House Prices, Local Demand, and Retail Prices*

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Abstract

We use detailed micro data to document a causal response of local retail price to changes in house prices, with elasticities of 15%-20% across housing booms and busts. Notably, these price responses are largest in zip codes with many homeowners, and non-existent in zip codes with mostly renters. We provide evidence that these retail price responses are driven by changes in markups rather than by changes in local costs. We then argue that markups rise with house prices, particularly in high homeownership locations, because greater housing wealth reduces homeowners' demand elasticity, and firms raise markups in response. Consistent with this explanation, shopping data confirms that house price changes have opposite effects on the price sensitivity of homeowners and renters. Our evidence has implications for monetary, labor, and urban economics, and suggests a new source of markup variation in business cycle models.

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How do prices and markups respond to demand shocks? This question is of central importance for business cycle modeling, and a large empirical literature has tried to provide answers using aggregate time-series data. However, this approach requires strong assumptions, both to identify aggregate demand shocks and to measure markups; consequently, the literature has arrived at conflicting conclusions regarding the cyclicality of markups (see Nekarda and Ramey, 2013, for a review). Analyzing only time-series data also makes it hard to isolate the channel that explains any observed relationship.

In this paper, we instead turn to micro data to provide direct causal evidence on the response of retail price-setting and household shopping behavior to changes in wealth and demand, and in doing so propose a new channel for business-cycle variation of markups. In a series of papers, Mian and Sufi (2011, 2014*a*) and Mian, Rao and Sufi (2013) document that exogenous local house price movements have strong effects on local demand. In this paper, we link retailer scanner price data and household purchase data to zip-code-level house prices to identify the response of price-setting and shopping behavior to these house-price-induced local demand shocks.[1]

We argue for a causal relationship using two alternative and complementary identification strategies. In our first set of results, we follow the identification strategy in Mian and Sufi (2011) and use measures of the local housing supply elasticity constructed by Saiz (2010) and Gyourko, Saiz and Summers (2008) as instruments for house price movements. Across a variety of empirical specifications, we estimate an elasticity of local retail prices to house price movements of 15%-20%. This elasticity is highly significant, and its magnitude implies that house-price-induced demand shocks account for roughly two-thirds of inflation differences across regions in our sample.

Our second identification strategy exploits variation in homeownership rates across zip codes. The same change in house prices will induce different real wealth and demand effects for homeowners and renters, since they differ in their net asset position in housing.[2] Consistent with these differential demand effects, we show that there is a strong interaction between homeownership rates and the relationship between house prices and retail prices. In zip codes with a high homeownership rate, house price increases lead to the largest increases in retail prices, while in zip codes with the lowest homeownership rates, house price increases sometimes even lead to declines in retail prices.

Taken together, we believe that our two identification strategies provide compelling evidence for a causal effect of house-price-induced demand shocks on local retail prices, since it is difficult to jointly explain both results via confounding explanations such as local supply shocks. Our first empirical results instrument for changes in house prices using measures of the housing supply elasticity, and

[1] These demand effects might arise from interactions with collateral and credit, or through more direct wealth effects. For our purposes, we only require that demand changes with house prices, and can remain agnostic about the channel.

[2] House price increases imply higher wealth and looser borrowing constraints for homeowners. In contrast, no such effects should be present for renters. Any changes in the local cost of living through higher rents (either explicit rents, or implicit rents when living in owner-occupied housing) affect both renters and homeowners the same way. Therefore, increasing house prices increase the wealth and credit access of homeowners relative to renters.

it is unclear why possible confounding supply-side shocks would be particularly strong in regions with lower housing supply elasticity. More importantly, any shock that might violate the exclusion restriction in our instrumental variables strategy would need to also vary with local homeownership rates, which dramatically narrows the list of potential concerns.

To provide further evidence for our causal interpretation of the observed relationship, we show that the relationship between house prices and retail prices survives an extensive set of robustness checks. In particular, we document that our results are not driven by changes in store or product quality, changes in income or gentrification, differences in the employment mix across locations, or store entry and exit. We also show that our results hold with coast and region fixed effects, so that our instrumental variables results are not driven by a spurious correlation between supply elasticity and region-specific shocks. Finally, our results hold when dropping the "sand-states" that saw the largest housing bubbles as well as other outliers, and so are not driven by unusual observations.

After arguing for a causal relationship between house prices and retail prices, we next consider *why* increases in house prices lead to higher retail prices. By definition, an increase in retail prices must be driven by either an increase in markups or by an increase in marginal costs. While we believe that identifying either channel would be interesting, we provide several pieces of evidence that support markup variation as the primary explanation for our empirical patterns.

First, our retail price data include only tradable goods in grocery and drug stores. These goods are not produced locally, so their wholesale cost should be independent of any local shocks. Since these wholesale costs represent nearly three-quarters of total costs and an even larger fraction of marginal costs in our stores, it is unlikely that geographic variation in marginal costs drives our retail price patterns. To provide additional support for this argument, we supplement our primary analysis using data from a large national retailer, which include measures of both marginal costs and markups. We use these high-quality internal profitability measures to directly show that this retail chain raises markups in locations with increasing house prices. Again, this house price effect on markups is strongest where homeownership rates are high.

While wholesale costs are the primary component of our retailers' marginal cost and do not drive our retail price patterns, we next directly consider two additional cost channels that might affect retail prices: local labor costs might rise in response to increased local demand, or local retail rents may rise.

Since labor costs are a small fraction of overall marginal cost for the stores in our data, explaining retail price movements through this channel would require extremely large responses of local labor costs to local demand. Consistent with this channel being unimportant, we find that controlling for local wages and a variety of other labor market conditions does not change our estimates.

Next, we provide evidence that our retail price results do not reflect pass-through of local retail rents or land prices. First, and most importantly, pass-through of local land prices cannot explain the fact that retail prices rise much more quickly with house prices in locations with high homeownership rates. If the relationship between retail prices and house prices was driven by direct cost pass-through of local land prices or rents, then the local homeownership rate should instead be irrelevant. Second, we match our data with information on local retail rents and find that they have no effect on our estimates. Finally, we exclude high-rent locations from our analysis (since these locations should have the highest fraction of rent in total costs), and obtain near-identical estimates.

Together, wholesale inventory costs, labor costs, and rent overhead represent essentially one-hundred percent of marginal costs for our retailers. Thus, if the variation in retail prices is not driven by variation in costs, it must be driven by variation in markups.

Why would firms raise markups in response to positive housing wealth shocks? In the final empirical section of our paper, we argue that positive wealth effects lead households to become less price-sensitive. In standard price-setting models, optimal markups will then rise as the elasticity of demand falls. We use data on individual household shopping behavior from Nielsen Homescan to show that when house prices rise, homeowners increase their nominal spending but purchase fewer goods with a coupon, and reduce the fraction of spending on generics and on items that are on sale. In contrast, renters reduce nominal consumption and appear to become more price sensitive, purchasing more goods on sale, more generics, and more items with a coupon. This is consistent with a model in which the value of leisure rises with wealth, so that wealthier households allocate less time to shopping for cheaper prices and thus become less price-sensitive (see Alessandria, 2009; Kaplan and Menzio, 2013; Huo and Ríos-Rull, 2014). Since house price changes have opposing wealth effects on homeowners and renters, this naturally explains the difference in shopping responses and again rationalizes our earlier retail price interactions with homeownership rates.[3]

Taken together, our empirical results provide evidence of an important link between changes in household wealth, shopping behavior and firm price-setting. Positive shocks to wealth cause households to become less price-sensitive and firms respond by raising markups and prices.

Implications: Our results have direct implications for understanding the demand effects of the housing boom and bust, which was central to the Great Recession. In an influential paper, Sinai and Souleles (2005) argue that house price changes should not lead to changes in homeowners' behavior, since higher house prices increase asset values but also increase homeowners' implicit rent. We join a recent literature that rejects this theoretical benchmark (e.g., Campbell and Cocco, 2007; Case, Quigley and Shiller, 2011; Carroll, Otsuka and Slacalek, 2011; Mian and Sufi, 2014*a*), but we also extend it in one important direction: we are able to decompose observed spending changes into nominal and real components, and our empirical evidence implies that some of the variation in local spending is capturing price variation rather than variation in real spending. Our results are therefore directly rel-

[3]Since roughly two-thirds of households are homeowners, average price sensitivity falls with house prices.

evant for learning about aggregate responses to housing wealth shocks from cross-sectional evidence. Indeed, Mian and Sufi (2014*a*) show that general equilibrium price effects resulting from the interaction between aggregate demand and aggregate supply are a key input to calculating the aggregate real effects of house price changes. Without price data, they explore various scenarios but must make strong assumptions about counterfactuals in order to draw any concrete conclusions.

In addition, we think that our results provide insights for understanding business cycles more generally. The house-price-induced demand shocks we identify share many features with aggregate business cycles. In particular, they are large, unanticipated, and, most important for understanding prices and markups, they generate similar changes in household shopping behavior. For example, Aguiar, Hurst and Karabarbounis (2013) show that there is an increase in time spent on shopping during recessions, and Nevo and Wong (2014) show that other measures of shopping intensity rose during the Great Recession (see also Krueger and Mueller, 2010). This similarity to business cycles contrasts our approach with existing micro studies of demand shocks such as Warner and Barsky (1995), Chevalier, Kashyap and Rossi (2003), Gicheva, Hastings and Villas-Boas (2010), and Gagnon and Lopez-Salido (2014), which study responses to predictable seasonal holidays, changes in gasoline prices, store strikes, and temporary weather events. While these demand shocks are interesting in their own right, it is less clear that they are informative for understanding business cycles.

To our knowledge, Coibion, Gorodnichenko and Hong (2014) are the first researchers to analyze geographic variation in price-setting to inform aggregate business cycles. They use the same scanner data as we do to find that prices do not respond to local unemployment rates. Beraja, Hurst and Ospina (2015) use a broader set of scanner data that is only available beginning in 2006, and draw the opposite conclusion. Our focus on exogenous changes in house prices allows us to isolate demand shocks, while local unemployment rates reflect a combination of local supply and demand factors, which complicates their interpretation.[4] We also jointly analyze household shopping behavior and firm price setting to argue that the relationship between house prices and retail prices reflects markup variation driven by changes in households' price sensitivity.

This type of markup variation has significant implications for business cycle modeling. In New Keynesian models, changes in markups have important effects on real economic activity. Increases in demand drive up nominal marginal costs, and sticky prices mean that average markups fall. This decline in markups then leads to a real increase in economic activity. In the simplest versions of these models, "flexible price" desired markups are constant so that if pricing frictions are removed, then actual markups are also constant. Our results suggest that even with no pricing frictions, markups

[4] The conflicting findings of Coibion, Gorodnichenko and Hong (2014) and Beraja, Hurst and Ospina (2015) could reflect the presence of time-varying confounding shocks, since supply and demand shocks have opposite implications for the correlation between retail prices and unemployment. In addition, even large increases in unemployment affect only a small part of the population directly, which reduces the econometric power for identifying demand shocks. In contrast, house price changes impact many more households, and therefore have the potential to induce a more significant demand shock.

can change for a second and complementary reason: countercyclical household shopping intensity pushes adjusting firms to choose relatively higher markups in booms. It is important to note that this need not imply procyclical total markups, but it does suggest that modeling the endogenous interaction between household shopping intensity and firm pricing behavior might improve our understanding of the monetary transmission mechanism. Indeed, medium-scale DSGE models such as Smets and Wouters (2007), Christiano, Motto and Rostagno (2010), and Justiniano, Primiceri and Tambalotti (2011) introduce markup ("cost-push") shocks to firms' desired markups in order to better match aggregate time-series data. However, in these DSGE models, movements in desired markups are treated as exogenous "structural" shocks, which are policy invariant. In contrast, our evidence suggests that these desired markups will respond endogenously to changes in monetary policy.

Our finding that markups vary for reasons besides sticky prices also complicates the interpretation of the large literature using aggregate time-series data to measure the cyclicality of markups.[5] These papers identify movements in the overall markup and often interpret their results as evidence in favor of or against New Keynesian models. However, if "flexible price" desired markups are procyclical, while sticky-price-induced markups are countercyclical, then the total markup measured in the data will depend on the relative strength of these two forces. If that relative strength varies across time (see Vavra, 2014), then this can potentially reconcile the conflicting conclusions about the importance of price stickiness in explaining markup variation in the literature.

Our conclusion that there is a strong equilibrium interaction between household shopping behavior and firm price-setting also relates to recent work by Huo and Ríos-Rull (2013) and Kaplan and Menzio (2013), who show that in the presence of product market frictions, cyclical changes in shopping behavior can feed back into firms' decisions to give rise to recessions that look demand-driven. We believe that we are the first paper to document an interaction between household and firm behavior at business cycle frequencies. Our focus on time-series variation in household and firm behavior also distinguishes our results from previous static work on similar subjects. For example, Handbury (2012) estimates non-homothetic price indices that vary with household wealth in the cross-section, and Manova and Zhang (2012) show that exporters set higher prices in wealthier product markets. However, it is possible that the forces which drive these long-run, static relationships between wealth and prices may have been irrelevant for variation in wealth at business cycle frequencies.[6]

The rest of the paper proceeds as follows: Section 1 describes our data. Section 2 describes the price-setting and shopping behavior results. Section 3 further discusses the implications of our findings, including implications for labor and urban economics not discussed above. Section 4 concludes.

