the strike. The mean of each statistic over these periods is reported in table 1. On average, stores on strike saw their weekly frequency of price changes drop 4.3 percentage points during the conflict, with a reduction in regular price adjustments accounting for most of the drop. The fraction of items on sale (the middle-left panel of figure 2) declined 1.2 percentage point to 22.0 percent while the mean saving conditional on a sale (the middle-right panel) was unchanged. In short, stores on strike broadly managed to maintain the importance of sales during the conflict. Stores not on strike experienced a somewhat smaller drop in the frequency of price changes, 3.0 percentage points, that reflected small declines in both sales-related and regular price adjustments.

On the one hand, the continued use of sales by stores whose demand tumbled suggests that engaging in price discrimination is an important endeavor of retailers independently of their level of demand. Perhaps it also reflects a desire from stores on strike to project a business-as-usual image, or an implicit promise to offer bargain shoppers some opportunities to buy a portion of their basket at a discounted price every week. Indeed, it is conceivable that price-sensitive shoppers who continued to patronize stores on strike would have found it unfair to see their loyalty rewarded with higher prices. On the other hand, the reduction in regular price adjustments seems consistent with limited human resources hindering stores' ability to reprice. It is also possible that, given markedly lower sales volumes, stores on strike would have tolerated larger deviations of regular prices from their optimum because fixed repricing costs would have been spread over a smaller number of units sold.[9] We also note that the level of individual prices continued to be coordinated within stores belonging to the same retail chain (the lower-right panel) despite variation across stores in the magnitude of the drop in demand and ability to use managerial staff to fill positions previously held by striking employees. In fact, the share of prices equal to the chain price edged up a couple of percentage points during the strike at both stores on strike and stores not on strike.

As noted above, stores on strike recouped only 4 out of every 5 dollars in lost business

[9] See Golosov and Lucas (2007) for an illustration. The widening of the price inaction region at stores whose demand fell should, in addition to lowering the frequency of price adjustments, boost the absolute size of their price changes. We do find an large increase (4.4 percentage points) in the average size of regular price adjustments. However, a notable increase (1.5 percentage points) is also present at stores that saw their demand jump.

once the conflict ended. To win back customers, they increased the frequency and depth of sales in the ensuing year.[10] The middle panels of figure 2 show that the share of discounted barcodes and the mean saving on the dollar, which captures both the frequency and depth of sales, both rose a couple of percentage points before slowly edging back.[11]

To further explore the strategic use of sales during and after the conflict, we break down discounts into 10-percentage-point bins, starting with discounts that are below or equal to 10 percent, then below or equal to 20 percent but greater than 10 percent, and so on. Figure 3 displays the contribution of discounts in each bin to total revenues (the top row of panels) and to total savings on the dollar (the lower row of panels).[12] The upper-left panel shows that the contribution of items to total revenues generally was declining in the size of the discounts extended. Before the strike, discounts up to 10 percent accounted for almost 8 cents out of every dollar in revenues at stores that were subsequently affected by the conflict, while items offered at discounts in excess of 50 percent accounted for only about 2 cents out of every dollar in revenues. The lower-left panel shows that discounts in the 40 percent to 50 percent range, which were both substantial and frequent, allowed shoppers to lock in the largest savings. By contrast, very small and very large discounts contributed little to actual consumer savings because they were either too small or too infrequent to have a large impact. The distribution of contributions to total revenues and to total savings of discounts had similar shapes in the pre-conflict period at stores unaffected by the strike than at stores affected by the strike, although discounts of medium sizes played a somewhat lesser role in their overall marketing strategy.

During the strike, the number of items on sales declined only a little at affected stores during the strike. The upper-left panel of figure 3 shows that the importance of small and medium discounts for total revenues fell most while that of discounts in excess of 50 percent actually rose some. This shift toward deep discounts may have been part of a marketing strategy to lure shoppers to stores despite the strike, as heavy discounts make for good

[10] In addition to the evidence presented in figure 2, these claims are supported by communications between the affected retailers and their investors. For example, one retailer mentioned in its 2004:Q2 earnings report that "post-strike sales [had] improved, but [had] not reached pre-strike levels," and that it "[continued] to focus on rebuilding sales through promotional pricing [...]."

[11] We measure the mean saving on the dollar as $\left(\sum_j (p_{j,t}^{reg} - p_{j,t}) q_{j,t}\right) / \left(\sum_j p_{j,t}^{reg} q_{j,t}\right)$, where $p_{j,t}^{reg}$ is the regular price of item j in week t identified by our sales filter.

[12] To construct figure 3, we first pool all weekly observations in each of our three time periods (before, during, and after the strike). For the top panels, we then weigh weekly observations by their share of total revenues (that is, $\omega_{i,t} = p_{i,t} q_{i,t} / \left(\sum_{t \in \mathcal{T}} \sum_j p_{j,t} q_{j,t}\right)$, where $\mathcal{T}$ is the set of weeks in the period of interest). The area under the curve integrates to the share of total revenues accounted by items on sale. Similarly, for the lower panels, we weigh weekly observations by their contribution to total savings over the period (that is, $\omega_{i,t} = (p_{i,t}^{reg} - p_{i,t}) q_{i,t} / \left(\sum_{t \in \mathcal{T}} \sum_j p_{j,t}^{reg} q_{j,t}\right)$). The area under the curve then integrates to the mean saving on the dollar.

advertising. That said, the contribution to total savings of heavily discounted items was offset by the lesser importance of small and medium discounts, leaving the mean saving on the entire basket about unchanged. For stores that were not on strike, we also witness a decline in the importance of small and medium discounts during the conflict period but do not find a corresponding rise in the importance of very large discounts.

Once the strike was over, the contribution of small and medium discounts to total revenues and to total savings rose above its pre-strike level at both groups of stores. The importance of discounts in excess of 50 percent also rose at stores that had not been on strike, catching up with the similar increase documented at stores on strike during the conflict. In sum, the evidence is consistent with stores that had been on strike offering somewhat more frequent discounts—small, medium, and large—than usual to win back customers, and stores that had avoided the strike doing the same to retain their new business. Set against the background of differing swings in demand during the strike and beyond, these similarities in the recourse to sales suggest that retailers attach much value to matching changes in their local competitors' pricing strategies.

3.2 A shorter labor conflict: the 2003 St. Louis grocery strike and lockout

The 2003 St. Louis grocery strike and lockout paints a broadly similar portrait as the longer conflict in Southern California: Pricing strategies of stores on strike and stores not on strike broadly resembled each other despite highly diverging demand. It started on October 7 when employees at several supermarket chains went on strike or were locked-out after voting down a tentative labor agreement. The dispute ended 24 days later when negotiating teams reached an agreement that proved acceptable to all parties.

The main effects of the conflict on prices and quantities are shown in figure 4. Due to the smaller size of the St. Louis sample (22 stores compared to 107 stores in Southern California), we compare establishments whose revenues dropped more than 10 percent (generically referred to as "on strike") to all other establishments in the sample (generically referred to as "not on strike"). Picketing was again effective at deterring shopping activities, with stores on strike seeing sales volumes plunge about 45 percent, on average, while stores not on strike experienced a modest increase. Reading through the weekly volatility, prices at stores not on strike rose a touch more around the time of the conflict than prices at stores on strike. However, this slight divergence was part of a broader trend over the two-year period displayed and thus difficultly attributable to the strike. Moreover, price movements on a like-for-like basis were much more similar between the two groups of stores over the period

displayed and around the strike in particular; our relative price measures controlling for the composition of the basket, which are shown at the bottom of figure 4 and employ the same methodology as those computed earlier for Southern California, were very stable.

Of note, sales volumes at stores on strike fully recovered as soon as the labor conflict was over, in contrast with the customer base erosion apparent at stores on strike in Southern California. Given limited household inventories, a 24-day conflict is arguably too long for customers unwilling to cross picket lines at their usual supermarket not to go shopping elsewhere. The recovery in sales volumes thus suggests some stickiness in consumer preferences for particular establishments that are not captured in standard models of store switching costs such as Kleshchelski and Vincent (2009), where store changes are permanent. In any case, the quick recovery in sales volumes probably explains why we did not observe a rise in the frequency and depth of discounts (shown in the middle panels of figure 5 and documented in table 2) in the wake of the St. Louis conflict.

4 Major weather events as exogenous demand shocks

Our next set of demand shocks were created by Mother Nature; their occurrence was unambiguously exogenous to retail activities in general and to supermarkets' pricing strategies in particular. We first look at Hurricane Katrina that, in the span of a few tragic weeks in the summer of 2005, led to massive population displacement. Stores located in areas that received an inflow of refugees experienced a sharp rise in store traffic that persisted for well over a year. We then look at shopping sprees triggered by snowstorms and hurricanes. Contrary to strikes and Hurricane Katrina, these storms do not feature a reconfiguration of retailers' customer base but rather a temporary increase in the demand of all customers.

4.1 Hurricane Katrina

In August 2005, Hurricane Katrina created an estimated $108 billion in property damages (in 2005 dollars), making it the most expensive natural disaster in U.S. history. It was also directly responsible for the tragic loss of about 1,200 lives and the displacement of roughly 1 million persons.[13] The city of New Orleans, Louisiana, sustained the most extensive damage due to the failure of its levee system, which led to the flooding of approximately 80 percent of the city. The flooding of residential areas made it impossible for many displaced households to return home for several months or years, and some households even chose to permanently relocate elsewhere. According to research conducted at the U.S. Census Bureau and reported

[13] These estimates are taken from Blake and Gibney (2011).

in Geaghan (2011), as of late 2009, 31,500 households in the New Orleans metropolitan area (7 percent of the area's total) did not consider themselves permanently resettled.[14]

The hurricane had disruptive consequences on retail activities in New Orleans and other affected states. Of the 23 New Orleans stores that participated in the IRI sample on the eve of the tragedy, five exited the sample as soon as the storm hit while one store ceased to report data for a period of eight months. Similarly, five out of the nine stores in the Mississippi sample did not report data for a week or two around the hurricane. Although the IRI dataset contains no information that would permit us to ascertain that these sample exits and missing reports were caused by the hurricane, we interpret their coincidental timing as strongly suggestive that they were.