[5] See Domowitz, Hubbard and Petersen (1986), Bils (1987), Haskel, Martin and Small (1995), Galeotti and Schiantarelli (1998), Rotemberg and Woodford (1999), Gali, Gertler and Lopez-Salido (2007), and Nekarda and Ramey (2013).

[6] For example, permanent differences in tastes could explain the static relationships, but would not generate the changes across time in individual household behavior which we document.

1 Data Description

To conduct the empirical analysis we combine a number of data sets. We begin by describing the construction of our key dependent variables: the local retail price indices and our measures of household shopping behavior. We then detail the sources for our other data.

1.1 Retail Price Data - IRI Data

Our primary retail price data are provided by IRI Worldwide, and have weekly store-level information for chain grocery and drug stores from 2001 to 2011.[7] The data set includes store-week-UPC sales and quantity data for products in 31 categories, which represent roughly 15% of household spending in the Consumer Expenditure Survey.[8] We also obtained the zip code location of each store in the data from IRI Worldwide. These zip code identifiers are not part of the standard academic data release, and we believe we are the first to exploit them.[9] In total, these data cover approximately 7,200 stores in over 2,400 zip codes. There are a large number of retailers in each metropolitan area. For example, the Chicago market contains observations from 131 unique retailers. Appendix Figure A1 shows the geographic distribution of the stores in this sample.

While the raw data are sampled weekly, we construct quarterly price indices, since this makes the time-unit comparable to that of various local controls, and reduces high-frequency noise. Let t index the quarter of observation, l a geographic location (MSA or zip code), c a product category, and i an individual UPC-store pair (henceforth item).[10] We construct the price of an item by dividing its total dollar value of sales (TS) by the total quantity of units sold (TQ). That is,

$$P_{i,l,c,t} = \frac{TS_{i,l,c,t}}{TQ_{i,l,c,t}}.$$

Here, total sales are inclusive of retailer discounts and promotions, but exclude manufacturer coupons. In our benchmark specification, we include all observed prices when constructing our price indices, since we are interested in how the broadest price aggregate responds to local demand. We later show the robustness of our results to using price indices constructed when excluding "sales" prices.[11]

Given these individual price observations, we next describe the construction of our location-

[7] These data are proprietary but are available for academic research purposes. For a description of the data acquisition process, see http://www.iriworldwide.com/Insights/Academics.aspx.

[8] These product categories cover mostly processed food and beverages, cleaning and personal hygiene products, so they are most similar to the BLS "food at home" index.

[9] The standard academic data release only includes geographic indicators for 47 broad geographic markets, often covering a major metropolitan area (e.g., Chicago), but sometimes covering regions with numerous MSAs (e.g., New England). See Bronnenberg, Kruger and Mela (2008) for additional description of the data.

[10] We track the price of identical items (UPC-store pairs) across time, so that changes in quality or issues with comparing non-identical products are not relevant for our results (quality changes across time will typically be associated with new UPCs). In particular, our price index is not affected by changes in the composition of goods or stores over time.

[11] We have identified sales using both the promotional price flag in the IRI data, as well as "v-shaped" price patterns.

specific price indices. This construction necessarily entails various measurement choices and challenges. In the main body of the paper we concentrate on describing our benchmark price index, but in Appendix C we provide more details on our price index construction, and show that our empirical results continue to hold for price indices constructed under various alternative assumptions.

Since we are interested in constructing price indices across time, we only include an item if it has positive sales in consecutive quarters. After constructing item-level prices, we create location-specific price indices using a procedure that largely mimics the construction of the CPI by the BLS.[12] In particular, we construct a geometric-weight price index with a consumption basket that is chained annually.[13] Let $\omega_{i,l,c,y(t)} = \frac{TS_{i,l,c,y(t)}}{\sum_{i \in c} TS_{i,l,c,y(t)}}$ be an item's share in a category's annual revenue, where $y(t)$ indexes the year in which quarter t is observed. In our benchmark results, we construct these revenue weights separately for each location to allow for spatial variation in item importance. That is, ω is indexed by l. In Appendix C, we also redo our analysis using national revenue weights, so that ω is no longer indexed by l, and using constant geographic weights, so that ω is no longer indexed by t. Under these alternative constructions, location-specific changes in household purchases, in product composition, or changes in product quality do not affect location-specific price indices. Our findings are robust to these alternative weights, which implies that the retail price responses we document require actual changes in price posting behavior, and cannot be explained by shifting weights.

We construct our price index in two steps. We first construct a category-level price index:

$$\frac{P_{l,c,t+1}}{P_{l,c,t}} = \prod_i \left(\frac{P_{i,l,c,t+1}}{P_{i,l,c,t}} \right)^{\omega_{i,l,c,y(t)}}.$$

We then construct an overall location-specific price index by weighting these category price indices by the revenue share of a particular category, $\omega_{l,c,y(t)} = \frac{\sum_{i \in c} TS_{i,l,c,y(t)}}{\sum_i TS_{i,l,y(t)}}$:

$$\frac{P_{l,t+1}}{P_{l,t}} = \prod_c \left(\frac{P_{l,c,t+1}}{P_{l,c,t}} \right)^{\omega_{l,c,y(t)}}.$$

Panel A of Figure I shows that a nationally-aggregated version of our price index qualitatively reproduces the behavior of the BLS food-at-home CPI.[14] While they do not match precisely, this is

[12] Since we focus on understanding aggregate inflation, we abstract from local variety effects explored in Handbury (2012).

[13] We chain our results annually rather than at higher frequencies to avoid "chain-drift" that can occur with frequent updating. See Ivancic, Erwin Diewert and Fox (2011) for a discussion. The CPI construction is similar but is a Laspeyres Index using a basket of goods that is only updated every five years.

[14] We normalize our indices to 1 in period $t = 0$, so computation of the location-specific price index only requires knowledge of how individual items in a location change prices across time, and not how the same product is priced across locations at a given point in time. We do this because we are interested in price responses to demand shocks at business cycle frequencies, not in permanent price level differences across locations. Since our empirical specification is in changes, we also avoid the measurement complications and biases discussed in Handbury and Weinstein (2014). It is nevertheless worth noting that methodological differences mean that their conclusion that retail price levels do not vary across locations after correcting for these biases is perfectly consistent with our time-series result. In particular, their paper uses household-level data and removes demographic effects such as income as well as retailer fixed effects. They do not add house price controls,

not surprising, since the categories and products sampled are not identical. The BLS also produces food-at-home CPIs for 27 metro areas, of which 19 overlap with locations in the IRI data set. Panel B of Figure I compares changes in our MSA-level price indices to changes in these metro area price indices. Again, there is a strong correlation between changes in our MSA price indices and those published by the BLS. The relationship is not perfect, but this is even less surprising for these disaggregated indices.[15] This figure also shows that there is substantial variation across MSAs in retail price movements, reflecting substantial local pricing within chains: the mean of the within-chain, within-UPC standard deviation of log prices is 4.7%.[16]

Finally, Panel C of Figure I shows that the cross-sectional variation in the food-at-home CPI produced by the BLS is very similar to the cross sectional variation in the broader CPI including all products. This suggests that the retail price responses to house prices that we document are likely to generalize to a broader set of goods than that covered by our IRI data.

1.2 Retail Price Data - Large Retailer Data

We use the IRI data as our primary measure of retail prices, since it covers many retail chains and has large geographic coverage. Unfortunately, IRI only collects data on prices and not on marginal costs. Since the second half of our paper focuses on decomposing price changes into markup and marginal cost variation, we supplement our primary IRI data with a data set on retail prices from a large U.S. retail chain, which does contain a reliable measure of marginal cost (see Eichenbaum, Jaimovich and Rebelo, 2011; Gopinath et al., 2011, for other papers using these data).[17]

This retailer reports UPC-store-level information for more than 125,000 unique UPCs from 250 stores in 39 MSAs, covering the period January 2004 to June 2007. Importantly, for each product, there is information on wholesale costs and adjusted gross profits, in addition to gross prices (i.e., list prices) and net price (i.e., list prices net of rebates, promotions, and coupons). We follow Gopinath et al. (2011) to construct measures of the marginal cost for each good as the difference between net prices and adjusted gross profits. This measure represents the retailer's cost net of discounts and inclusive of shipping costs; it is viewed by the retailer as measuring the replacement cost of an item,

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so any correlation between retail price and house price levels that is related to household demographics or retailer location is absorbed. That is, their price index is a conceptually distinct object, and the controls in their specification mostly absorb our variation of interest. It is also worth noting that we replicate their conclusion that there is a mild negative relationship between city size and retail prices. This is to be expected as both our results and theirs remove effects of UPC heterogeneity.

[15] For most regions, the increase in the CPI is modestly larger than the increase in the IRI index. This reflects standard substitution bias, since we use a chained index while the BLS uses a fixed basket. On average, sampling error is less of a concern for our data than for the BLS, since we have 50,000 price observations per MSA-quarter while their indices are constructed from roughly 2,000 price observations per metro-quarter. We also cover substantially more markets than the BLS.

[16] Section 1.2 confirms this within-chain variation using data from a single large retailer. Finally, many retailers operate fairly locally: the average chain in our data operates in only 4 MSAs.

[17] We do not use this as our primary data set, since it only includes information from a single chain. It also covers a shorter time horizon, and has much less geographic variation than the IRI data.

and is the cost measure used in their pricing decisions (see Eichenbaum, Jaimovich and Rebelo, 2011). Using these data, we construct a net price index and a marginal cost index for each location, using the approach described in Section 1.1.

1.3 Shopping Data

We use Homescan data from AC Nielsen to measure household-level shopping behavior.[18] The data set contains a weekly household-level panel for the period 2004-2011. The panel has large coverage, with 125,000 households in over 20,000 zip codes recording prices for 400 million unique transactions. The product coverage is somewhat broader than that in the IRI data, and essentially captures broad non-service retail spending. Roughly half of expenditures are in grocery stores, a third of expenditures are in discount/warehouse club stores, and the remaining expenditures are split among smaller categories such as pet stores, liquor stores, and electronics stores. While the data set includes store identifiers, these codes are anonymized so that researchers cannot recover the exact identity of a retailer, and geographic identifiers include only the first three-digits of a store's zip code.

Households report detailed information about their shopping trips using a barcode scanning device provided by Nielsen. After a shopping trip, households enter information including the date and store location. They then scan the UPC-barcode of all purchased items. The price of the item is collected in one of two ways: for trips to stores that partner with Nielsen, the average price of the UPC for that store-week is automatically recorded. For trips to stores that do not partner with Nielsen, households hand-enter the price paid from their receipt. In addition to the price, households also record whether a product was purchased while "on sale" or using a coupon.[19] In addition, since we know the UPC of each item, information is available on whether a product is generic or name-brand. We use this information to construct quarterly expenditure shares for goods purchased in each of these categories for each household.

While panelists are not paid, Nielsen provides incentives such as sweepstakes to elicit accurate reporting and reduce panel attrition. Projection weights are provided to make the sample representative of the overall U.S. population.[20] A broad set of demographic information is collected, including age, education, employment, marital status, and type of residence. Nielsen maintains a purchasing threshold that must be met over a 12-month period in order to eliminate households that report only a small fraction of their expenditures. The annual attrition rate of panelists is roughly 20%, and new households are regularly added to the sample to replace exiting households.

[18] These data are available for academic research through a partnership with the Kilts Center at the University of Chicago, Booth School of Business. See http://research.chicagobooth.edu/nielsen for more details on the data and the relationship.

[19] Starting in 2007, there is a documented sharp decline from roughly 30% to 24% in the fraction of products purchased on sale. This is due to a change in the scanner technology that was introduced to new households in 2007. Since this was a household-specific change and we include household fixed effects, this does not affect any of our conclusions.

[20] We use these projection weights in all reported results, but our results are similar when weighting households equally.

1.4 Other Data

In addition to the IRI data, "large retailer" data, and Nielsen data, we use a number of other data sets in our analysis. We obtain house price indices at both the zip code level and the MSA level from CoreLogic, which computes repeat sales price indices from individual transactions data.[21] We also use information on average effective retail rents from 2000-2014 for 45 MSAs. These rent data are compiled by the REIS corporation from telephone surveys of property managers and leasing agents, and include quarterly information on the average rent paid per square foot of retail space.

Homeownership rates by zip code come from the 5-year estimates of the 2011 American Community Survey (ACS).[22] Data on education levels, age, and population density also come from the respective waves of the ACS. We obtain wage data from the the Quarterly Census of Employment and Wages conducted by the BLS. Employment shares and information on the number of retail establishments come from the County Business Patterns produced by the U.S. Census, and we classify NAICS sectors into tradable and construction using the definitions in Mian and Sufi (2014*b*). As discussed in the next section, our measures of housing supply elasticity to instrument for house price changes come from Gyourko, Saiz and Summers (2008) and Saiz (2010).

2 Empirical Analysis

We next provide an overview of our empirical strategy for identifying the impact of house price changes on retail prices. We use two complementary identification strategies to show that our relationship is causal, and that house-price-induced demand shocks drive changes in retail prices.

Our first approach uses across-MSA variation in housing supply elasticity as an instrument for changes in house prices. This approach isolates differences in house price growth that are plausibly orthogonal to factors that might directly influence retail prices.

Our second approach exploits a unique feature of house price movements to provide additional evidence that they causally influence retail price. In particular, house price movements induce differential wealth effects for homeowners and renters due to these households' different net housing asset positions. With this in mind, we show that the relationship between house prices and retail prices depends strongly on local homeownership rates. There is no reason that confounding shocks should interact with the fraction of homeowners in a zip code, but such an interaction is exactly what would be expected if higher retail prices were driven by positive house-price-induced demand shocks.