As figure 6 shows, the hurricane also affected retail activities at stores that remained in business. In the weeks and months that followed the disaster, stores that continued to report data to the IRI experienced sales volumes that were, on average, about 20 percent higher than before the disaster. We found an equally large rise in the smaller Mississippi sample (not shown); a smaller effect is also apparent for the Houston, Texas, sample (also not shown). We interpret these persistent rises as evidence that the hurricane displaced shopper traffic toward establishments located in areas that were relatively unaffected by the storm.[15] Supporting our interpretation is the fact that, according to Geaghan (2011), 74 percent of displaced New Orleans householders reported living with an acquaintance.[16] In addition, the persistent increase in sales volumes of food products as well as housekeeping and personal care products, which are shown separately in figure 6, were of a similar proportion, consistent with mass relocation boosting the demand of supermarkets' entire product offering. Indeed, we observe increases in all 29 product categories in the sample. The upper-left panel of figure 6 also features a short-lived but outsized 60-percent surge in sales volumes of housekeeping supplies and personal care products the precise week that Hurricane Katrina hit. Revenues from product categories such as toothbrushes and razors witnessed transitory increases in

[14] Three weeks after Hurricane Katrina, authorities in Texas ordered 1.8 million persons to evacuate coastal areas along the Gulf of Mexico ahead of Hurricane Rita's landfall. Transitory jumps in sales volumes are apparent for the main markets that hosted refugees. We exclude this storm from our analysis because the effects of mass population relocation cannot be disentangled from those of shopping sprees ahead of the storm by households sheltering in place. We will explore this latter phenomenon in section 4.2.

[15] Whether market-wide demand also rose is less clear, at least initially. On the one hand, the arrival of a hurricane typically boosts spending on a wide range of products, a phenomenon that we document in section 4.2. On the other hand, mass population exodus in the months and years after Hurricane Katrina arguably depressed market-wide demand, even if demand at continuing stores may have been persistently higher than in the past.

[16] The fact that most refugees sought shelter with family members and friends suggests a similarity in consumer characteristics between displaced and hosting households. See Lach's (2007) analysis of a mass migration from the former Soviet Union to Israel for evidence that large influx of consumers with price elasticities and search costs differing from the native population can affect retail prices.

excess of 100 percent, again consistent with population displacement being a key driver of retail activities over the period.

Our empirical evidence provides little if any support to the view that retailers took advantage of higher demand brought about by the hurricane to raise prices, either initially when spending on personal care products skyrocketed or over the medium run when store traffic was boosted by mass relocation. The upper-right panel of figure 6 shows that the price of food products and of personal care and housekeeping products both rose in the weeks that followed the storm before erasing some of these gains. When we average over the period covered by the federal emergency declaration, we find that prices were 1.4 percent higher overall than they were over the 26-week period before the hurricane. As table 3 reports, this increase is modestly larger than the average rise across IRI markets not directly affected by Hurricane Katrina (that is, excluding New Orleans, Mississippi, and Houston). Muted price movements translate into small estimates of the short-term elasticity of supply. If we measure the price impact as the rise in the New Orleans price index in excess of the average rise for IRI markets not directly affected by Hurricane Katrina, then our estimate of the (short-run) elasticity of supply is 0.03.

When we compute the elasticity of supply using price and quantity movements over the longer period after the federal emergency relative to the pre-storm period, we get a supply elasticity estimate of 0.13. However, we are reluctant to interpret this medium-run estimate as suggestive that retailers took advantage of higher demand to boost prices for two main reasons. First, this estimate may be biased upward by hurricane-related disruptions to the region's food supply. Second, price pressure was not broad based, as the price of housekeeping and personal care products rose in line with the national average.

The remaining panels of figure 6 provide further results regarding the impact of the hurricane on retailers' broader pricing strategy. The frequency of price changes edged down during the federal emergency period, and reverted to its pre-hurricane average for about a year before sliding in the fall of 2006. The hurricane had a somewhat more apparent effect on the mean saving on the dollar, which temporarily slid from between 5 and 6 percent of the regular price in the weeks before the hurricane to a low near 2 percent, before rebounding at the end of the federal emergency declaration. Much of this decline reflects a lower proportion of items on sales, especially in late September and early October, rather than a shift in customer spending toward items with lesser or no discounts, as hinted by the relative stability of both the mean saving on the dollar and the share of items on sale in the first couple weeks after the storm.

One may suspect that the absence of significant upward movements in prices during or after Hurricane Katrina could reflect legal impediments to price adjustments. Indeed, on

August 26, 2005, Louisiana Governor Kathleen Babineaux Blanco proclaimed a 30-day state of emergency that automatically triggered a law prohibiting price gouging. The law stated that "prices charged or value received for goods and services sold [...] may not exceed the prices ordinarily charged for comparable goods and services in the same market area at or immediately before the time of the state of emergency." These price restrictions were allowed to lapse on September 25, 2005, even though the state of emergency was systematically renewed through 2007. Figure 6 does not suggest that they were a material constraint on prices, as their expiration was not associated with unusual upward movements in prices. For the period covered by the restrictions, it is conceivable that retailers had some room to adjust prices upward by limiting the number and the depth of sales and promotions. Indeed, as noted above, we do observe some decline in the mean saving on the dollar and share of items on sales early after the tragedy. However, we are reluctant to directly attribute these movements to a response to legal price restrictions because of a number of other factors at play. For instance, hurricane-related supply chain disruptions could have increased the cost of bringing goods to consumers; the law against price gouging explicitly permitted the pass-through of such costs to consumers. Similarly, stockouts of items on sale could have contributed to the fall in the mean saving on the dollar as households turned to consuming non-sale items in greater proportion.

4.2 Major snowstorms and hurricanes

Major weather events such as large snowstorms and hurricanes can affect the demand for supermarket products through several channels. Storms that result in the closing of schools and workplaces force households to consume a greater proportion of their meals at home, thus boosting demand for food items. Similarly, the demand for personal care and housekeeping products may rise as households engage in greater home production or take advantage of their trip to the supermarket to purchase items other than food. Storms may also displace consumption across time periods by making it difficult or impossible to shop on certain days. In particular, some households may seek to build their domestic inventories in anticipation of a storm to ensure continued supplies, while others may need to replenish their inventories once the storm is over.

4.2.1 Identification

We have identified 59 combinations of an IRI market and a major snow episode whose disruptive consequences were favorable to the triggering of a shopping spree over the IRI sample period. Many of these combinations feature a peak in average store revenues of 10 percent or

more relative to the previous few weeks. While there were hundreds of smaller snowstorms in our sample, we expressly choose to leave them aside because storms whose disruptive effects last only a couple of days are less likely to leave a clear imprint in supermarket data collected weekly.

In identifying disruptive snowstorm episodes, we account for the fact that some localities have a greater ability to cope with snowfall than others. Snow accumulations that have crippling effects in Southern states, where snowstorms are scarce and snow plowing equipment is in short supply, may have only limited disruptive effects in Northern states, where local authorities are accustomed to clearing snow off the streets rapidly. To do so, we match our IRI scanner data with the U.S. National Oceanic and Atmospheric Administration's Regional Snowfall Index (RSI) and the Federal Emergency Management Agency's (FEMA) list of federal disaster declarations. The RSI controls for differences in historical snow precipitation and the authorities' ability to cope with snow through the setting of precipitation thresholds that are specific to nine U.S. regions (the West Coast of the United States is not covered due to insufficiently frequent snowstorms). Storms whose social impact is roughly in the top half of historical storms in their region are given a rank between 1 and 5. Many of the snow episodes in our sample were ranked "3–major," "4–crippling," or "5–exceptional," placing them in the top 5 percent of storms in terms of regional-level disruptiveness. Many storms were also granted an "emergency" or a "disaster relief" status by FEMA because they were of "such severity and magnitude that effective response [was] beyond the capabilities of the State and the local governments and that Federal assistance [was] necessary."[17]

We validate our list of snow episodes against daily snowfall measurements reported by local weather stations to avoid situations in which a disruptive storm at the regional level results in little snow accumulation or passes as rain in a particular market. We also use local daily snowfall measurements to include a number of storms that likely had large localized effects but whose regional impact, as measured by the RSI, was small. In a few cases, our snow episodes cover two snowstorms rather than one because the separate meteorological systems are indistinguishable in weekly data. Finally, we incorporate a few major snowstorms from the U.S. West Coast for which RSI scores are not available. The list of snowstorms, along with their cumulative snowfall, RIS classification, and FEMA declaration, is provided in our online appendix.

We follow a similar strategy for identifying hurricanes that are likely to induce shopping sprees. We look at all "emergency" and "major disaster" declarations by FEMA that are

[17] Disaster declarations put into motion short- to long-term federal relief, some of which may be directed to individuals. Emergency declarations are more limited in scope and seek to meet specific emergency needs or to help prevent major disasters from occurring. Both types of declaration require presidential approval.

attributed to a hurricane. We then validate our list against daily rainfall and maximum wind speed measurements from local weather stations.[18] In total, we have identified 21 combinations of an IRI market and a hurricane, which we also list in our online appendix.

4.2.2 Illustration: 2009–2010 winter in Washington, D.C.

Figure 7 illustrates some effects of major storms on retail activities using two snow episodes that hit Washington, D.C., during the 2009–2010 winter. The first episode began on Friday, December 18, 2009, and left 41.7 centimeters (16.4 inches) of snow at D.C.'s Reagan National Airport. Federal offices were closed the following Monday and operated on an unscheduled leave basis for two more days due to impracticable roads in parts of the metropolitan area. The second snow episode was more disruptive, consisting of two back-to-back blizzards that together blanketed the U.S. capital with 72.6 centimeters (28.6 inches) of snow. Federal offices closed early as the first blizzard moved in on Friday, February 5, 2010, remained closed through February 12, and then operated on an unscheduled leave basis through February 16. As is typical of the episodes in our sample, the National Weather Service and local media began reporting on the approaching snowstorms several days ahead of their occurrence.

The upper-left panel of figure 7 shows that sales volumes peaked 20 to 45 percent above their trend in the December 2009 and February 2010 snow episodes. The timing of the surge in quantities differs between the two episodes, although the use of weekly retail data limits our ability to identify their timing precisely (note that IRI weeks run from Monday to Sunday). As is apparent for both episodes, the quantity of food products and of personal care and housekeeping products spiked around the storms, supporting our treatment of major snowstorms as shocks to the overall demand of supermarkets rather than as shocks specific to some product categories. In the first episode, quantities of both groups of products rose 10 percent in the week of the storm relative to their recent trend, and rose even more in the ensuing week. In the second episode, sales volumes surged 27 percent for personal care and housekeeping products in the week encompassing the beginning of the second episode and 45 percent for food products. The demand for food products remained above its recent trend during the ensuing week. The large impact of the second storm may be due to the anticipation of greater snow totals (and, if our experience is representative, it could also reflect some learning from the first episode that shopping in advance of the storm avoids some headaches).