The use of these two complementary identification strategies substantially reduces the set of con-

[21] Our empirical patterns persist when using Zillow house price indices, but these are only available for a smaller set of locations. Zillow computes median sales price indices rather than repeat sales price indices. While these are affected by changes in the composition of houses that are sold, the data requirements are lower. This might reduce noise in the estimation of repeat sales indices at geographically disaggregated levels such as zip codes.

[22] While there are some small changes in homeownership rates over the housing boom and bust, cross-sectional differences are highly persistent, so we focus on homeownership rates at a particular point in time.

founding explanations for our results, since geographic variation in homeownership rates is quite distinct from geographic variation in housing supply elasticity. Alternative stories must explain not just why housing supply elasticity would not satisfy the instrumental variables exclusion restriction, but also why such violations would then interact with local homeownership rates.

In addition to documenting a causal link from house prices to retail prices, we provide evidence on the economic mechanism driving this relationship. In general, an increase in retail prices must reflect an increase in marginal costs or an increase in markups. We argue that that our results primarily reflect markup movements by first showing that our patterns are not driven by changes in observable costs. We then present direct evidence that households become less price sensitive after their housing wealth rises; this increases firms' optimal markups. Just as suggested by our retail price results, we show that this change in household price sensitivity differs strongly by homeownership status.

2.1 Price-Setting Behavior - MSA Level

We first analyze the relationship between house prices and retail prices. We split the sample into the periods 2001-2006, when house prices in the U.S. were generally rising, and 2007-2011, when house prices were generally falling. This allows for an asymmetric impact of house price increases and decreases on retail prices. We begin by sorting MSAs into quintiles by their house price growth over the housing boom and housing bust. The top row of Figure II shows how retail prices evolve for MSAs in the top and bottom quintile of house price growth over each period. Clearly, retail price growth was significantly stronger in those MSAs that experienced higher house price growth.[23]

The middle row of Figure II shows the more disaggregated correlation between MSA-level house price growth and retail price growth over the periods 2001-2006 (Panel C) and 2007-2011 (Panel D). In both periods there is a strong positive correlation between house price growth and retail price growth. This positive bivariate correlation is confirmed by the OLS regressions of retail price changes on house price changes over these periods in column 1 of Table I. Appendix Table A1 provides summary statistics on the dependent variable and controls. The estimated coefficient suggests an elasticity of retail prices to house prices of about 6%-8%. In column 2 we also include controls for changes in economic conditions, such as changes in the unemployment rate, changes in wages, and changes in the employment shares in the grocery retail, construction, and non-tradable sector. The estimated elasticity of retail prices to house prices is unaffected. However, even after the inclusion of control variables, these estimates do not establish causality, since there might be an unobserved third factor, such as time-varying productivity, that could simultaneously move both house prices and retail prices. If we cannot directly control for this third factor in the OLS regression, we will obtain a biased estimate of

[23] While the difference in retail prices between high and low house-price-growth MSAs during the bust is smaller than during the boom, the elasticity is higher, because the difference in house price changes is smaller in the bust. In addition, sorting over 2001-2011 house price growth rather than separately over the boom and the bust produces similar patterns.

the elasticity of retail prices to house prices.

2.1.1 Price-Setting Behavior - Instrumental Variables Identification Strategy

Our first approach to dealing with this possible omitted variable bias is to exploit an instrumental variable that is correlated with house price changes over our periods of interest, but that does not directly affect retail prices. In particular, we follow an extensive recent literature that exploits across-MSA variation in housing supply elasticity as an instrument for changes in house prices (see, for example, Mian and Sufi, 2011, 2014*a*; Adelino, Schoar and Severino, 2013; Brown, Stein and Zafar, 2013; Bhutta and Keys, 2014). The intuition for this instrument is that for a fixed housing demand shock during the housing boom, house prices should rise more in areas where housing supply is less elastic.[24] This, in turn, generates increases in local demand in these areas (see previous references, and results in Section 2.5). During the housing bust, it is then precisely those areas where house prices rose the most that see the largest declines in house prices and demand (Glaeser, 2013).

We use two measures of housing supply elasticity as instruments for house price changes: the primarily geography-based measure of Saiz (2010), and the regulation-based measure from the Wharton Regulation Index (Gyourko, Saiz and Summers, 2008). Saiz (2010) uses information on the geography of a metropolitan area to measure the ease with which new housing can be constructed. The index assigns a high elasticity to areas with a flat topology without many water bodies, such as lakes and oceans. Gyourko, Saiz and Summers (2008) conduct a nationwide survey to construct a measure of local regulatory environments pertaining to land use or housing. Their index aggregates information on who can approve or veto zoning requests, and particulars of local land use regulation, such as the review time for project changes. In areas with a tighter regulatory environment, the housing supply can be expanded less easily in response to a demand shock, and prices should therefore rise by more. Appendix Table A2 presents results from the first-stage regression 1. Both instrument are highly predictive of house price changes over both periods, with low-elasticity MSAs experiencing larger house price gains during the housing boom, and larger house price drops during the housing bust.[25]

The exclusion restriction requires that housing supply elasticity affects retail prices only through its impact on house prices (see Appendix A for a formal statement of the exclusion restriction). To provide some evidence for the validity of the Saiz (2010) instrument, Mian and Sufi (2011, 2014*a*) show that wage growth did not accelerate differentially in elastic and inelastic CBSAs between 2002 and 2006. The authors also show that the instrument is uncorrelated with the 2006 employment share

[24] This national demand shock could, for example, result from the relaxation in downpayment requirements, or a decline in interest rates (see Favilukis, Ludvigson and Van Nieuwerburgh, 2010).

[25] Unsurprisingly, the power of the instrument is significantly stronger during the housing boom than during the housing bust. The first-stage F-stats of the Saiz (2010) instrument are 44.8 for 2001-2006, and 16.6 for 2007-2011. They are 39.1 and 12.6, respectively, for the Gyourko, Saiz and Summers (2008) instrument. Supply elasticity has predictive power during the bust because it reflects ex-post unraveling of the differential house price bubble. See Appendix A for additional discussion.

in construction, construction employment growth in the period 2002-2005, and population growth in the same period. Consistent with this, we find no relationship between housing supply elasticity and income growth in our sample: during the housing boom, income growth has a correlation of 0.040 with the Saiz (2010) instrument and -0.007 with the Wharton Regulation Index. These correlations are -0.224 and 0.054, respectively, for the housing bust, and never statistically significant.[26]

One channel that could violate the exclusion restriction is if changes in the degree of local retail competition were correlated with the housing supply elasticity. This might occur if the regulatory or geographic environment hindered the entry of new retail stores. In Section 2.3 we directly address this concern, and show that differential changes in competition do not explain our results.

The first and second stages of the IV regression are given by equations 1 and 2, respectively.

$$\Delta log(HousePrice)_m = \rho SupplyElasticity_m + \delta X_m + \epsilon_m \quad (1)$$

$$\Delta log(RetailPrice)_m = \beta \Delta log(\widehat{HousePrice})_m + \gamma X_m + \varepsilon_m \quad (2)$$

The unit of observation is an MSA, denoted by m. We estimate these regressions separately for the housing boom (2001-2006) and bust (2007-2011). The dependent variable in the second-stage regression is the change in retail prices over the period of interest. The coefficient of interest is β, which captures the causal effect of house price growth on retail price growth. X_m is a vector of controls.

We first present results using the housing supply elasticity from Saiz (2010) as an instrument for house price changes. Column 3 of Table I presents estimates from the second-stage regression. The elasticity of retail prices to house prices is about 12-13% during both the housing boom and the housing bust. These elasticities are about two times as large as the estimates from the OLS regressions presented in columns 1 and 2. This is consistent with the presence of local productivity shocks, which would lower retail prices but raise house prices. In other words, supply shocks directly imply an opposite relationship from demand shocks. Since our instrumental variables approach isolates the demand shock, it will produce a larger estimate.[27] Column 4 includes control variables for local economic conditions. The robustness of the estimated coefficients to the addition of these controls, as well as additional controls for changes in income and demographics that will be added in Section 2.3, helps to alleviate concerns about whether our instruments satisfy the exclusion restriction.

[26]The main objective in our paper is to document the response of retail prices and markups to demand shocks. Our identification approach exploits cross-sectional variation in house price changes to provide us with differential demand shocks. We believe that the IV approach in this section and homeownership interaction in the next section strongly point to a causal link from house-price-induced local demand shocks to retail prices. Nevertheless, it is worth noting that many potential violations of the exclusion restriction in our IV approach involve a correlation between housing supply elasticity and local demand factors. While we find no evidence for such a correlation, its presence would only mildly change our interpretation. In particular, while not all of the markup response would then represent a response to changes in house prices, it would still represent a markup response to demand changes. This would have the same implication for business cycle models as our preferred causal interpretation (see Section 3.1.1).

[27]In addition, measurement error in house price growth will also bias down the OLS estimates.

Column 5 and 6 of Table I show the instrumental variables estimates using the Wharton Regulation Index as an alternative measure of housing supply elasticity to instrument for house price changes. The estimated elasticity of retail prices to house prices is slightly stronger, with estimates between 15% and 22% depending on the exact specification.[28]

2.2 Price-Setting Behavior - Zip Code Level Identification Strategy

In the previous section we measured both house prices and retail prices at the MSA level. There are some advantages of these MSA-level estimates relative to estimates using house price and retail price measures at more disaggregated levels such as zip codes. First, nearly all grocery spending for a household should occur within MSAs, but this may not hold for zip codes. Second, both house price changes and retail price changes are measured more precisely for MSAs than for zip codes.[29] Third, our housing supply elasticity instruments do not vary at the zip code level. Therefore, we think the elasticities at the MSA level are the most reasonable to take away from our analysis.

Nevertheless, we now extend our analysis to the zip code level, because the large variation in homeownership rates across zip codes allows us to explore a separate, complementary identification strategy. In particular, the same change in house prices will induce different demand effects for homeowners and renters, since these households differ in their net asset position in housing. While house price increases can raise wealth or relax borrowing constraints for homeowners, they have no such effects on renters.[30] If house prices are capitalized into apartment rents or renters plan to purchase in the future, then higher house prices represent negative wealth shocks for renters.[31] Thus, if the positive relationship between retail prices and house prices is truly driven by house-price-induced demand shocks, then we would expect a stronger relationship in zip codes with high homeownership rates.

To explore this prediction, the bottom row of Figure II shows the average retail price level for zip codes in the top and bottom quartile of house price growth between 2001 and 2011. Panel E focuses on zip codes in the bottom quarter of the homeownership rate distribution (average of 46%), Panel F on zip codes in the top quarter of the homeownership rate distribution (average of 86%). Those zip codes with larger house price increases have higher retail price growth. However, as one would expect if house prices work through a wealth channel, the differential price growth is much larger in

[28] Even when looking at the two most distant point estimates of the elasticities, a t-test gives a p-value of 0.1673, so we cannot reject equality of the estimated elasticities across specifications.

[29] The repeat sales house price index at the zip code level is often based on a small number of sales. However, this measurement error biases us towards finding no relationship between house prices and retail prices, and our results persist using the median sales price index constructed by Zillow.

[30] These differential effects occur even in the framework of Sinai and Souleles (2005), since only homeowners receive the benefit of an increase in asset prices while both homeowners and renters face an increase in implicit rent.

[31] Using apartment rent data from REIS we find that the elasticity of apartment rents to house prices is 0.34 in the housing boom and 0.10 in the housing bust, so that there is significant capitalization of house prices into apartment rents, in particular during the boom (see Appendix Figure A2). The less-than-full pass-through of house price movements to rents is consistent with swings in the price-rent ratio over this period (see, for example, Sinai, 2013).

zip codes with higher homeownership rates than it is in zip codes with low homeownership rates.

Regression 3 formalizes this insight. As before, we estimate this specification separately for the housing boom period and the housing bust period. Since we do not have housing supply measures at the zip code level, we focus on ordinary least squares estimates.[32]

$$\begin{aligned}\Delta log(RetailPrice)_z &= \beta \Delta log(HousePrice)_z + \gamma HomeownershipRate_z + \\ &\quad \delta \Delta log(HousePrice)_z \times HomeownershipRate_z + \psi X_z + \varepsilon_z \end{aligned} \tag{3}$$

The results of this regression are presented in Table II. Columns 1 and 5 show the elasticity of retail prices to house prices without controlling for other covariates for the periods 2001-2006 and 2007-2011, respectively. The estimated elasticities are approximately 50% of the size of the MSA-level OLS estimates presented in Table I. As discussed above, this likely reflects attenuation bias relative to the MSA specifications, due to greater measurement error, plus the fact that some fraction of household spending will occur outside of a household's zip code of residence. The addition of control variables in columns 2 and 6 has little effect on the estimated elasticities.

Importantly, columns 3 and 7 of Table II interact house price changes with the homeownership rate in the zip code. The results show that house price increases are associated with particularly large increases in retail prices in zip codes with high homeownership rates. For zip codes with low homeownership rates, the effect of higher house prices on retail prices is, if anything, negative, although this point estimate is not statistically significant.[33]

These results significantly strengthen the argument for a causal effect of house prices on retail prices. In particular, any omitted variables that might be correlated with our housing supply elasticity instruments in Section 2.1, and which would thus violate the exclusion restriction, would also have to have a differential impact on homeowners and renters in order to explain our results.