[18] We exclude Hurricane Katrina and Hurricane Rita due to their exceptional mass population displacement, a phenomenon that we analyzed separately in section 4.1. We also drop most observations related to Hurricane Ike and Hurricane Lili because they came on the heel of other disruptive hurricanes that required federal assistance. Our regression results are robust to keeping these latter hurricanes in the sample.

The two snowstorm episodes left no apparent imprint on price indexes, as shown in the upper-right panel. Similarly, the frequency of price changes, the mean saving on the dollar, the share of items on sales, and the share of store revenues derived from items on sales were all within the range experienced over the 2009–2010 winter. Our econometric analysis below, on the broader sample of snowstorms and hurricanes, confirms these impressions.

4.2.3 Econometric analysis

There is some uncertainty regarding the precise timing of when weekly retail activities should feel the effects of storms most strongly. Storms that hit early in the week or whose disruptive effects are anticipated may affect retail activities prior to the storm. Similarly, storms that hit late in the week or that require domestic inventory rebuilding by households afterwards could have some effect in the week after the storm. As a first step into our investigation, we ignore this timing issue and compare various retailing statistics in the week corresponding to the observed peak in quantities to their respective average in the weeks prior to the storm. More precisely, for a storm beginning in week t, we identify the peak in quantity over the weeks $t-1$, t, and $t+1$ and then compare the statistic of interest for that peak week by its average over the weeks $t-4$ to $t-2$ (the "pre-storm period").[19]

Our sample of snow episodes supports the patterns apparent in figure 7, namely that major snowstorms boost consumer spending on supermarket items while having little if any influence on pricing. The mean peak in quantities around snowstorms is 12.9 percent higher than the average over the pre-storm period, whereas the peak in prices is only 0.1 percent higher. A simple statistical test that the population mean of the ratio across snow episodes equals 1 is rejected for quantities but not for prices. The peak quantity and price responses, as well as their statistical significance, are nearly identical for hurricanes. For both types of storms, we also find similar statistics on broader features of our stores' pricing strategies between the week of the peak in quantities and the pre-storm period. This remarkable finding suggests that retailers broadly retain their usual pricing strategies.

We next follow a regression-based approach similar to that used by Chevalier, Kashyap, and Rossi (2003) in their study of demand peaks around holidays. We regress our statistics of interest (illustrated here with the log of our market-specific price index) on a quadratic time trend and a set of dummies marking the week immediately before ($W_{c,t}^{pre}$), the week during ($W_{c,t}^{during}$), and the week after ($W_{c,t}^{after}$) a storm,

$$\log\left(P_{c,t}^{F}\right) = \alpha_{c,0} + \alpha_{c,1}t + \alpha_{c,2}t^2 + \beta_{-1}W_{c,t}^{pre} + \beta_0 W_{c,t}^{during} + \beta_1 W_{c,t}^{after} + \varepsilon_{c,t}.$$

[19] We normalize the quantity and price indexes in each market by dividing their value during the quantity peak week by their average during the reference period.

The estimated coefficients on the week dummies can be interpreted as the average movement in the left-hand side statistic (relative to its trend) across the various storm episodes in the sample. To ensure that our quadratic trend fits the level of the individual series properly, we retain only data from the first week in October through the last week in March in the case of snowstorms and from the first week of July to the last week of December in the case of hurricanes (there are no qualifying storms outside of these broadly-defined snowstorm and hurricane seasons). We next fit a quadratic time trend that is specific to each IRI market and 6-month season combination, represented by the subscript c in the above equation. We then run separate regressions for snowstorms and hurricanes.

Table 4 reports the results for our key statistics of interest. For major snowstorms, we find that the boost to quantities occurs almost entirely in the week of the storm. We also observe a statistically-significant small decline in quantities of about 1.9 percent in the week after the storm. This decline is suggestive that greater shopping activities during snowstorms result in the bringing forward of some household expenditures, leading to a small pull back in the week immediately after. The corresponding estimates for prices does not suggest any changes. In the case of hurricanes, we find a statistically significant boost to spending in both the week immediately before and the week of the storm. This finding suggests that hurricanes primarily pull forward consumption expenditures, perhaps due to a greater predictability of these events and a greater risk that households could experience reduced supplies for a protracted period. A pull back is also observed in the week immediately after a hurricane, although it is not statistically significant at standard confidence levels.

The absence of a price response does not appear to be due to legal impediments against price increases. Indeed, Zwolinski (2008) lists 33 U.S. states restricting price rises during declared emergencies. The laws typically seek to prevent opportunistic rises in the price of gasoline, hotel rooms, construction materials, and food items that are most susceptible to cause public uproar. Although restrictive, nearly all of the laws explicitly permit price increases that are attributable to additional costs or expenses related to an emergency; 12 states even explicitly permit price increases that do not exceed some value between 10 to 25 percent. Price setters may also have some room to raise prices by reducing the depth or frequency of sales. Price increases are typically measured against the prices "ordinarily charged" or observed over some period prior to the disaster. Therefore, a retailer may not break the law if it were to raise effective prices by diminishing the frequency or depth of sales and promotions. In any case, as a check of the importance of price gouging laws, we added to our regression a set of law dummies interacted $W_{c,t}^{pre}$, $W_{c,t}^{during}$, and $W_{c,t}^{after}$. We could not reject the hypothesis that price responses in markets not subject to laws against price gouging were the same as price responses in markets subject to such law.

The observation that retailers deviate little if at all from their usual pricing strategies during major storms contrasts with the finding that prices tend to fall during periods of peak demand, in particular short-lived booms around holidays (see Warner and Barsky (1995), MacDonald (2000), and Chevalier, Kashyap, and Rossi (2003)). The difference could reflect a number of factors. Notably, whereas the timing of holidays is perfectly predictable, the occurrence of a major snowstorm or a hurricane can be anticipated at most a week or so in advance, and only with great uncertainty. This limited predictability may not leave enough time for manufacturers to adjust production or for retailers to implement "loss-leader" pricing strategies to lure customers into their stores. Circulars may already have been printed and, in any case, there can be lags of several months in the planning of sales and promotions (see Anderson *et al.* (2013) for a discussion). It is also conceivable that the ability to advertise and shop ahead of holildays makes the demand more price elastic in those times, at least for adverstized products, which in turn could push down markups.

5 What factors account for a flat retail supply curve?

We have shown that supermarkets do not boost prices in response to peaks in demand brought about by labor conflicts, mass population relocation, and shopping sprees around storms and hurricanes. This evidence points to flat short- to medium-run supply curves in the retail sector. In this section, we discuss some factors that might contribute to this flatness.

We first note that information friction theories do not seem to offer a plausible explanation. Our shocks were easily observable, benefiting from extensive media coverage. For this reason, it seems inconceivable that supermarkets would have failed to adjust prices in response to a labor conflict or a major storm because they were either unaware its occurrence (as would be the case with Mankiw and Reis' (2002) sticky information model) or because they were overly focused on idiosyncratic shocks (as would be the case in Maćkowiak and Wiederholt's (2009) rational inattention model). Our evidence also does not support the idea that price gouging laws, where present, were important impediments to price increases. In the case of Hurricane Katrina, the expiration of anti-gouging provisions four weeks after the disaster struck was not accompanied by significant price rises. We also find no evidence in support of larger price responses to snowstorms in markets where no such laws exist relative to markets where they do.

The applicability of countercyclical markup theories based on countercyclical collusion (Rotemberg and Saloner (1986)) or economies of scale in consumer search (for example, Bils (1989) and Warner and Barsky (1995)) seems limited in our context. In the case of

countercyclical collusion, the retail markup falls when demand exogenously rises because retailers have greater incentives to deviate from cartel prices. In constrast with this prediction, the number of establishments where households were either willing or able to shop arguably declined during our labor conflicts and Hurricane Katrina, which could in principle have made collusion easier to sustain. In the case of economies of scale in consumer search, consumers increased their search intensity in periods of high demand, such as during holiday seasons, making their demand more elastic than in normal times and thus limiting retailers' ability to enjoy high margins. Although shopping intensity jumped around our major snowstorms and hurricanes, households arguably had limited time to search at multiple establishments. As a result, their demand may have been less elastic than otherwise, in constrast with the assumption at the heart of the theory.

A number of other theories seem consistent with some but not all of our findings. Notably, the lack of a price response to unanticipated, short-lived demand shocks could be consistent with standard sticky-price and sticky-plan theories, and is supported by evidence of lags in retailers' planning of sales and promotions and infrequent regular prices adjustments presented in Anderson *et al.* (2013). That said, rigidities in prices or in pricing strategies do not appear to be a fully satisfactory explanation for the lack of a stronger price response to our most persistent shocks—namely the Southern California labor conflict and Hurricane Katrina—for which supermarkets often adjusted item prices several times but did not, on net, raise prices significantly.

Theories emphasizing the customer base as an asset for retailers also get some partial backing from our findings to the extent that one interprete the assumption of consumer loyalty to specific items or brands in terms of loyalty to particular establishments or retail chains. These theories include switching costs (for example, Klemperer (1995) and Kleshchelski and Vincent (2009)) and models of deep habits (Ravn, Schmitt-Grohé, and Uribe (2006)). Consistent with these theories, the effect of the Southern California strike and lockout on store traffic was not fully reversed at the end of the conflict. However, the effect was much less pronounced than posited by these theories. Moreover, customer base losses can take time to materialize, as evidenced by the full bounce back in sales volumes at stores impacted by the relatively short St. Louis conflict. In addition, these theories could have difficulties explaining the lack of a strong response to our shortest-lived shocks, as they imply that demand is much less elastic in the short run than over the long run.

Overall, we retain two explanations that appear broadly consistent with our findings. The first explanation is that retailers' marginal costs and markups are insensitive to the level of demand. Although we do not observe these variables in our dataset, a number of elements support the plausibility of this explanation. As noted in section 3, goods acquisition costs

account for the bulk of supermarket expenses. Given the regional character of our shocks, the preponderance of national brand in our sample, and the ability of most of our retailers' to move stocks across regions, it is conceivable that acquisition costs could remain fairly stable even as local demand fluctuates. With respect to markups, Eichenbaum, Jaimovich, and Rebelo (2011) show that item-level markups vary little around their mean using data from a large U.S. supermarket chain. Relatedly, Fabiani *et al.* (2006) provide evidence of the pervasiveness of constant markup rules based on a large survey of European firms. The micro evidence on the sources of variations in exchange rates is further supportive of limited variation in item markups; using data from a leading North American retailer, Gopinath *et al.* (2011) show that variation in markups account for none of the cross-border variation in retail prices between Canada and the United States.