One concern with the interpretation of the homeownership rate interaction could be that zip-code-level homeownership rate is proxying for the effects of some other neighborhood characteristic. For example, high-homeownership zip codes have lower population density, and therefore might have inhabitants that do more of their grocery shopping within the zip code. This could explain the larger measured response of local retail prices to local house prices in those areas, without relying on differential wealth effects. Similarly, low-homeownership zip codes primarily house younger citizens,

[32] Variation in homeownership rates occurs primarily within MSAs while our instruments do not vary within MSAs. Since the sources of variation are nearly orthogonal, IV interaction regressions have very little power. Thus, while we find similar effects, they are only marginally significant.

[33] One might worry that this relationship is driven by larger measurement error in house prices in areas with more renters, which could lead to larger attenuation bias. However, there is no strong relationship between homeownership rates and turnover: in zip codes in the bottom quartile of the homeownership distribution, about 2.1% of the housing stock turned over every year between June 2008 and March 2015; in zip codes in the top quartile of the homeownership rate distribution, this share was 2.2%. In addition, all results persist if we measure the change in house prices in regression 3 at the MSA-level.

who might be less responsive to house price changes for reasons unrelated to homeownership rates. To see whether these factors can explain our findings, columns 4 and 8 of Table II include controls for the population density (measured in thousand inhabitants per square mile) and the share of inhabitants under the age of 35, as well as their interaction with the change in house prices. Reassuringly, the estimated coefficients on the interaction of house price changes and homeownership rates is, if anything, slightly larger in this specification.

2.3 Changes in Markups or Pass-Through of Changes in Marginal Cost?

The previous sections provide evidence of a strong impact of house-price-induced demand shocks on retail prices. By definition, a change in retail prices can be decomposed into a change in marginal costs and a change in markups. While either channel would be interesting, in this section we provide several pieces of evidence that the relationship between house prices and retail prices is driven largely by markup variation. First, the vast majority of our retailers' marginal costs is the non-locally determined costs of goods sold; marginal costs should therefore not move substantially in response to local demand shocks. Consistent with this, we introduce data from a large national retailer that allow us to directly measure marginal costs and markups, and show that the estimated price response captures movements in markups. We then directly control for changes in retail rents and labor costs that could affect retailers' non-inventory marginal cost. We also argue that most cost-based stories for our pricing patterns would not interact with the local homeownership rate, and therefore cannot explain our findings from Section 2.2.

2.3.1 Local Share of Marginal Cost

For the typical grocery store, the cost of goods sold makes up approximately 75% of total costs.[34] It is more difficult to decompose the remaining 25%, but the majority of those costs represent fixed overheads (e.g., store rental costs, utilities, and corporate salaries) rather than costs that directly vary with sales. Thus, the cost of goods sold is likely to make up substantially more than 75% of all marginal costs. Furthermore, our data only include tradable goods, which are generally not produced locally. Thus, local demand shocks should not affect the retailers' cost of goods sold.[35] For this reason, a change in a retailer's local demand is unlikely to be correlated with the vast majority of its marginal

[34] For example, in its 2013 10-K statement, Safeway reports a cost of goods sold of $26.6bn, compared to operating and administrative expenses (which include store occupancy costs and backstage expenses, which, in turn, consist primarily of wages, employee benefits, rent, depreciation and utilities) of $8.9bn. Similarly, Walmart reported "cost of sales" of $385bn, compared to "operating, selling and administrative expenses" of $91.3bn.

[35] In commodity flows survey data, 76% of food and beverage shipments by gross value added are shipped more than 50 miles, and so are not locally produced. However, the local share of gross value added is much larger than the relevant local share of net value added. Local distributors are important for grocery stores, but they represent a small share of value added in production. Industry input-output tables from the BEA imply that the intermediate share of trucking/warehousing for food and beverage stores is 12.4%. This implies that a 24% local share in gross inputs corresponds to less than a 3% share of input costs for food and beverage stores being determined within an MSA.

cost, which implies that the increase in retail prices we observe mostly reflects higher markups.

While local wholesale costs should not be quantitatively important in general, there are certain products that do have a larger local cost component. If changes in local marginal costs were important, we would expect that those goods would contribute significantly to our estimated elasticity. In column 1 of Table III, we repeat the empirical analysis from Table I using a retail price index which excludes product categories classified as "perishable" or as "liquid" by Bronnenberg, Kruger and Mela (2008). Perishable products are more likely to be sourced locally, and thus have their prices affected by local shocks. Similarly, liquid products such as carbonated beverages are frequently bottled locally, and are thus subject to similar concerns. We obtain very similar estimates of the elasticity when excluding these potentially problematic product categories from our local retail price indices, confirming that a pass-through of local marginal costs is unlikely to explain our findings.

2.3.2 Marginal Cost Evidence from Large Retailer

To provide further evidence that we are capturing changes in markups rather than a pass-through of marginal costs, we next turn to the data from a large U.S. retail chain described in Section 1.2. As described in Eichenbaum, Jaimovich and Rebelo (2011), these data include a complete measure of the marginal cost of each item, which the retailer uses when determining prices and thus markups.

From these data we construct zip code-level price, marginal cost, and markup indices from January 2004 to June 2007.[36] We then run regression 3, using changes in the net price index and changes in the markup index as the dependent variables. This allows us to test directly whether changes in house prices lead to changes in marginal costs or changes in markups.

Table IV presents the results. Column 1 shows that, on average, zip codes with higher house price growth see an increase in retail prices. Interestingly, despite the fact that these data only cover one retailer, and a different time period, the estimated elasticity is similar to that in Table II, which was estimated using the broader IRI data. Importantly, column 2 shows that this increase in retail prices represents an increase in markups, rather than a pass-through of marginal cost.

Columns 3 and 4 of Table IV interact house price changes with the homeownership rate in the zip code. Columns 5 and 6 also control for the interaction of house price changes with demographic variables such as population density and age composition. Consistent with results in Section 2.2, the response of markups and prices to changes in house prices is increasing in the homeownership rate of the zip code. In zip codes with the lowest homeownership rates, increase in house prices actually cause retail prices and markups to fall.

[36] All of our results are unchanged if we use gross prices, or wholesale costs instead of marginal costs. Since our data only contain information from 39 MSAs, many with only a single store, we do not repeat the analysis at the MSA level.

2.3.3 Labor Costs

The previous sections argued that the cost of goods sold constitutes the vast majority of retailers' marginal costs, and that those marginal costs were unaffected by local demand shocks. We next address two other cost pass-through channels: labor costs and retail rents. If there was an increase in the shadow cost of labor, for example because of higher wages, retail prices might increase as retailers pass through this (small) component of marginal cost.

However, the controls for changes in the unemployment rate, changes in average weekly wages, and changes in employment shares in our baseline regressions in Table I already suggest that our findings are unlikely to be explained by the pass-through of labor costs.[37] The positive coefficient on the unemployment change between 2001-2006 in Panel A of Table I may seem surprising, but likely reflects important local supply shocks. If we control for the mean unemployment rate over the sample period instead of the change, as suggested by early Phillips curve relationships, then the coefficient becomes negative. This does not change the coefficient on house price changes (see column 2 of Table III). In addition, columns 3 and 4 of Table III show the robustness of the results in Table I to alternative labor market measures. First, local unemployment measures can be sensitive to measured local labor force participation, but using changes in employment-to-population ratio leaves our results unchanged. Second, grocery stores tend to hire labor which is less educated than the average population, but using controls for changes in wages or unemployment among those with at most a high school diploma in the ACS yields nearly identical results.

2.3.4 Retail Rents

We next explore whether a pass-through of higher commercial rents can explain the retail price response to house prices.[38] The most important piece of evidence against this channel is that the response of house prices to retail prices grows with local homeownership rates, and is essentially zero in areas with mainly renters (see Section 2.2). An increase in local rents should affect a firm's costs in the same way whether the firm is located in an area with many or few homeowners. Thus, an explanation for our price patterns which relies on pass-through of local land prices into commercial rents and retail prices will struggle to explain the observed homeownership interaction.

Nevertheless, we next directly control for changes in retail rents in our empirical specifications, using data on annual effective retail rents that we obtained from REIS for 45 MSAs. Appendix Fig-

[37] In addition, even if average local wages did vary with local house prices, national wage bargaining would reduce the effect that average local wages have on the wage cost of large national retailers.

[38] Whether rents should be considered a fixed cost or a variable cost in the running of a retail business depends on the time horizon considered. In the short-run, rents should probably not be considered a component of marginal cost (Gopinath et al., 2011). In an environment with entry and exit, an increase in fixed overhead costs would lead to a decline in the number of stores, and the resulting reduction in competition should lead to an increase in markups. As long as marginal costs remained constant, this pass-through channel would still represent an increase in markups.

ure A2 shows the relationship between changes in house prices and changes in retail rents over our sample period. The elasticity of retail rents to house prices is 0.2 in the housing boom, and 0.08 in the housing bust. This relatively low pass-through of house prices to retail rents is consistent with the long duration of retail lease contracts. As a first back-of-the-envelope calculation, even if retail rent made up an unrealistic 20% of marginal costs, these estimates suggest that rent pass-through could explain at most one-fifth of our retail price movements.[39]

To assess more formally the extent to which the (small) changes in retail rents can explain our results, Table V includes the average retail rent as a control variable in regression 2. While the statistical significance of the elasticity estimates declines due to the smaller sample size, our results suggest that the increase in retail prices in response to higher house prices is not driven by the pass-through of higher retail rents. If anything, controlling for changes in retail rents increases the estimated response of retail prices to changes in house prices.[40] As further evidence, in column 5 of Table III we exclude the six MSAs in our data with the largest level of retail rents, as identified in the 2012 Retail Research Report provided by Colliers International.[41] In these markets, retail rents are likely to make up a larger fraction of total costs; therefore, if the pass-through of higher retail rents were a significant factor in explaining our results, we would expect the estimated elasticity to be smaller when excluding cities with high retail rents. Contrary to this, the estimated elasticity is unchanged.

2.3.5 Demographic Changes/Gentrification

We next explore whether our results are driven by migration and changing demographics rather than by changes in the behavior of individuals already living in a location. If richer, less price-sensitive households moved into a location when house prices increase, or if retailers responded to an overall increase in demand due to more people living in an MSA, then this could change the interpretation of our results.[42] In column 6 of Table III we control for changes in income, and in column 7 for changes in the fraction of population that has completed at least high-school and at least a bachelor degree. In column 8 we control for population growth over our sample period. Our estimates are unaffected by the addition of these control variables. Consistent with this, Section 2.5 shows that individual household shopping behavior does indeed change in response to house price movements.

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[39] If 20% of marginal costs are retail rents, and the rent elasticity is 20%, then a doubling of house prices will increase marginal costs by $20\% \times 20\% = 4\%$. This is about one-fifth of the total increase in retail prices.

[40] While not significant, the point estimate of rents on retail prices is actually negative. While this may seem counter-intuitive, it can easily be explained if there are productivity shocks that vary across locations. In that case, higher productivity will simultaneously lead to lower prices and higher rents.

[41] The six cities are Boston, MA; Chicago, IL, New York, NY; Los Angeles, NY; San Franisco, CA; Washington, DC.

[42] In a constant elasticity model, optimal markups are only a function of the elasticity, and total demand should not matter. In other models, total demand could have an effect on the optimal markup.

2.3.6 Grocery Retail Entry

As discussed in Section 2.1, the most pertinent potential challenge to using housing supply elasticity as an instrument for house price changes is that changes in the competitiveness of the retail sector might be correlated with both the housing supply elasticity and with changes in retail prices. In particular, in areas with low supply elasticity we might observe not only faster house price increases in response to positive national housing demand shocks, but also less entry into the retail sector. If this were the case, low-elasticity cities would experience significant price growth both because of higher house prices and because there was less entry. On the other hand, our story suggests that retail markups increase in less elastic areas that experience increases in house prices; this increase in profitability should attract more entry, even if regulation might make such entry harder.

We first test the correlation between our measures of supply elasticity and measures of entry in the grocery retail sector. We use County Business Patterns data to measure entry as the change in the number of grocery retail establishments, normalized by the population.[43] Columns 1 to 3 of Appendix Table A3 shows the relationship between entry and house prices, as well as the correlation between entry and housing supply elasticity during the housing boom (Panel A) and housing bust (Panel B). We find that, during the housing boom, there was more entry in the less elastic regions that experienced larger increases in house prices. This provides further evidence that those regions experienced increases in retail markups: without an increase in profitability it is hard to explain why there would be more entry in less elastic regions where entry is, if anything, more restricted. Similarly, there is no evidence that our instrumental variables estimates for the housing bust are driven by an entry-based violation of the exclusion restriction.

In addition, column 9 of Table III directly controls for the change in the number of retail establishments per inhabitant (in addition to the share of grocery retail employment that is controlled for in all regressions reported in Table III). If anything, the estimated elasticity is slightly larger.[44] These results increase our confidence that the exclusion restriction is valid, and that differential housing supply elasticity does not directly affect the change in retail prices through affecting retail entry.

While locations with lower housing supply elasticity do not see reduced entry during the housing boom, one might wonder whether there is a more subtle interaction between supply elasticity, house price growth, and entry. In particular, increases in local house prices might reduce entry and competition, but only in locations where housing supply is inelastic. This, in turn, could result in an upward bias of our instrumental variables estimates, in which case the estimated markup variation would be partially driven by an entry channel rather than by a household demand channel. However, columns 4 and 5 of Appendix Table A3 show that there is no evidence for such interaction effects. Furthermore,

[43] We define grocery retail as NAICS codes 4451, 4452, 4453, and 4461. These roughly match the the store types in IRI data.