The second explanation is that fairness motives may be at play. Boosting prices to take advantage of high demand could risk antagonizing customers, leading to longer-term losses that more than offset any short-run benefits. For instance, Kahneman, Knetsch, and Thaler (1986) analyze responses to public opinion surveys and conclude that households perceive firms that raise prices in response to shifts in demand as acting unfairly.[20] For our major snowstorms and hurricanes, retailers may have wanted to avoid being perceived as unjustly profiting off their customers' unusually high marginal utility for fear of losing their business in the future. That said, we note that our sample excludes products whose consumption is essential during storms, such as de-icing salt, batteries, or snow shovels. Our sample instead contains products consumed year-round and for which several brands are typically available. A retailer that would want to take advantage of temporarily high demand may thus have to raise prices over a broad product offering. This feat could be achieved by reducing the number or depth of sales rather than by raising regular prices but our results offer only limited support for these channels.

6 Conclusion

We have shown that the level of supermarket prices responds little to large swings in demand brought about by labor conflicts, mass population relocation, and shopping sprees around storms and hurricanes. This evidence is consistent with flat short- to medium-term supply curves in the retail sector. In particular, the evidence seems inconsistent with the marginal cost of retailers being sensitive to the level of demand because of fixed factors of production.

[20] This conclusion is supported by the subsequent literature; see the review by Xia, Monroe, and Cox (2004). See also Rotemberg (2005, 2011) for an exploration of the pricing and promotional sales implications of fairness within the context of a modern microfounded pricing model.

It seems sensible to conjecture that our low supply elasticity estimates could reflect relatively steady marginal costs and markups, or possibly a fear of antagonizing customers through price changes perceived as unfair.

If our retailers responded little to large demand shocks overall, they did seem concerned with keeping up with the pricing strategies of their local competitors. This fact was most clearly seen during and after the Southern California supermarket strike and lockout, when retailers with radically different demand shocks nonetheless matched their local competitors' pricing movements and recourse to sales of various depths.

The above observations invite a reconsideration of the place occupied by the retail sector in macro models. Many modelers conflate the notion of producers and retailers, and then calibrate their model to match the pricing features of retailers only. Our analysis suggests that the retail sector's short- to medium-term supply curve is perhaps much flatter than that of other sectors such as manufacturing; this finding in line with the low-margin, high-volume nature of the retail industry. If so, then a better understanding of pricing behavior and deviations from constant returns to scale at lower levels of the production chain seem much needed.

References

[1] Anderson, Eric, Emi Nakamura, Duncan Simester, and Jón Steinsson (2013). "Informational Rigidities and the Stickiness of Temporary Sales," NBER Working Papers 19350, National Bureau of Economic Research, Inc.

[2] Bils, Mark (1989). "Pricing in a Customer Market," *Quarterly Journal of Economics*, vol. 104(4), pages 699–717.

[3] Bils, Mark and Peter J. Klenow (2004). "Some Evidence on the Importance of Sticky Prices," *Journal of Political Economy*, vol. 112(5), pages 947–985.

[4] Blake, Eric S. and Ethan J. Gibney (2011). "The Deadliest, Costliest, and Most Intense United States Tropical Cyclones from 1851 to 2010 (and Other Frequently Requested Hurricane Facts)," NOAA Technical Memorandum NWS NHC-6, U.S. National Oceanic and Atmospheric Administration.

[5] Bronnenberg, Bart J., Michael W. Kruger, and Carl F. Mela (2008). "Database Paper: The IRI Marketing Data Set," *Marketing Science*, vol. 27(4), pages 745–748.

[6] Burstein, Ariel and Christian Hellwig (2007). "Prices and Market Shares in a Menu Cost Model," NBER Working Papers 13455, National Bureau of Economic Research, Inc.

[7] Cavallo, Alberto, Eduardo Cavallo, and Roberto Rigobon (2013). "Prices and Supply Disruptions during Natural Disasters," NBER Working Papers 19474, National Bureau of Economic Research, Inc.

[8] Chevalier, Judith A., Anil K. Kashyap, and Peter E. Rossi (2003). "Why Don't Prices Rise During Periods of Peak Demand? Evidence from Scanner Data," *American Economic Review*, vol. 93(1), pages 15–37.

[9] Coibion, Olivier, Yuriy Gorodnichenko, and Gee Hee Hong (2015). "The Cyclicality of Sales, Regular and Effective Prices: Business Cycle and Policy Implications," *American Economic Review*, vol. 105(3), pages 993–1029.

[10] Dhyne, Emmanuel, Luis J. Álvarez, Hervé Le Bihan, Giovanni Veronese, Daniel Dias, Johannes Hoffmann, Nicole Jonker, Patrick Lünnemann, Fabio Rumler, and Jouko Vilmunen (2005). "Price Setting in the Euro Area: Some Stylized Facts from Individual Consumer Price Data," Working Paper 524, European Central Bank.

[11] Eichenbaum, Martin, Nir Jaimovich, and Sergio Rebelo (2011). "Reference Prices, Costs, and Nominal Rigidities," *American Economic Review*, vol. 101(1), pages 234–262.

[12] Fabiani, Silvia, Martine Druant, Ignacio Hernando, Claudia Kwapil, Bettina Landau, Claire Loupias, Fernando Martins, Thomas Mathä, Roberto Sabbatini, Harald Stahl, and Ad Stokman (2006). "What Firms' Surveys Tell Us about Price-Setting Behavior in the Euro Area," *International Journal of Central Banking*, vol. 2(3), pages 3–47.

[13] Federal Emergency Management Agency, "Declared Disasters by Year or State," last accessed April 20, 2015, https://www.fema.gov/disasters.

[14] Gagnon, Etienne, David López-Salido, and Nicolas Vincent (2013). "Individual Price Adjustment along the Extensive Margin," chapter in: *NBER Macroeconomics Annual 2012*, vol. 27, pages 2450–2486.

[15] Geaghan, Kimberly A. (2011). "Forced to Move: An Analysis of Hurricane Katrina Movers 2009 American Housing Survey: New Orleans," Social, Economic, and Housing Statistics Division Working Paper Number 2011-17, U.S. Census Bureau.

[16] Golosov, Mikhail and Robert E. Lucas Jr. (2007). "Menu Costs and Phillips Curves," *Journal of Political Economy*, vol. 115, pages 171–199.

[17] Gopinath, Gita, Pierre-Olivier Gourinchas, Chang-Tai Hsieh, and Nicholas Li (2011). "International Prices, Costs, and Markup Differences," *American Economic Review*, vol. 101(6), pages 234–262.

[18] Gopinath, Gita and Oleg Itskhoki (2010). "Frequency of Price Adjustment and Pass-Through," *Quarterly Journal of Economics*, vol. 125 (2), pages 675–727.

[19] Hong, Gee Hee and Nicholas Li (2013). "Market Structure and Cost Pass-Through in Retail," Working Papers tecipa-470, University of Toronto, Department of Economics.

[20] Kahneman, Daniel, Jack L. Knetsch, and Richard Thaler (1986). "Fairness as a Constraint on Profit Seeking: Entitlements in the Market," *American Economic Review*, vol. 76(4), pages 728–41.

[21] Karadi, Peter and Adam Reiff (2012)."Large shocks in menu cost models," Working Paper Series 1453, European Central Bank.

[22] Klemperer, Paul (1995). "Competition When Consumers Have Switching Costs: An Overview with Applications to Industrial Organization, Macroeconomics, and International Trade," *Review of Economic Studies*, vol. 62(4), pages 515–539.

[23] Klenow, Peter J. and Oleksiy Kryvtsov (2008). "State-Dependent or Time-Dependent Pricing: Does It Matter for Recent U.S. Inflation?," *Quarterly Journal of Economics*, vol. 123(3), pages 863–904.

[24] Kleshchelski, Isaac and Nicolas Vincent (2009). "Market Share and Price Rigidity," *Journal of Monetary Economics*, vol. 56(3), pages 344–352.

[25] Kruger, Michael W. and Daniel Pagni (2012). *IRI Academic Data Set Description*, version 2.0, Chicago: Information Resources Incorporated.

[26] Lach, Saul (2007). "Immigration and Prices," *Journal of Political Economy*, vol. 115(4), pages 548–587.

[27] MacDonald, James M. (2000). "Demand, Information, and Competition: Why Do Food Prices Fall at Seasonal Demand Peaks?," *Journal of Industrial Economics*, vol. 48(1), pages 27–45.

[28] Maćkowiak, Bartosz and Mirko Wiederholt (2009). "Optimal Sticky Prices under Rational Inattention." *American Economic Review*, vol. 99(3), pages 769–803.

[29] Mankiw, N. Gregory and Ricardo Reis (2002). "Sticky Information Versus Sticky Prices: A Proposal To Replace The New Keynesian Phillips Curve," *Quarterly Journal of Economics*, vol. 117(4), pages 1295–1328.

[30] Nakamura, Emi (2008). "Pass-Through in Retail and Wholesale," *American Economic Review: Papers & Proceedings*, vol. 98(2), pages 430–437.

[31] Nakamura, Emi and Jón Steinsson (2008). "Five Facts about Prices: A Reevaluation of Menu Cost Models," *Quarterly Journal of Economics*, vol. 123(4), pages 1415–1464.

[32] National Climate Data Center, "NCDC Climate Data Online: Daily Surface Data," last accessed April 20, 2015, https://www.ncdc.noaa.gov/cdo-web/.

[33] Ravn, Morten, Stephanie Schmitt-Grohé, and Martín Uribe (2006). "Deep Habits," *Review of Economic Studies*, Oxford University Press, vol. 73(1), pages 195–218.

[34] Rotemberg, Julio J. (2005). "Customer anger at price increases, changes in the frequency of price adjustment and monetary policy," *Journal of Monetary Economics*, vol. 52(4), pages 829–852.

[35] Rotemberg, Julio J. (2011). "Fair Pricing," *Journal of the European Economic Association*, vol. 9(5), pages 952–981.

[36] Rotemberg, Julio J. and Garth Saloner (1986). "A Supergame-Theoretic Model of Price Wars during Booms," *American Economic Review*, vol. 76(3), pages 390–407.

[37] Shea, John (1993). "Do Supply Curves Slope Up?," *Quarterly Journal of Economics*, vol. 108(1), pages 1–32.

[38] Warner, Elizabeth J. and Robert B. Barsky (1995). "The Timing and Magnitude of Retail Store Markdowns: Evidence from Weekends and Holidays," *Quarterly Journal of Economics*, vol. 110(2), pages 321–352.

[39] Xia, Lan, Kent B. Monroe, and Jennifer L. Cox (2004). "The Price Is Unfair! A Conceptual Framework of Price Fairness Perceptions," *Journal of Marketing*, vol. 68(4), pages 1–15.

[40] Zwolinski, Matt (2008). "The Ethics of Price Gouging," *Business Ethics Quarterly*, vol. 18(3), pages 347–378.

Table 1: Mean statistics before, during, and after the 2003–2004 Southern California supermarket strike

Statistic	On Strike			Not on Strike		
	Before	During	After	Before	During	After
Quantity index	1.000	0.513	0.904	1.000	1.336	1.036
	(0.004)	(0.020)	(0.004)	(0.005)	(0.018)	(0.006)
Price index	1.000	1.013	1.012	1.000	1.021	1.009
	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.001)
Frequency of posted price changes	45.9	41.6	48.1	34.1	31.1	32.5
	(0.4)	(0.6)	(0.3)	(0.3)	(0.4)	(0.3)
Frequency of regular price changes	20.0	16.1	19.4	15.2	13.4	12.7
	(0.3)	(0.4)	(0.3)	(0.2)	(0.3)	(0.2)
Share of items on sale	23.2	22.0	25.0	18.1	16.8	18.9
	(0.3)	(0.5)	(0.2)	(0.2)	(0.3)	(0.2)
Mean saving on the dollar	9.4	9.4	10.2	8.3	6.8	8.5
	(0.1)	(0.2)	(0.2)	(0.1)	(0.2)	(0.1)
Share of revenues from items on sale	30.4	28.2	31.4	27.3	24.2	28.1
	(0.4)	(0.7)	(0.3)	(0.4)	(0.4)	(0.3)
Share of prices equal to chain price	53.2	55.8	51.9	59.9	62.4	63.5
	(0.4)	(0.5)	(0.3)	(0.3)	(0.3)	(0.2)
Addendum						
Price index, IRI sample ex. striking markets	*1.000*	*1.004*	*1.019*	*1.000*	*1.004*	*1.019*

Notes: The table reports the mean of the time-series statistics shown in figures 1 and 2, along with their standard deviation in parentheses, over three distinct periods. The label "before" corresponds to the 26-week period immediately before the strike, the label "during" to the 20-week period of the strike, and the label "after" to the 59-week period immediately after the strike. The price and quantity indexes are scaled such that their geometric mean equals 1 in the "before" period; all other statistics are in percent. The price index in addendum applies to the full IRI sample excluding the Los Angeles, San Diego, and St. Louis markets.

Table 2: Mean Statistics before, during, and after the 2003 St. Louis supermarket strike

Statistic	**On Strike**			**Not on Strike**		
	Before	During	After	Before	During	After
Quantity index	1.001	0.566	0.967	1.002	1.053	0.939
	(0.007)	(0.060)	(0.004)	(0.011)	(0.022)	(0.008)
Price index	1.000	1.001	1.005	1.000	1.011	1.032
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Frequency of posted	24.7	22.5	24.9	22.6	21.1	22.6
price changes	(0.3)	(3.1)	(0.2)	(0.3)	(0.8)	(0.2)
Frequency of regular	6.3	5.6	6.5	5.6	3.9	5.1
price changes	(0.3)	(1.4)	(0.2)	(0.3)	(0.3)	(0.1)
Share of items	19.7	17.9	19.5	17.8	18.4	18.2
on sale	(0.2)	(0.5)	(0.1)	(0.3)	(0.1)	(0.2)
Mean saving on the dollar	9.7	9.1	10.3	9.6	10.8	10.8
	(0.2)	(0.5)	(0.1)	(0.1)	(0.4)	(0.1)
Share of revenues	28.5	23.5	28.8	26.5	28.3	28.1
from items on sale	(0.4)	(0.9)	(0.2)	(0.4)	(0.5)	(0.3)
Share of prices	94.3	92.7	94.0	87.5	87.3	87.7
equal to chain price	(0.2)	(1.1)	(0.2)	(0.4)	(0.4)	(0.3)
Addendum						
Price index, IRI sample	*1.000*	*1.001*	*1.017*	*1.000*	*1.001*	*1.017*
ex. striking markets						

Notes: The table reports the mean of the time-series statistics shown in figures 4 and 5, along with their standard deviation in parentheses, over three distinct periods. The label "before" corresponds to the 26-week period immediately before the strike, the label "during" to the 4-week period of the strike, and the label "after" to the 75-week period immediately after the strike. The price and quantity indexes are scaled such that their geometric mean equals 1 in the "before" period; all other statistics are in percent. The price index in addendum applies to the full IRI sample excluding the Los Angeles, San Diego, and St. Louis markets.

Table 3: Mean statistics before, during, and after 2005 Hurricane Katrina in New Orleans, Louisiana

Statistic	All			Food			Housekeeping and Personal Care		
	Before	During	After	Before	During	After	Before	During	After
Quantity index	1.001	1.220	1.135	1.001	1.224	1.136	1.001	1.188	1.131
	(0.008)	(0.019)	(0.009)	(0.008)	(0.021)	(0.010)	(0.010)	(0.049)	(0.007)
Price index	1.000	1.014	1.030	1.000	1.015	1.031	1.000	1.004	1.025
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Frequency of posted price changes	30.6	28.9	30.0	30.9	29.4	30.3	28.7	25.6	28.4
	(0.3)	(0.5)	(0.3)	(0.4)	(0.6)	(0.3)	(0.6)	(0.8)	(0.5)
Mean saving on the dollar	5.2	3.9	5.0	4.9	4.0	4.8	7.2	3.2	6.6
	(0.1)	(0.3)	(0.1)	(0.1)	(0.4)	(0.1)	(0.4)	(0.5)	(0.2)
Share of items on sale	14.1	13.4	14.1	14.1	13.6	13.9	13.7	11.3	15.5
	(0.3)	(0.6)	(0.1)	(0.3)	(0.6)	(0.2)	(0.4)	(0.8)	(0.3)
Share of revenues from items on sale	18.1	15.7	17.4	17.6	15.8	16.6	21.9	15.2	22.9
	(0.4)	(1.0)	(0.2)	(0.4)	(1.1)	(0.2)	(0.8)	(1.3)	(0.5)
Addendum									
Price index, IRI sample ex. Katrina	*1.000*	*1.007*	*1.017*	*1.000*	*1.007*	*1.016*	*1.000*	*1.007*	*1.026*
Price index, matched BLS sample	*1.000*	*1.008*	*1.013*	*1.000*	*1.007*	*1.008*	*1.000*	*1.010*	*1.033*

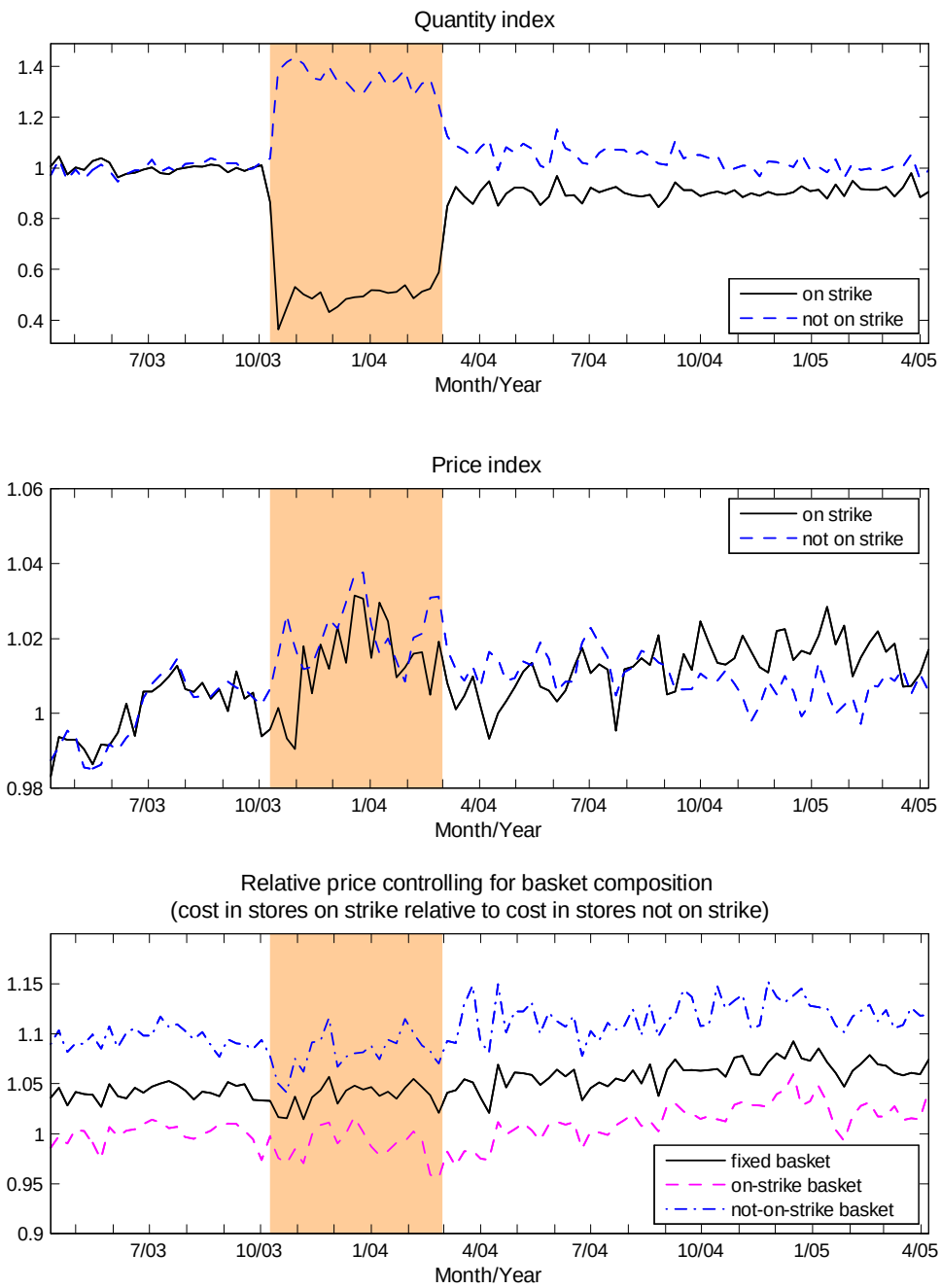
Notes: The table reports the mean of each statistic shown in figure 6, along with their standard deviation in parentheses, over three distinct periods. The label "before" corresponds to the 26-week period immediately before Hurricane Katrina struck New Orleans, the label "during" to the 10-week federal emergency period declared by FEMA, and the label "after" to the 69-week period immediately after the lifting of the federal emergency. The price and quantity indexes are scaled such that their geometric mean equals 1 in the "before" period; all other statistics are in percent. The price index in addendum applies to the full IRI sample excluding the New Orleans and Mississippi markets.