[44] Results are similar when we control for changes in the absolute number of retail establishments.

in order to explain the interaction with local homeownership rates in Section 2.2, there would need to be a strong relationship between entry and owner occupancy rates. However, when we include an additional interaction between entry and house price changes in regression 3, this does not affect our previous results. Appendix Table A5 also shows that much of the response of retail prices to house prices occurs at relatively high frequencies, where entry is unlikely to be of quantitative significance.

2.3.7 Product or Store Quality

It is worth re-emphasizing, at this point, that none of our results are explained by either changes in the composition of stores, or by changes in the composition of products within a store. This is because our price indices are capturing price changes for the same UPC in the same store. If a low-quality product is replaced with a higher-quality product with a higher price, this product substitution itself does not affect our price index. Similarly, if higher house prices lead to the entry of higher-quality stores which charge higher prices, this also does not affect our price index.

A second concern might be that changes within a particular store could affect the quality of the same UPC over time. However, while it might be conceivable that, as house prices go up, stores increase the "freshness" of their produce, this is much less likely for the the type of processed foods and toiletries that we observe in our data. In addition, increasing freshness would result in higher shipping and inventory cost, yet we observe no change in marginal cost in our large retailer data set.

Finally, one might be concerned about time-varying changes in the "shopping experience" of buying identical goods. Even if these were important, many changes to the shopping experience, such as upgrading the store, are fixed costs, so pass-through of these costs would reflect an increase in markups. Given the evidence in Section 2.3.1, any changes to the shopping experience that increase the marginal cost of selling a particular product, such as hiring more staff to reduce checkout lines, are likely to have a quantitatively small effect on retail prices. In addition, such increases in store quality through labor-intensification of the production process involve hiring more staff. This should show up as a change in the employment share of the grocery sector. Since we control for this employment share in all specifications, such labor-intensification cannot explain our findings. Finally, while grocery stores might be renovated during housing boom periods, it is unlikely that store owners actively degrade store appearance during the housing bust and differential depreciation during the housing bust would operate on a longer time scale, and cannot explain the results at quarterly frequency described in Appendix B.

2.4 Robustness Checks

We next provide additional robustness checks to the results presented above. We first address the geographic clustering of our measures of housing supply elasticity, which raises concerns that they might be correlated with unobserved regional shocks. To show that such unobserved shocks do not explain our results, we add geographic controls to the instrumental variables regression. In columns 1-3 of Appendix Table A4 we add a coastal indicator, four census region fixed effects, and nine census division fixed effects, respectively. The estimated elasticity of retail prices to house prices is unchanged, suggesting that it is not explained by regional shocks.

Next, while we believe that using the broadest price index available is the appropriate benchmark, a large literature has explored the implications of sales for monetary policy.[45] Column 4 of Appendix Table A4 shows that our results are robust to excluding temporary "sales" prices from the price index.

Finally, we want to ensure that our results are not driven by extreme outliers. In column 5 of Appendix Table A4 we exclude the MSAs with the largest and smallest 5% house price growth; in column 6 we drop observations from states that experienced some of the largest swings in house prices: California, Arizona and Florida. Our results are robust across these specifications.

2.5 Shopping Behavior

In the previous sections we documented a positive, causal relationship between house prices and retail prices. We argued that this relationship is not driven by an increases in retailers' marginal costs, and is therefore best explained by an increase in retail markups. The fact that the relationship is larger in zip codes with higher homeownership rates suggests that it is driven by house-price-induced demand shocks. In this section we provide further evidence on why retailers adjust markups following such shocks, arguing that this is the optimal response to a decrease in overall price elasticity. In particular, we show that increases in house prices lead homeowners to increase their nominal spending and to become less price sensitive, while renters purchase less and become more price sensitive.[46]

We use household-level information on purchasing behavior from Nielsen Homescan to analyze how changes in house prices affect household shopping behavior. Motivated by the differential response of retail prices to house prices in zip codes with different homeownership rates, we allow homeowners and renters to respond differently to house price changes.[47] The dependent variable in regression 4 captures the shopping behavior of household i, in zip code z, in quarter q.[48]

[45] For example, Nakamura and Steinsson (2008), Guimaraes and Sheedy (2011) and Kryvtsov and Vincent (2014).

[46] In most models, wealthier households place a higher value on leisure, and therefore allocate less time to shopping for cheaper prices (see Alessandria, 2009; Kaplan and Menzio, 2013; Huo and Ríos-Rull, 2014); this increases the optimal markup for firms. While it is possible that some individuals place positive utility value on the process of shopping, this is unlikely to be of aggregate importance, in particular for the set of grocery goods that we consider in this paper.

[47] We identify households living in one-family non-condo residences as homeowners, and families living in 3+ family, non-condo residences as renters. Replacing the household-level measure of homeownership with the zip code-level homeownership rate does not affect our estimates (See Appendix Tables A6 and A8 for details of that robustness check).

[48] There are a number of reasons we move to a quarterly specification for these regressions, rather than the long-difference specifications that we considered for the retail price analysis. First, the Nielsen data only start in 2004. More importantly, long-difference specifications rely on us observing the household for long time periods. Due to sample turnover, this would significantly reduce our sample size. Estimating quarterly regressions allows us to use households that we only observe for a limited amount of time (though we observe all households for at least one year). To facilitate comparability, Appendix Section B presents quarterly specifications of our retail price results.

$$
\begin{aligned}
ShoppingOutcome_{i,z,q} = \quad & \psi_i + \xi_q + \beta_1 log(HousePrices)_{z,q} + \beta_2 Homeowner_{i,q} + \\
& \beta_3 log(HousePrices)_{z,q} \times Homeowner_{i,q} + \gamma X_z + \epsilon_{i,q}
\end{aligned} \tag{4}
$$

We measure local house prices at the quarter × zip code level.[49] We include quarter fixed effects, ξ_q, to control for any aggregate time-trends. Importantly, we also control for household fixed effects, ψ_i. This keeps constant any household-specific determinants of shopping behavior, such as the disutility from comparing prices or the baseline preference for generic goods. The parameter β_1 is informative for changes in the shopping behavior of renters as house prices increase. The sum $\beta_1 + \beta_3$ captures how the shopping behavior of homeowners changes.

Columns 1 and 2 of Table VI show that increases in house prices lead to more retail spending by homeowners, but to reduced spending by renters (though that effect is not statistically significant). This evidence is highly consistent with homeowners consuming out of their increased housing wealth, and this increase in housing wealth generating a demand shock.

In columns 3 and 4 the dependent variable is the expenditure share on goods that are on sale. We find that as house prices increase, homeowners are less likely and renters are more likely to purchase goods that are on sale. This suggests that the increase in housing wealth makes homeowners less price sensitive and renters more price sensitive.

In columns 5 and 6 we use the share of purchases of cheaper generic goods as the dependent variable. A higher share of generic purchases again suggests higher price sensitivity. In columns 7 and 8 the dependent variable is the share of purchases made with a coupon, another measure of price sensitivity. Both measures decrease with house prices for homeowners, but increase for renters.[50]

One might be concerned that changing expenditure shares could reflect changes in the composition of goods purchased by households as they become richer, rather than changes in households' shopping intensity and price sensitivity. For example, a decline in the expenditure share on sale items could either reflect a reduction in the shopping intensity devoted to the same goods, or a change in the composition of purchases towards goods that are less often on sale. In the latter case we would see changes in expenditures share but this would not necessarily indicate a decline in price sensitivity. To test whether this is the case, Table VII presents results from a version of regression 4 in which the unit of observation is a shopping outcome for each household × quarter × product category.[51] As before,

[49] Appendix Tables A7 and A8 show that our results are robust to measuring house prices at the quarter × MSA level.

[50] The negative coefficient on homeowner, β_2, may seem counterintuitive, but household fixed effects mean it is only identified off of households that change tenure in the sample. It suggests that following a house purchase, households become more price sensitive, perhaps because of high mortgage expenditures. If we remove household fixed effects, homeowners have higher expenditures and are less price sensitive than renters. Dropping all households that change homeownership status leaves estimated coefficients for β_1 and β_3 unchanged.

[51] The individual product categories are health & beauty care, dry grocery, frozen food, dairy, deli, packaged meat, fresh

we include household fixed effects, but now also add product category $\times$ quarter fixed effects.

Columns 1 and 2 show that, for homeowners, higher house prices lead to higher total expenditures within each product category; higher house prices lead to lower expenditures for renters, though the effect is not statistically significant. The magnitude is similar to that in Table VI. Columns 3 and 4 show that the share of products bought on sale within each product category varies with house prices in the same way as when we pool across product categories. Similar results are obtained when looking at the share of goods purchased with a coupon and the share of generic goods purchased. This suggests that the observed changes in expenditure shares are truly driven by changing household price sensitivity, and not by compositional changes in the types of products purchased.

Finally, one might be interested in analyzing the extent to which our findings are driven by changes in the share of goods that are on sale (or in local availability of generics or coupons), rather than by changes in households' effort in searching for these sales. That is, we want to isolate changes in purchases which are driven by changes in household behavior from those driven by changes in firm behavior. To do this, we would ideally like to include zip code $\times$ quarter fixed effects to capture time-variation in the propensity of goods in a zip code to be on sale. However, this removes almost all of the variation, since we often only observe one household per zip code. In Appendix Table A7 we thus repeat regression 4 including MSA $\times$ quarter fixed effects. This controls for MSA-level changes in the share of goods offered on sale in response to changes in house prices. The estimated interaction between house prices and homeownership status remains economically and statistically significant.

The evidence in this section shows that wealth effects from higher house prices make homeowners less price elastic and renters more price elastic. Therefore, as house prices increase, retailers can increase their markups, in particular in areas with many homeowners.

2.6 Discussion of Magnitude

Our results have importance implications for retail price variation across locations, as well as for aggregate inflation. Over the housing boom, the 90-10 percentile difference in house price growth across MSAs was 45%. Multiplying this difference by our estimated boom elasticities of 15-23% implies that moving from the 10th percentile of MSA house price growth to the 90th percentile of MSA house price growth generates an increase in relative retail prices of 7-10%. This compares to an overall 90-10 difference in retail price changes of 12%. The same calculation in the housing bust implies that house price differences generate a 5-6% 90-10 retail price movement, as compared to an actual 90-10 difference of 8.4%. Given that the differential housing boom-bust across locations was one of the most important regional factors during this time period, we think it is indeed plausible that much of the variation in regional retail price changes can be explained through this channel.

produce, non-food grocery, alcoholic beverages and general merchandize.

How much did the increase in housing values over the boom contribute to aggregate inflation? Using CoreLogic Data, house prices grew by 36.5% from 2001-2006, while the overall CPI increased by 14.5%. Thus, real housing values increased by 22%. Multiplying this change by our IV elasticities of 15-23% implies that house price movements and their associated effects on retail markups explain roughly one-fourth to one-third of the overall increase in retail prices during this period.[52]

Thus, house price movements generate significant but plausible changes in retail prices. It is important to note that in addition to house price movements, there are many factors such as oil prices, trade patterns and monetary policy that influence aggregate inflation, so one would not expect the aggregate price level to precisely mirror the housing boom and bust. Indeed, Beraja, Hurst and Ospina (2015) argue that the presence of offsetting aggregate shocks is crucial for understanding aggregate inflation. For example, during the housing boom, increasing imports from China likely held down overall retail price increases despite upward pressure from house prices. Conversely, during the housing bust and the Great Recession, increasing financial frictions likely pushed prices to increase (see Ball and Mazumder, 2011; Del Negro, Giannoni and Schorfheide, 2014; Gilchrist et al., 2014).

Is the magnitude of markup variation implied by these price movements plausible? If markup variation explains all of the observed elasticities, then this implies markup changes of 3-5 percentage points over the housing boom and bust. We directly observe an average markup of roughly 60% for our large anonymous retailer, so a 3-5 percentage point movement does not seem unreasonable.[53] Assuming a constant elasticity of substitution, this implies a reduction from 2.65 to 2.55 in this elasticity.

Finally, it is important to note that all of our empirical estimates measure the response of prices and markups to house prices at medium-run business cycle frequencies. Section 2.3.6 shows that for the time horizons in our sample, store entry plays only a small role. However, over longer time horizons, entry should diminish these initial markup responses. That is, our conclusion that optimal markups are procyclical does not imply that there will be trend growth in markups in the long-run.

3 Implications

In this section we explore the implications of our empirical results. We divide our discussion into two parts: In the first part, we discuss implications that arise from our finding of procyclical desired flexible price markups. While we believe that we have made a strong case for interpreting our empirical results as markup variation, a number of important implications of our findings do not rely on this interpretation. Therefore, after describing the implications of markup variation, we turn to implications of price variation that would persist even if marginal costs had changed significantly.

[52] $0.22 \times 0.153/0.145 = 0.232; 0.22 \times 0.23/0.145 = 0.349$

[53] In the Census of Retail Trade, the average retail markup is only modestly lower at 39%. See Faig and Jerez (2005).