Table 4: Effect of major snowstorms and hurricanes on retailing activities

	Quantity index (in logs*100)	Price index (in logs*100)	Frequency of price changes (percent)	Frequency of regular price changes (percent)	Mean saving on the dollar (percent)	Share of items on sale (percent)	Share of revenues from items on sale (percent)	Share of prices equal to chain price (percent)
Major snowstorms								
Quantity peak analysis								
Pre-storm average	0.00	0.00	31.96	10.77	8.91	16.97	26.86	70.02
Peak week value	12.88	0.12	31.53	10.30	8.46	16.85	25.93	70.15
t-test of zero difference	14.14	1.51	-1.36	-2.70	-3.46	-0.68	-3.11	0.37
Regression results								
Estimated coefficients								
Week before storm	0.74	0.09	-0.64	-0.20	-0.17	-0.29	-0.47	0.29
	(1.47)	(1.85)	(-2.81)	(-1.52)	(-1.51)	(-1.86)	(-1.92)	(1.09)
Week during storm	11.70	0.00	-0.33	-0.49	-0.32	-0.03	-0.73	0.22
	(23.36)	(-0.02)	(-1.47)	(-3.69)	(-2.88)	(-0.17)	(-3.04)	(0.84)
Week after storm	-1.85	0.07	-0.61	-0.44	-0.26	-0.36	-0.91	0.38
	(-3.69)	(1.28)	(-2.69)	(-3.27)	(-2.34)	(-2.29)	(-3.78)	(1.42)
p-value, joint significance	0.00	0.20	0.00	0.00	0.00	0.05	0.00	0.33
Number of snowstorms	59	59	59	59	59	59	59	50
Observations	1,456	1,456	1,456	1,456	1,456	1,456	1,456	1,213
Hurricanes								
Quantity peak analysis								
Pre-storm average	0.00	0.00	30.19	11.05	7.22	15.73	24.08	72.91
Peak week value	12.84	0.06	30.64	11.60	7.22	16.10	24.54	71.84
t-test of zero difference	9.14	0.45	0.97	1.22	0.01	1.35	1.18	-2.33
Regression results								
Estimated coefficients								
Week before hurricane	6.44	-0.18	-0.20	0.41	0.09	-0.10	-0.35	-0.12
	(7.36)	(-2.31)	(-0.55)	(1.68)	(0.49)	(-0.44)	(-0.91)	(-0.29)
Week during hurricane	6.03	0.13	0.71	0.82	-0.34	-0.34	-0.64	-1.02
	(6.90)	(1.67)	(1.96)	(3.33)	(-2.59)	(-1.50)	(-1.66)	(-2.60)
Week after hurricane	-0.97	0.09	0.99	1.05	-0.39	-0.39	-1.38	-0.17
	(-1.11)	(1.17)	(2.72)	(4.29)	(-2.24)	(-1.71)	(-3.61)	(-0.43)
p-value, joint significance	0.00	0.01	0.01	0.00	0.01	0.20	0.00	0.08
Number of hurricanes	21	21	21	21	21	21	21	14
Observations	476	476	476	476	476	476	476	300

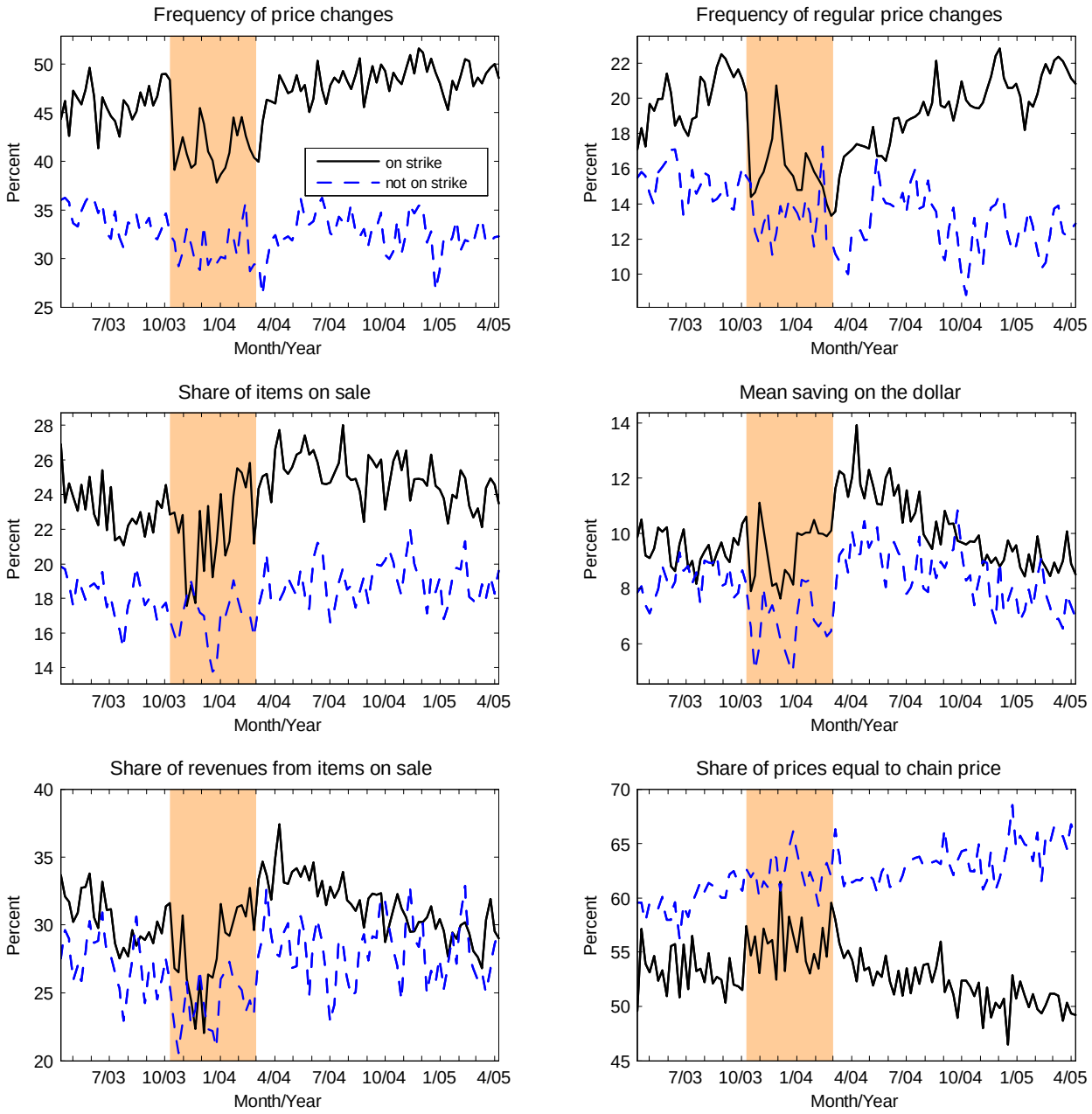
Notes: For a storm beginning in week t, we define the "peak week" as the week corresponding to the observed peak in quantities over the period $t-1$, t, and $t+1$. Our "response at peak analysis" reports the mean across storms of each statistic during the peak week and during a "pre-storm period" covering weeks $t-4$ to $t-2$. The mean of the logged quantity and price indexes are normalized to zero during the pre-storm period. The t-statistics in parentheses test the hypothesis that the difference between the value during the peak week and the average during the pre-storm period is zero across storms in the sample. The "regression results" are reported for a regression of each statistic on a quadratic time trend specific to each 6-month snowstorm or hurricane season and market and a set of dummies for the weeks around the storms. The p-values are for F-tests of the hypothesis that the coefficients on the week before, week during, and week after dummies are jointly different from zero. Not all markets have a sufficiently large number of stores per retail chain to allow for the computation of chain prices.

Figure 1: Impact of 2003-2004 Southern California supermarket strike on quantities and prices



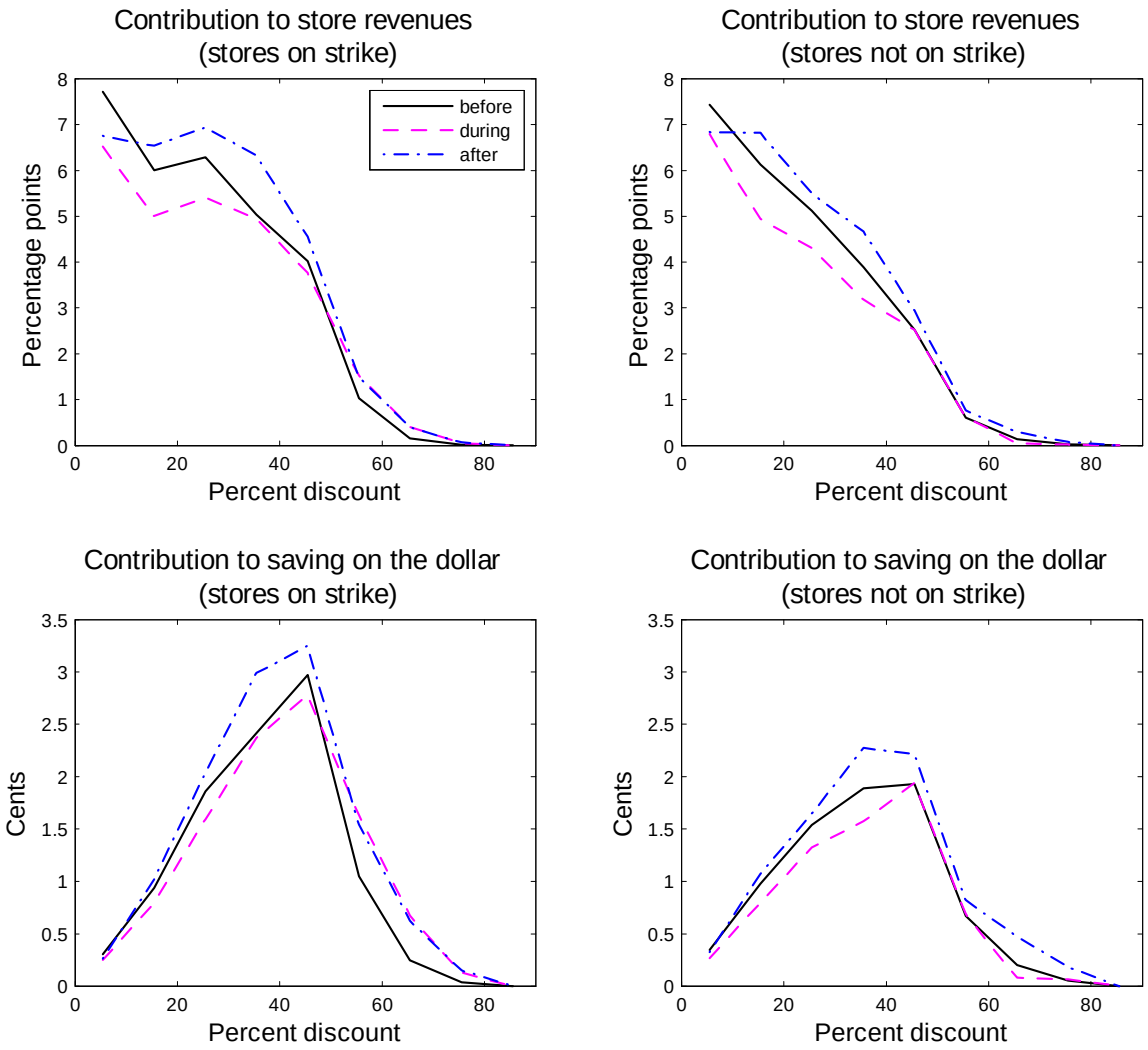
Notes: The shaded areas indicate the strike period. The series combine data from the San Diego and Los Angeles markets and, with the exception of the price ratios, are seasonally adjusted. The labels "on strike" ("not on strike") corresponds to stores that experienced a drop (increase) in revenues of 10 percent or more. The price and quantity indexes are scaled so that their geometric mean equals 1 in the 26-week period immediately before the strike. Our online appendix provides the methodological details.

Figure 2: Some key pricing features of the 2003-2004 Southern California supermarket strike



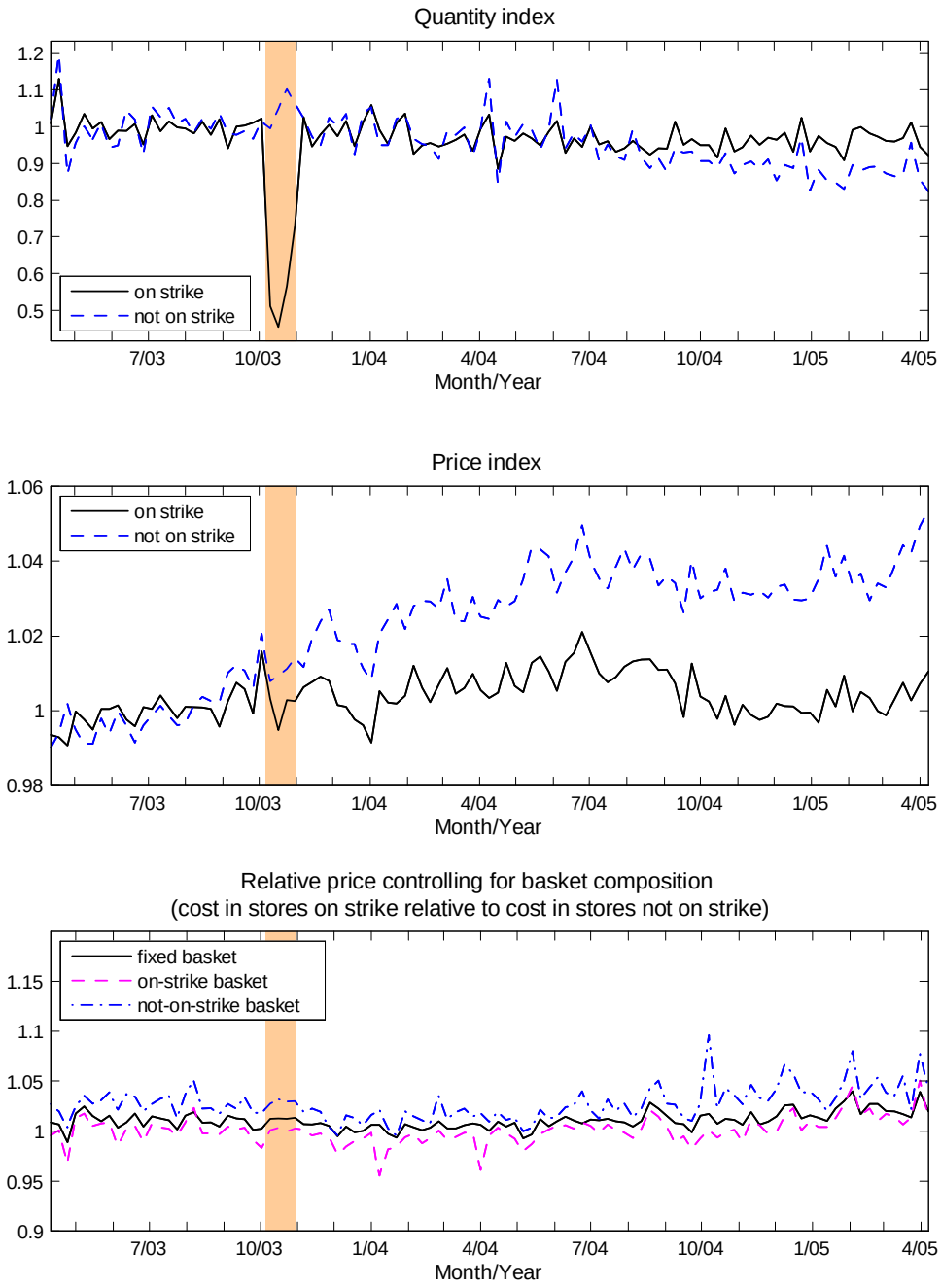
Notes: The shaded areas indicate the strike period. The series combine data from the San Diego and Los Angeles markets and, with the exception of the price ratios, are seasonally adjusted. The labels "on strike" ("not on strike") corresponds to stores that experienced a drop (increase) in revenues of 10 percent or more. The price and quantity indexes are scaled so that their geometric mean equals 1 in the 26-week period immediately before the strike. Our online appendix provides the methodological details.

Figure 3: Importance of discounts before, during, and after the 2003-2004 Southern California supermarket strike



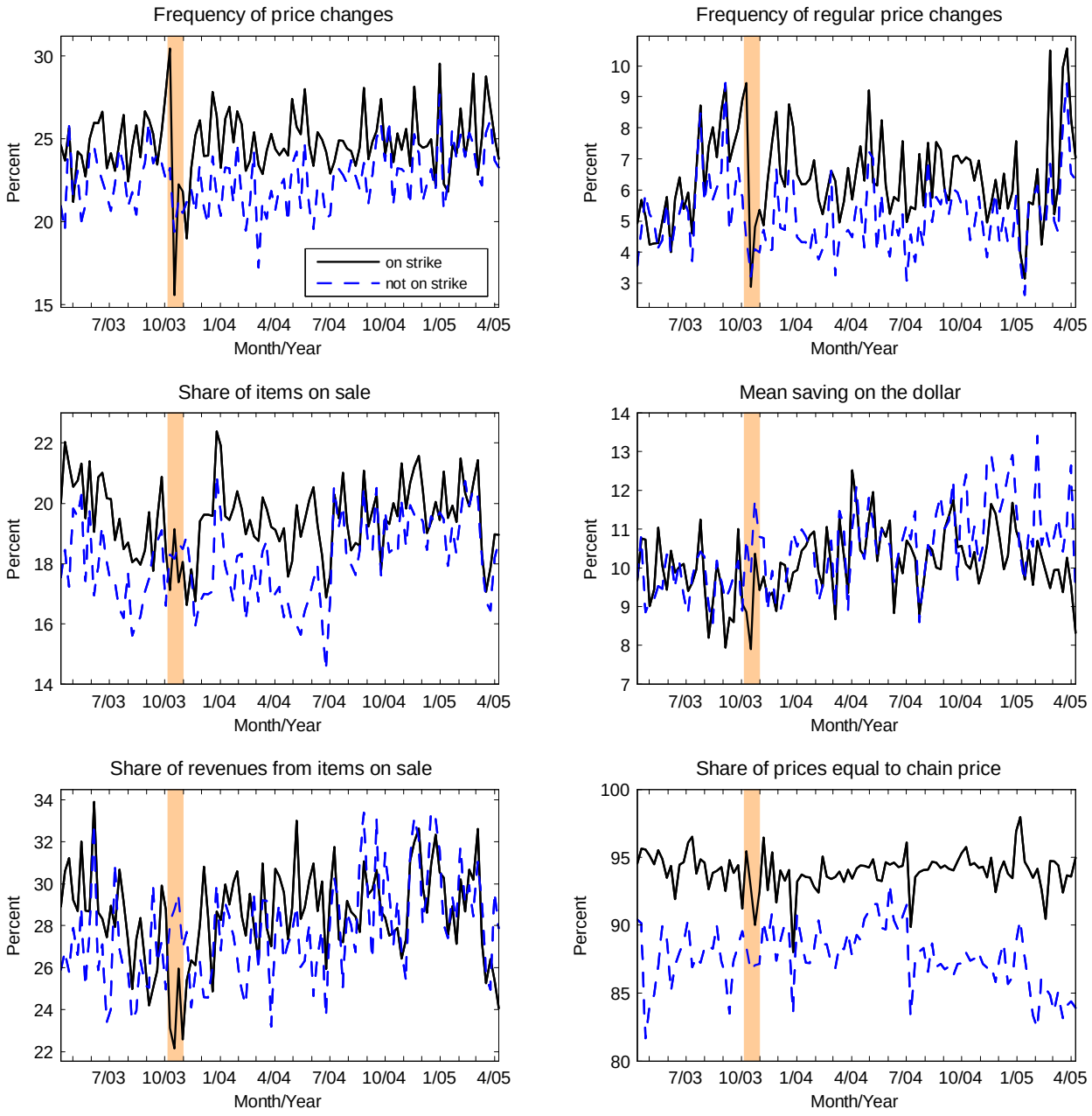
Notes: The promotional discounts are aggregated in 10-percentage-point bins, starting with the interval (0,10] percent, then (10,20] percent, and so on. Observations in the top panels are weighted by each item's contribution to total revenues, so that the area under the curve integrates to the share of total revenues derived from items on sale. Observations in the lower panels are weighted by each item's total dollar savings over the period (that is, $\omega_{i,t} = (p_{i,t}^{reg} - p_{i,t})q_{i,t} / \sum_j p_{i,t}^{reg} q_{j,t}$), so that the area under the curve integrates to the average discount offered by retailers. All statistics combine data from the San Diego and Los Angeles markets and not seasonally adjusted. The series labeled "on strike" ("not on strike") corresponds to stores that experienced a drop (increase) in revenues of 10 percent or more. The label "before" corresponds to the 26-week period immediately before the strike, the label "during" to the 21-week period of the strike, and the label "after" to the 58-week period immediately after the strike.