3.1 Implications of Markup Variation

3.1.1 Business Cycle Modeling

In many business cycle models, firm markups play an important role in determining the real response to expansionary monetary policy (see Goodfriend and King, 1997). For example, in New Keynesian models, firms produce differentiated products and have some pricing power for their variety.[54] Firm i faces demand with elasticity of substitution θ, and nominal price P_t^i:

$$c_t^i = \left(\frac{P_t^i}{P_t}\right)^{-\theta} C_t, \qquad \text{where} \qquad C_t = \left(\int \left(c_t^i\right)^{1-1/\theta} di\right)^{\frac{\theta-1}{\theta}}$$

is a consumption aggregate, and the aggregate price-level is given by:

$$P_t = \left(\int \left(p_t^i\right)^{1-\theta} di\right)^{\frac{1}{1-\theta}}.$$

With flexible prices, profit maximization implies that firms should set prices as a constant markup over nominal marginal cost, Ψ_t :

$$P_t^i = \frac{\theta}{\theta - 1}\Psi_t.$$

The average markup in the economy is, in turn, crucial for determining real output. Defining the average markup as the ratio of the price level to marginal cost, $\mu_t = \frac{P_t}{\Psi_t}$, the cost-minimizing solution for labor input, given demand for a firm's product, must satisfy:

$$W_t = \Psi_t \frac{\partial F\left(n_t, k_t\right)}{\partial n_t}.$$

Substituting from the above definition then gives that

$$\mu_t \frac{W_t}{P_t} = \frac{\partial F\left(n_t, k_t\right)}{\partial n_t},$$

so a higher average markup corresponds to a higher marginal productivity of labor, and a real reduction in output. In practice, average markups can change if marginal cost moves and some firms are unable to adjust prices, or if some adjusting firms' desired markups change. In the traditional New Keynesian mechanism, θ does not move, so firms' desired markups are constant, and all actual markup variation is driven by sticky prices. For example, expansionary monetary policy drives up aggregate demand and marginal cost, and leads to a reduction in realized markups for firms with sticky prices. This generates a reduction in aggregate markups and an increase in real output. Thus, sticky prices contribute to countercyclical markups in response to demand shocks.

[54] This product differentiation can come from any feature of the product, including store location.

In this paper we identify an entirely separate channel that puts procyclical pressure on markups, even in an economy with flexible prices. In particular, as households become wealthier, θ_t falls and firms' desired markups increase. In practice, both the sticky price channel and the shopping intensity channel will affect aggregate markups. Sticky prices will put upward pressure on markups in recessions, while greater shopping intensity will push in the opposite direction.[55] To be clear, the countercyclical shopping channel we identify does not imply that the sticky price effect is unimportant, or that total markups are not countercyclical; however, this shopping channel has important implications for the conduct of monetary policy.[56]

To see this, consider the DSGE models in Smets and Wouters (2007), Christiano, Motto and Rostagno (2010), and Justiniano, Primiceri and Tambalotti (2011). These models allow for exogenous "cost-push" shocks to the desired markup and find they play an important role in explaining inflation dynamics. However, there is an important distinction between markup movements in these papers and in ours. In these DSGE models, movements in the desired markup are interpreted as exogenous "structural" shocks, and as such they do not respond to policy. In contrast, we provide evidence for endogenous desired markups: during booms, households become less price-sensitive, and firms raise markups in response. This is an important distinction, because our results imply that desired markups will work against the traditional expansionary effects of stimulus policy. Expansionary monetary policy may lower markups through a traditional New Keynesian channel, which will in turn drive output up. However, as output begins to rise, households will become less price-sensitive, which puts upward pressure on markups. Treating movements in the desired markup as exogenous structural shocks shuts down this feedback. That is, a standard Lucas critique applies to treating the endogenous response of households as policy invariant.

3.1.2 Aggregate Time-Series Movements of Markups

Our results also contribute to a large literature using aggregate time-series data to measure the cyclicality of markups, μ_t, in an attempt to test New Keynesian models. Nekarda and Ramey (2013) review that literature. While looking at time-series variation in total markups might be the right approach for measuring the total effects of a policy change, if one is interested in isolating the effects of sticky prices, one needs to hold price elasticity θ_t fixed. If firms' desired markups are constant, then measured μ_t only moves due to sticky prices, but once θ_t changes across time, then this no longer holds.

[55] In Appendix D we argue that with standard New Keynesian pricing frictions, our estimated elasticity of retail prices to house prices is actually a lower bound on the effect of house price changes on firms' desired markups.

[56] Our evidence on firm price setting and markups comes from a set of non-durable retail goods so one should be cautious in generalizing to aggregate markups. However, Panel C of Figure I shows that across cities, the food-at-home CPI moves closely with the broader CPI, which suggests that the price movements we identify generalize to a large set of goods. It is also important to note that our channel for markup variation is complementary to ones in which price elasticity changes because of cyclical shifts in the composition of purchases towards more price-sensitive buyers of durable goods (Bils, 1989), or towards less price-sensitive higher-income households (Edmond and Veldkamp, 2009).

Furthermore, if price flexibility varies across time, as suggested by Vavra (2014), then the decomposition of the total markup into a "desired markup effect" and a "sticky price effect" will also vary across time. Without this decomposition, it is hard to determine what aggregate markups tell us about New Keynesian models. Business cycle movements in markups may reflect movements in desired markups rather than the contribution of sticky prices. Time-variation in the strength of these two effects can also potentially reconcile conflicting evidence on the response of total markups to demand shocks. For example, Gali, Gertler and Lopez-Salido (2007) find that markups fall in response to expansionary monetary policy. However, using an identical methodology, Nekarda and Ramey (2013) show that this result changes when using revised data for the last few years of the sample.

3.2 Implications of Price Variation

3.2.1 Housing Wealth Effect, and Aggregate Implications

We also contribute to the literature that analyzes the effects of house price changes on household behavior (e.g., Case, Quigley and Shiller, 2011; Carroll, Otsuka and Slacalek, 2011). From a theoretical perspective, it is unclear whether changes in house prices should induce significant wealth effects for homeowners. In particular, Sinai and Souleles (2005) argue that while house price increases lead to higher values of homeowners' housing assets, they simultaneously increase the houses' implicit rent. If households never move or die, these effects exactly cancel out, so that homeowners are not affected by house price changes. Our results strongly reject this theoretical benchmark and show that homeowners clearly change their behavior in response to house price changes.[57]

Our results also affect the interpretation of studies that estimate the response of household consumption to housing wealth shocks using sub-national variation in house prices. (e.g., Campbell and Cocco, 2007; Mian and Sufi, 2014*a*). These studies find strong responses of nominal consumption to local house price movements, but since they do not have access to disaggregated price indices, they cannot further decompose nominal consumption growth into real consumption growth and inflation. Mian and Sufi (2014*a*) specifically make this point when extrapolating their local estimates to consider the aggregate effects of the housing boom and bust. In particular, they caution that the inflation response to demand shocks is a critical input to this aggregate calculation for which they do not have direct empirical evidence. Our results suggest that such caution is indeed warranted. In particular,

[57] A number of channels can lead house price changes to have real effects. For example, if households can borrow against housing collateral, then increases in house prices will lead to a loosening of households' borrowing constraints. Relatedly, Mian and Sufi (2014*a*) point out how such a response might occur in the presence of "cash-on-hand" consumers. More direct housing wealth effects can also obtain from a target bequest channel. In addition, if households plan to move to lower-priced areas in the future, their discounted implicit rental cost goes up by less than the house price. Furthermore, if households plan to refinance their mortgage, the higher house prices reduce the loan-to-value ratio of the mortgage, and thus the monthly mortgage payments that need to be made. Finally, even if households are not planning on moving, behavioral stories can also lead to similar effects. While our empirical evidence strongly rejects theoretical models that suggest housing price changes should not have any effects on household behavior, our evidence is somewhat less strong about which particular channel is driving our result.

we find that house-price-induced demand shocks lead to higher retail prices, explaining at least some of the observed increases in nominal consumption.

Our first identification strategy is identical to that in Mian and Sufi (2014*a*), so the retail price responses we measure are directly relevant for measuring local real responses to housing wealth shocks. We find that marginal costs do not respond to these local housing wealth shocks. In contrast, when aggregate demand increases, this will increase marginal cost, so that prices will also rise through more traditional channels. In that sense, the local retail price response to local demand shocks that we measure most likely understates the aggregate price response to aggregate demand shocks.

3.2.2 Implications for Urban and Labor Economics

The response of local retail prices to local house prices can also help inform important parameters in models of urban economics (e.g., Shapiro, 2006; Albouy, 2009). In equilibrium models along the lines of Roback (1982), households and firms have to be indifferent between locating in different areas. Each area is endowed with its own productivity and consumption amenities. Wages must be higher in more productive locations, otherwise firms would want to move there. Housing costs also have to be higher in those more productive regions to discourage all households from moving there. Land prices capitalize consumption amenities, making it more expensive to live in more desirable regions. The utility consequences of a change in land prices depend on whether this change has an impact on the cost of traded and non-traded consumption goods. This affects the adjustment mechanism to local shocks, as well as the incidence of these shocks. Our causal estimate of the impact of house prices on retail prices therefore directly informs the calibration of these equilibrium models.

A related literature considers the extent to which local price changes provide insurance against local shocks. For example, Notowidigdo (2011) argues that negative labor market shocks cause house prices to fall, which can help households smooth consumption by reducing housing expenditures. Our findings suggest that local retail prices provide a general equilibrium channel that further dampens the effects of negative local wealth or productivity shocks: local productivity shocks that reduce house prices and housing wealth will cause retail prices to fall, making it cheaper to live in that area.

4 Conclusion

We link detailed geographic data on local house prices, retail prices, and household shopping behavior to provide new evidence on how the economy responds to changes in demand. We argue that exogenous increases in house prices lead to changes in demand for homeowners who become less price sensitive, and that firms respond by raising markups. Consistent with this interpretation, we find much stronger retail price responses to changes in house prices when homeownership rates are high. We also find evidence of differential shopping responses to house price changes for owners and renters. The economic magnitude of our price effect is large but not implausible: we estimate elasticities of retail prices to house prices of 15%-20%, and show that this channel can explain a large fraction of geographic variation in retail price changes.

As we discussed above, our results have a variety of applications from business cycle modeling to urban economics; in addition, we believe that this type of geographically disaggregated analysis can be extended to explore additional important questions. For example, our data could be used to learn about local house price expectations of households and firms. While we concentrated on constructing price indices for identical items in a fixed set of stores, there are also interesting questions about how product quality responds to increases in house prices and gentrification. In future work, we plan to further explore how the markup variation we identify interacts with local industry dynamics and firm entry. We are also interested in exploring the implications of our markup channel for income inequality within and across cities.

On the business cycle front, more could be learned by studying the response of local prices to various alternative shocks. We have concentrated on the response of retail prices to local house prices, but in future research we plan to explore the response to local credit shocks as well as to large labor market shocks such as the relocation of major employers. This should provide a broader picture of how inflation responds to various changes in economic conditions.

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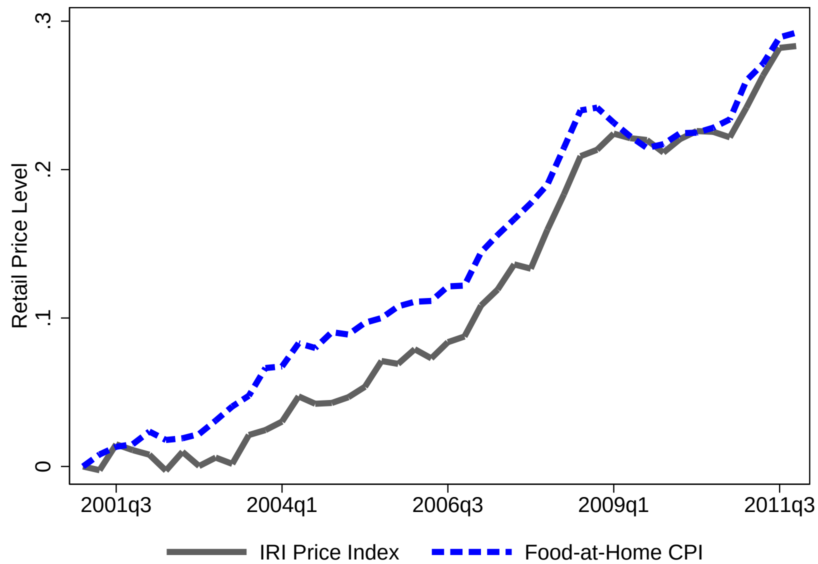
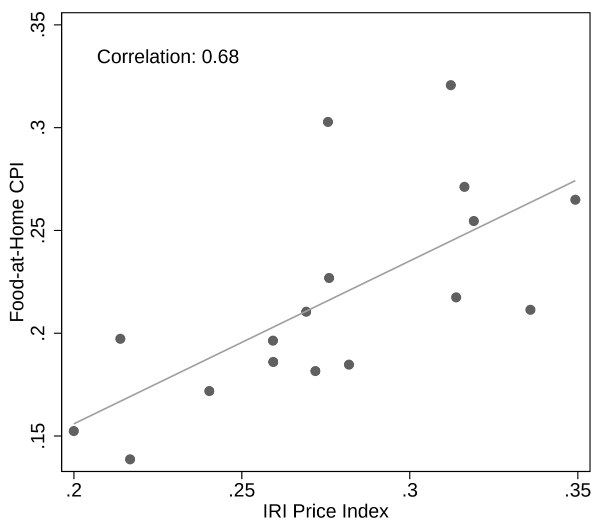
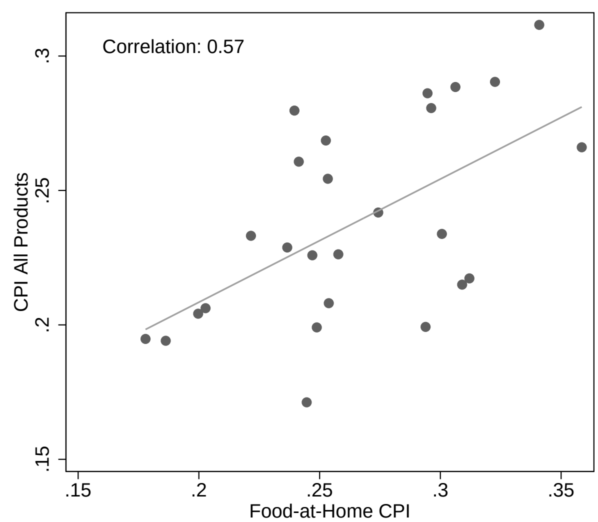
Figure I: Price Index vs. BLS