Figure 4: Impact of 2003 St. Louis supermarket strike on pricing decisions



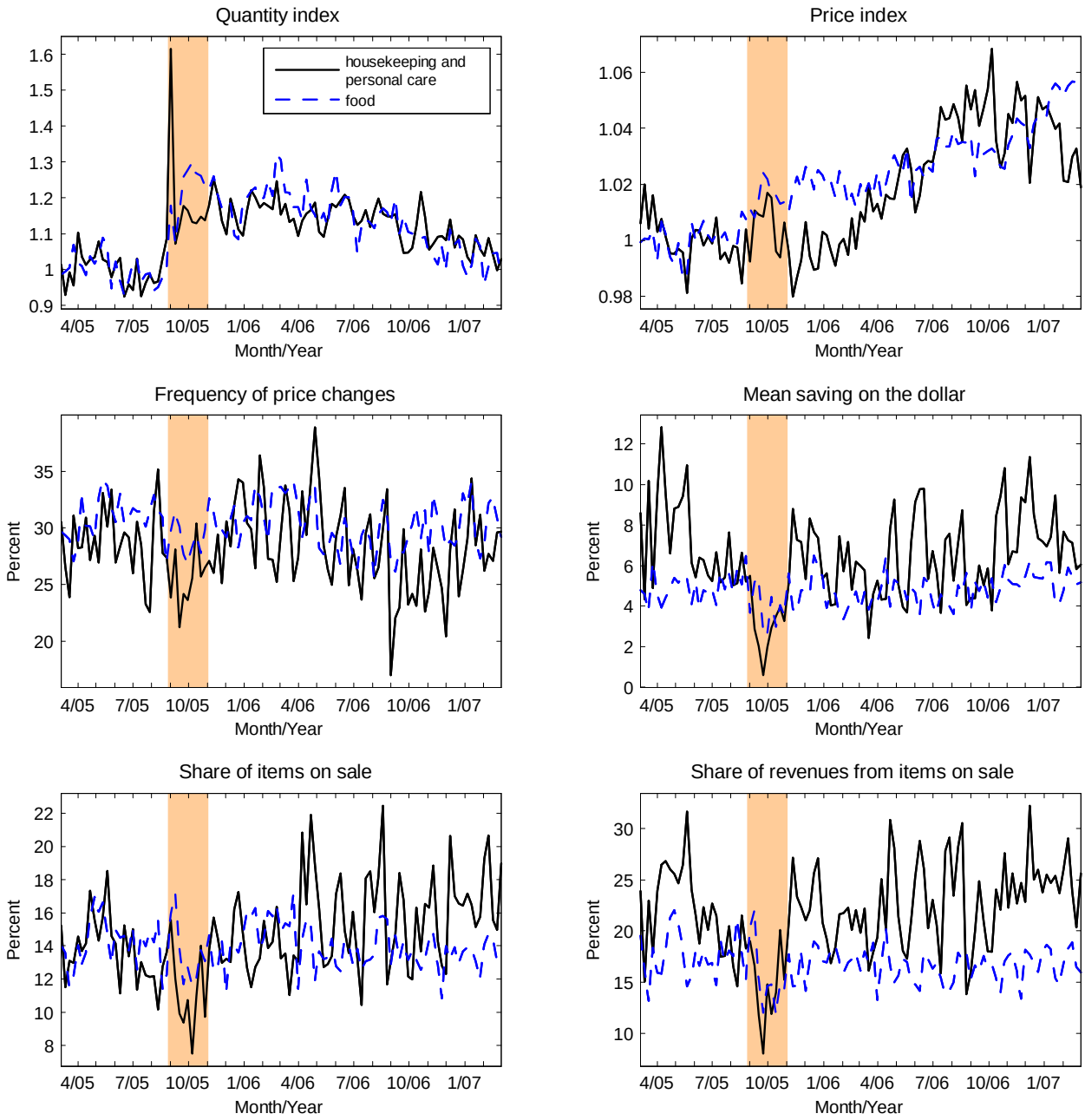
Notes: All series are seasonally adjusted. The shaded areas indicate the strike period. The price and quantity indexes are scaled so that their geometric mean equals 1 in the 26-week immediately period before the strike. The series labeled "on strike" and "not on strike" correspond to stores that experienced a drop in revenues of 10 percent or more and an increase in revenues of 10 percent or more, respectively. Our online appendix provides the methodological details.

Figure 5: Impact of 2003 St. Louis supermarket strike on pricing decisions



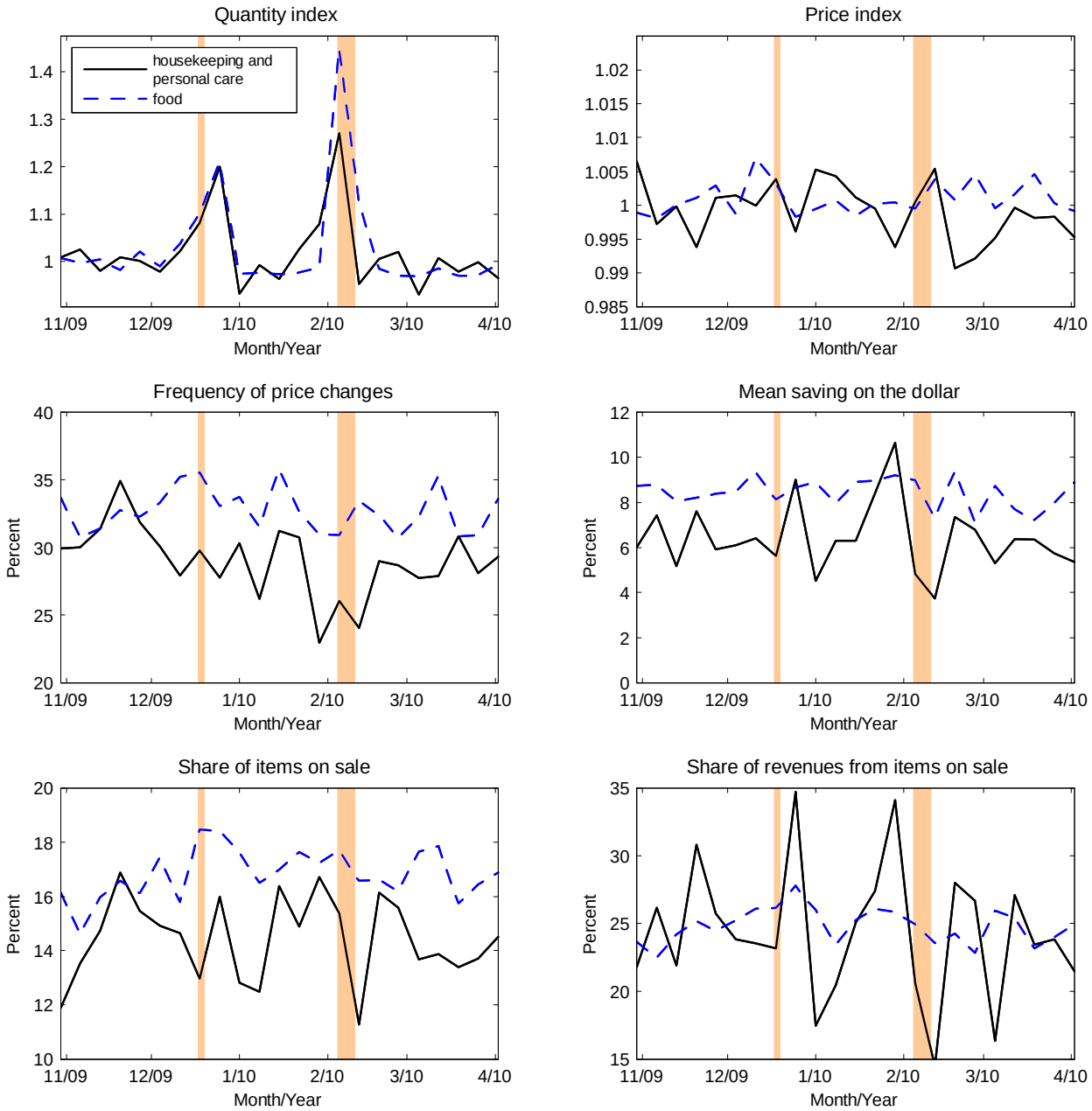
Notes: All series are seasonally adjusted. The shaded areas indicate the strike period. The price and quantity indexes are scaled so that their geometric mean equals 1 in the 26-week period immediately before the strike. Our online appendix provides the methodological details.

Figure 6: Impact of 2005 Hurricane Katrina on pricing decisions in New Orleans, Louisiana



Notes: All series are seasonally adjusted. The price and quantity indexes are scaled so that their geometric mean equals 1 in the 26-week period immediately prior to the strike. The shaded areas indicate the period (August 29, 2005, to November 1, 2005) covered by FEMA's major disaster declaration. Our online appendix provides the methodological details.

Figure 7: Illustration of snowstorms effects: winter 2009–2010 in Washington, D.C.



Notes: The leftmost shaded areas mark the December 18–19, 2009, nor'easter while the rightmost shaded areas mark a pair of blizzards that hit the U.S. capital on February 5–10, 2010. The price and quantity indexes are scaled so that their geometric mean equals 1 in November 2009. All series are seasonally adjusted. Our online appendix provides the methodological details.