(A) IRI Data vs. Food-at-Home CPI



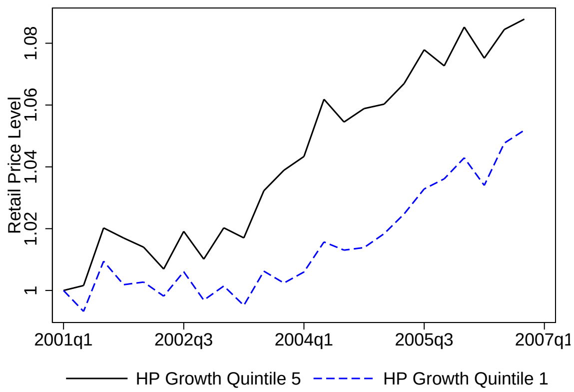
(B) Metro-Level Comparison: $log(P_{2011}) - log(P_{2001})$



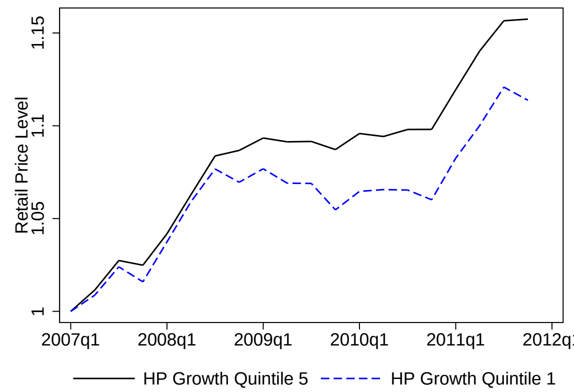
(C) Metro-Level Comparison: $log(P_{2011}) - log(P_{2001})$

Note: Figure shows comparisons of our price indices produced with IRI data to price indices provided by the BLS. Panel A compares our aggregate price index to the food-at-home CPI. Panel B compares the change in prices between 2001 and 2011 using our local price indices to the change in the metro area food-at-home price indices provided by the BLS for the set of MSAs where we have overlapping data. Panel C compares the change in prices between 2001 and 2011 of metro area food-at-home prices to the change in "all product" prices from the BLS.

Figure II: Retail Prices vs. House Prices



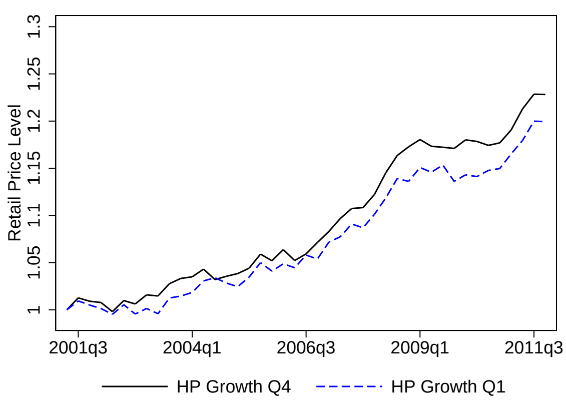
(A) Retail Price Level: 2001-2006



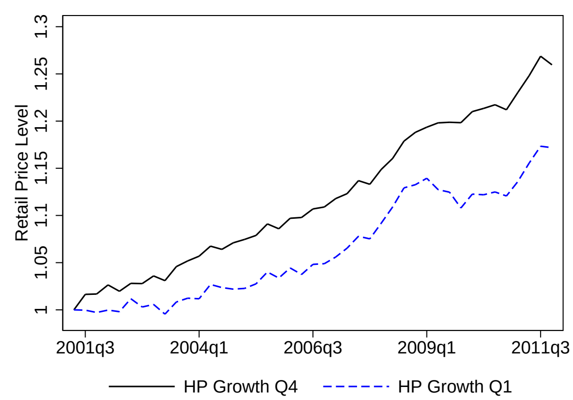
(B) Retail Price Level: 2007-2011

(C) Retail Prices vs. House Prices: 2001-2006

(D) Retail Prices vs. House Prices: 2007-2011



(E) Retail Price Level – Q1 of Homeownership



(F) Retail Price Level – Q4 of Homeownership

Note: The top row shows the average retail price level over time for MSAs in the top quintile (solid black line) and bottom quintile (dashed blue line) of house price appreciation for the period 2001-2006 (Panel A), and the period 2007-2011 (Panel B). The middle row shows the MSA-level correlation between changes in house prices and changes in retail prices for the period 2001-2006 (Panel C), and the period 2007-2011 (Panel D), as well as the line of best fit. The bottom row shows the average retail price level over time for zip codes in the top quartile (solid black line) and bottom quartile (dashed blue line) of house price appreciation between 2001 and 2011. Panel E shows results of zip codes in the bottom quartile of the homeownership rate distribution, Panel F shows results of zip codes in the top quartile of the homeownership rate distribution.

Table I: Retail Prices vs. House Prices: MSA-Level Analysis

	Panel A: Time Period: 2001 - 2006 Dependent Variable: Δ Retail Prices					
	OLS		IV Saiz		IV Wharton	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ House Prices	0.057***	0.068***	0.129***	0.153***	0.224***	0.230***
	(0.020)	(0.023)	(0.042)	(0.058)	(0.048)	(0.048)
Δ Share Grocery Retail Employment		-0.068		0.132		0.219
		(0.360)		(0.376)		(0.391)
Δ Share Nontradable Employment		0.073		-0.082		-0.130
		(0.182)		(0.175)		(0.174)
Δ Share Construction Employment		-0.060		0.012		0.039
		(0.098)		(0.114)		(0.130)
Δ Unemployment		0.039**		0.070**		0.095***
		(0.018)		(0.029)		(0.026)
Δ Wage		0.039		0.038		-0.005
		(0.055)		(0.060)		(0.061)
Number of Observations	125	125	112	112	112	112

	Panel B: Time Period: 2007 - 2011 Dependent Variable: Δ Retail Prices					
	OLS		IV Saiz		IV Wharton	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ House Prices	0.085***	0.086***	0.124***	0.146***	0.147***	0.157***
	(0.015)	(0.018)	(0.041)	(0.049)	(0.048)	(0.043)
Δ Share Grocery Retail Employment		-0.090		0.008		-0.000
		(0.264)		(0.275)		(0.282)
Δ Share Nontradable Employment		0.086		-0.000		-0.003
		(0.139)		(0.169)		(0.172)
Δ Share Construction Employment		0.050		-0.024		-0.040
		(0.127)		(0.135)		(0.147)
Δ Unemployment		0.000		0.017		0.019
		(0.011)		(0.015)		(0.014)
Δ Wage		-0.030		-0.060		-0.063
		(0.044)		(0.048)		(0.049)
Number of observations	126	126	112	112	112	112

Note: Table shows results from the following OLS regression: $\Delta log(RetailPrice)_m = \beta \Delta log(HousePrice)_m + \gamma X_m + \varepsilon_z$ in columns 1 and 2, and from instrumental variables regression 2 in the other columns. The unit of observation is an MSA, the dependent variable is the change in retail prices in 2001-2006 (Panel A) and 2007-2011 (Panel B). We instrument for the change in house prices using measures of the housing supply elasticity provided by Saiz (2010) in columns 3 and 4, and the Wharton Regulation Index described in Gyourko, Saiz and Summers (2008) in columns 5 and 6. Robust standard errors in parenthesis. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).

Table II: Retail Prices vs. House Prices: Zip Code-Level Analysis

	Dependent Variable: Δ Retail Prices							
	Period: 2001-2006				Period: 2007-2011			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ House Prices	0.023***	0.048***	-0.045	-0.170*	0.040***	0.030***	-0.035	-0.072
	(0.007)	(0.009)	(0.032)	(0.095)	(0.007)	(0.008)	(0.033)	(0.084)
Δ Unemployment		0.056***	0.059***	0.057***		-0.015**	-0.016**	-0.014*
		(0.012)	(0.012)	(0.013)		(0.008)	(0.008)	(0.008)
Δ Wage		0.058**	0.048	0.047		0.011	0.006	0.007
		(0.029)	(0.029)	(0.029)		(0.024)	(0.025)	(0.025)
Δ Share Grocery Retail Employment		-0.230	-0.252	-0.241		0.131	0.119	0.078
		(0.300)	(0.297)	(0.295)		(0.225)	(0.222)	(0.216)
Δ Share Nontradable Employment		0.080	0.095	0.085		0.018	0.026	0.037
		(0.123)	(0.123)	(0.122)		(0.101)	(0.101)	(0.101)
Δ Share Construction Employment		-0.152**	-0.177***	-0.184***		0.072	0.078	0.114
		(0.065)	(0.065)	(0.071)		(0.071)	(0.071)	(0.076)
Homeownership Rate			-0.063**	-0.120***			0.030	0.021
			(0.027)	(0.046)			(0.019)	(0.030)
Δ House Prices			0.142***	0.222***			0.095**	0.123*
× Homeownership Rate			(0.047)	(0.081)			(0.047)	(0.071)
Population Density				0.001				-0.000
				(0.002)				(0.001)
Δ House Prices				-0.002				0.002
× Population Density				(0.003)				(0.003)
Share below 35 years				-0.002**				-0.000
				(0.001)				(0.001)
Δ House Prices				0.003*				0.000
× Share below 35 years				(0.002)				(0.002)
N	708	708	708	708	846	846	846	846

Note: Table shows results from regression 3. The unit of observation is a zip code, the dependent variable is the change in retail prices in 2001-2006 in columns 1 - 4, and the change in retail prices in 2007-2011 in columns 5 - 8. Robust standard errors in parenthesis. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).

Table III: Markup or Marginal Cost?

	Dependent Variable: Δ Retail Prices								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Instrument with Saiz Supply Elasticity; 2001 - 2006								
Δ House Prices	0.145***	0.115**	0.111**	0.168**	0.159***	0.148**	0.157***	0.158***	0.151***
	(0.065)	(0.046)	(0.047)	(0.048)	(0.059)	(0.058)	(0.058)	(0.056)	(0.058)
	Panel B: Instrument with Saiz Supply Elasticity; 2007 - 2011								
Δ House Prices	0.121***	0.131***	0.140***	0.129***	0.127**	0.129***	0.164***	0.140***	0.134***
	(0.059)	(0.039)	(0.045)	(0.045)	(0.049)	(0.043)	(0.052)	(0.051)	(0.045)
	Panel C: Instrument with Wharton Regulation Index; 2001 - 2006								
Δ House Prices	0.194***	0.196***	0.194***	0.265***	0.224***	0.259***	0.262***	0.272***	0.223***
	(0.052)	(0.045)	(0.042)	(0.087)	(0.048)	(0.051)	(0.053)	(0.055)	(0.047)
	Panel D: Instrument with Wharton Regulation Index; 2007 - 2011								
Δ House Prices	0.246***	0.155***	0.166***	0.156***	0.152***	0.159***	0.184***	0.149***	0.154***
	(0.054)	(0.042)	(0.044)	(0.045)	(0.045)	(0.042)	(0.051)	(0.043)	(0.042)
Controls	✓	✓	✓	✓	✓	✓	✓		
Robustness	Exclude liquids and perishable goods	Average unemployment rate	Control for Employment to Population	Control for low education wage & unemployment	Drop high retail rent cities	Control for changes in income	Control for changes in education	Control for population growth	Control for Entry

Note: Table shows results from regression 2. The unit of observation is an MSA, the dependent variable is the change in retail prices in 2001-2006 (Panels A and C) and 2007-2011 (Panels B and D). In Panels A and B we instrument for the change in house prices using the housing supply elasticity measure provided by Saiz (2010); in Panels C and D we instrument for house price changes using the Wharton Regulation Index described in Gyourko, Saiz and Summers (2008). All specifications control for changes in the unemployment rate, changes in wages, and changes in the employment share in the construction, non-tradable, and grocery retail sector. Column 1 drops all product categories classified as "perishable" in Bronnenberg, Kruger and Mela (2008), as well as all liquids from our construction of the local price index. Column 2 controls for the average unemployment rate over the sample, rather than for changes in the unemployment rate. Column 3 controls for changes in the employment-to-population ratio, rather than changes in the unemployment rate. Column 4 controls for changes in the wage and unemployment of lower-educated people in the ACS, defined as those with at most a high school diploma. Column 5 drops the 6 cities with the highest level of retail rents. Column 6 controls for changes in income using data from the IRS. Column 7 controls for changes in the share of people who have completed high school, and changes in the share of people who have completed a bachelor degree. Column 8 controls for population growth using data from the annual population estimates for Metropolitan Statistical Areas produced by the U.S. Census. Column 9 controls for changes in the number of grocery retail establishments per 1,000 citizens. Robust standard errors in parenthesis. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table IV: Zip Code Pricing Results - Large Retailer

	(1) Δ Retail Prices	(2) Δ Markups	(3) Δ Retail Prices	(4) Δ Markups	(5) Δ Retail Prices	(6) Δ Markups
Δ House Prices	0.018**	0.039***	-0.065*	-0.032	-0.093	-0.132*
	(0.008)	(0.007)	(0.036)	(0.031)	(0.083)	(0.077)
Δ Unemployment	0.029**	0.044***	0.028*	0.043**	0.026*	0.045***
	(0.014)	(0.017)	(0.014)	(0.017)	(0.014)	(0.017)
Δ Wage	0.049**	0.058***	0.050**	0.058***	0.053**	0.051**
	(0.019)	(0.019)	(0.019)	(0.020)	(0.021)	(0.023)
Δ Share Grocery Retail Employment	-0.112	0.315	-0.119	0.309	-0.099	0.278
	(0.156)	(0.226)	(0.152)	(0.225)	(0.152)	(0.214)
Δ Share Nontradable Employment	0.193**	0.181*	0.182**	0.174	0.176**	0.182*
	(0.091)	(0.108)	(0.088)	(0.106)	(0.088)	(0.101)
Δ Share Construction Employment	0.094	-0.119	0.089	-0.123	0.074	-0.118
	(0.077)	(0.077)	(0.075)	(0.077)	(0.077)	(0.078)
Homeownership Rate			-0.016	-0.017	-0.038**	-0.041**
			(0.011)	(0.011)	(0.019)	(0.019)
Δ House Prices			0.115**	0.098**	0.146**	0.172***
× Homeownership Rate			(0.047)	(0.041)	(0.072)	(0.065)
Population Density					-0.001	0.001
					(0.001)	(0.001)
Δ House Prices					0.005	-0.000
× Population Density					(0.004)	(0.004)
Share below 35 years					-0.000	-0.001*
					(0.000)	(0.000)
Δ House Prices					-0.001	0.002
× Share below 35 years					(0.002)	(0.002)
N	192	192	192	192	192	192

Note: Table shows results from regression 3. The unit of observation is a zip code, the dependent variable is the change in retail prices (columns 1, 3, and 5) or retail markups (columns 2, 4, and 6) for a large national retailer between January 2004 and June 2007. Robust standard errors in parenthesis. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table V: Controlling for Retail Rent

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Time Period: 2001 - 2006								
	Dependent Variable: Δ Retail Prices								
	OLS			IV (Saiz)			IV (Wharton)		
Δ House Prices	0.063**	0.084**	0.074**	0.088*	0.138	0.122	0.188***	0.459*	0.470*
	(0.028)	(0.034)	(0.036)	(0.052)	(0.131)	(0.139)	(0.059)	(0.238)	(0.269)
Δ Retail Rent		-0.101	-0.092		-0.221	-0.194		-1.192	-1.216
		(0.122)	(0.122)		(0.413)	(0.421)		(0.771)	(0.846)
Δ Wage			0.115			0.097			-0.036
			(0.173)			(0.170)			(0.160)
N	45	45	45	42	42	42	42	42	42
	Panel B: Time Period: 2007 - 2011								
	Dependent Variable: Δ Retail Prices								
	OLS			IV (Saiz)			IV (Wharton)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ House Prices	0.104***	0.114***	0.109***	0.105***	0.114***	0.105**	0.132**	0.129**	0.119**
	(0.022)	(0.023)	(0.023)	(0.041)	(0.044)	(0.041)	(0.052)	(0.053)	(0.051)
Δ Retail Rent		-0.121	-0.120		-0.233	-0.232		-0.275	-0.270
		(0.123)	(0.121)		(0.180)	(0.163)		(0.209)	(0.193)
Δ Wage			0.133*			0.125*			0.120
			(0.077)			(0.069)			(0.074)
N	45	45	45	42	42	42	42	42	42

Note: Table shows results from regression 2. The unit of observation is an MSA, the dependent variable is the change in retail prices in 2001-2006 (Panel A) and 2007-2011 (Panel B). We show results from an OLS specification (columns 1-3), as well as instrumental variables specifications that instrument for the change in house prices using the Saiz (2010) measure of housing supply elasticity (columns 4-6) and the Wharton Regulation Index described in Gyourko, Saiz and Summers (2008) (columns 7-9). The sample is restricted to MSAs for which we observe retail rents in the REIS data. Robust standard errors in parenthesis. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).

Table VI: Effect of House Prices on Shopping Behavior

DEPENDENT VARIABLE:	LOG(EXPENDITURE)		SHARE "DEAL"		SHARE GENERIC		SHARE COUPON	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(House Price)	-0.018	-0.021	0.012**	0.021***	-0.002	0.001	0.006**	0.007***
	(0.014)	(0.015)	(0.005)	(0.005)	(0.003)	(0.003)	(0.002)	(0.003)
$\mathbb{1}_{Homeowner}$	-0.214***	-0.221***	0.112***	0.127***	0.029**	0.040***	0.063***	0.073***
	(0.070)	(0.072)	(0.025)	(0.026)	(0.013)	(0.013)	(0.012)	(0.012)
log(House Price)	0.050***	0.052***	-0.022***	-0.025***	-0.005**	-0.008***	-0.012***	-0.014***
$\times \mathbb{1}_{Homeowner}$	(0.014)	(0.014)	(0.005)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)
Unemployment Rate		0.078		0.146***		0.021		-0.029*
		(0.086)		(0.027)		(0.016)		(0.015)
Average Weekly Wage		0.007		0.004		- 0.000		0.001
		(0.014)		(0.004)		(0.002)		(0.002)
Share Grocery Retail		0.123**		-0.025		-0.008		0.004
Employment		(0.050)		(0.016)		(0.009)		(0.009)
Share Nontradable		0.167***		-0.058***		0.007		-0.027***
Employment		(0.052)		(0.017)		(0.009)		(0.009)
Share Construction		-0.298***		0.082***		0.011		0.041**
Employment		(0.098)		(0.032)		(0.019)		(0.018)
Quarter Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Household Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
R-squared	0.715	0.715	0.867	0.867	0.730	0.731	0.764	0.764
$\bar{y}$	6.697	6.700	0.281	0.281	0.174	0.175	0.079	0.079
N	830,142	802,200	839,142	802,200	839,142	802,200	839,142	802,200

Note: Table shows results from regression 4. The unit of observation is a household-quarter, the sample is 2004 to 2011. The dependent variables are the log of total household expenditure (columns 1 and 2), the expenditure share of products that are on sale (columns 3 and 4), the expenditure share of generic products (columns 5 and 6), and the expenditure share of products purchased with a coupon (columns 7 and 8). House prices are measured at the zip code level. All specifications include household and quarter fixed effects. In columns 2, 4, 6, and 8 we also include additional control variables. Each observation is weighted by the household sampling weight. Standard errors are clustered at the zip code $\times$ quarter level. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table VII: Effect of House Prices on Shopping Behavior - Disaggregated by Product Category

Dependent Variable:	log(Expenditure)		Share "Deal"		Share Generic		Share Coupon	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(House Price)	-0.016	-0.015	0.008*	0.018***	-0.001	-0.001	0.006***	0.007***
	(0.014)	(0.014)	(0.004)	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)
$\mathbb{1}_{Homeowner}$	-0.170**	-0.184***	0.092***	0.105***	0.030**	0.041***	0.063***	0.073***
	(0.067)	(0.069)	(0.023)	(0.023)	(0.012)	(0.012)	(0.011)	(0.011)
log(House Price) $\times \mathbb{1}_{Homeowner}$	0.040***	0.044***	-0.018***	-0.020***	-0.006**	-0.008***	-0.012***	-0.014***
	(0.013)	(0.014)	(0.004)	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)
Unemployment Rate		0.152*		0.155***		-0.017		-0.034**
		(0.083)		(0.025)		(0.014)		(0.014)
Average Weekly Wage		0.001		0.005		0.000		0.002
		(0.013)		(0.004)		(0.002)		(0.002)
Share Grocery Retail Employment		0.125**		-0.053***		0.009		-0.021**
		(0.051)		(0.015)		(0.009)		(0.009)
Share Nontradable Employment		0.135***		-0.025*		0.005		0.003
		(0.048)		(0.015)		(0.009)		(0.009)
Share Construction Employment		-0.172*		0.100***		-0.024		0.054***
		(0.094)		(0.029)		(0.017)		(0.017)
Product Category × Quarter Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Household Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
R-squared	0.640	0.641	0.664	0.664	0.460	0.460	0.494	0.495
$\bar{y}$	4.444	4.446	0.271	0.271	0.189	0.189	0.077	0.077
N	6,055,647	5,793,112	6,055,647	5,793,112	6,055,647	5,793,112	6,055,647	5,793,112

Note: Table shows results from regression 4. The unit of observation is a household-quarter-product category, the sample is 2004 to 2011. The dependent variables are the log of total household expenditure (columns 1 and 2), the expenditure share of products that are on sale (columns 3 and 4), the expenditure share of generic products (columns 5 and 6), and the expenditure share of products purchased with a coupon (columns 7 and 8). House prices are measured at the zip code level. All specifications include household fixed effects and product category × quarter fixed effects. In columns 2, 4, 6, and 8 we also include additional control variables. Each observation is weighted by the household sampling weight and the expenditure share of the product category in the household's total expenditure. Standard errors are clustered at the zip code × quarter level. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).

House Prices, Local Demand, and Retail Prices

Online Appendix

Johannes Stroebel Joseph Vavra

A Identification Concerns and Instrumental Variables

In Section 2.1 we presented results from an instrumental variables regression to estimate the elasticity of changes in retail prices to changes in house prices. In this appendix we formalize the endogeneity concern inherent in the OLS specification, and provide a more detailed, formal discussion of the exclusion restriction required to use housing supply elasticity as an instrument for house price changes.

Imagine that retail prices are affected by house prices, observable characteristics X_m, and unobservable characteristics, D_m.

$$\Delta log(RetailPrice)_m = \beta \Delta log(HousePrice)_m + \gamma X_m + \underbrace{\psi D_m + \omega_m}_{\varepsilon_m} \tag{A1}$$

Since we cannot control for D_m, it gets subsumed in the OLS error term ε_m. The OLS regression will then produce a biased estimate of the coefficient β if D_m also affects changes in house prices, that is, if the regressor is correlated with the error. For example, imagine that productivity increases in a particular neighborhood, which would lead to an increase in house prices and a decrease in retail prices. Omitting productivity from the OLS regression would therefore bias down our estimate of the true elasticity of house prices to retail prices.

The well-known idea of an instrumental variables research design is that if we can find a variable that predicts house price changes, but that is uncorrelated with D_m, we can produce unbiased estimates of β. In Section 2.1 we introduced measures of the housing supply elasticity as instruments for the change in house prices. The idea of these instruments is that during the housing boom period, house prices in less elastic areas increased by more in response to the national demand shock. During the reversal period, it was precisely those areas that experienced the biggest boom that also saw the largest bust, i.e., $cov(SupplyElasticity_m, \Delta log(HousePrice)_m) \neq 0$. This "inclusion restriction" is verified by the first-stage regression A2 (also regression 1 in the paper).

$$\Delta log(HousePrice)_m = \rho SupplyElasticity_m + \delta X_m + \epsilon_m \tag{A2}$$

The intuition for the instrument suggests that we would expect ρ to be negative when predicting price changes during the boom period, and positive when predicting price changes during the bust period. This is verified in Appendix Table A2, which shows the first-stage coefficients ρ for both instruments, as well as for both the boom and the bust period.

The identifying assumption, or the "exclusion restriction," is that the instrument has to be uncorrelated with any unobserved shocks that affect both house prices and retail prices, D_m.

$$Cov(SupplyElasticity_m, D_m) = 0 \tag{A3}$$

The exclusion restriction is inherently untestable: if we observed D_m we would control for it directly by including it in X_m, thereby avoiding the omitted variables problem. However, the fact that controlling for many observable characteristics in Tables I and III does not affect the estimated coefficient for β lends credibility to the validity of the instrument. Furthermore, in Section 2.2 we present an alternative identification strategy using the interaction of house price changes with homeownership rates. To also explain these results, the unobserved shock D_m would have to differentially affect house prices in zip codes with different homeownership rates.

B Price-Setting Behavior - High Frequency Results

In Section 2.1 we presented our baseline results using "long-difference" specifications in which we estimate the effect of changes in house prices over longer periods on changes in retail prices over the same period. We next provide more temporally disaggregated results. We document a strong relationship between house prices and retail prices at quarterly frequencies, suggesting that our results are relevant even for high-frequency business cycle analysis. In regression A4, the unit of observation is an MSA-quarter, and the key dependent variable is the log of the retail price level in that quarter.

$$log(RetailPrice)_{m,q} = \beta log(HousePrice)_{m,q} + \gamma X_{m,q} + \xi_m + \delta_q + \varepsilon_{m,q} \tag{A4}$$

Columns 1 and 2 of Appendix Table A5 show the results from this OLS regression. All specifications include quarter fixed effects, and standard errors are clustered at the MSA level to account for serial correlation in prices.[58] The estimated elasticity is 5%, which suggests that much of the long-run response of retail prices to house prices occurs at relatively high frequencies.

While our instruments for house price changes in Section 2.1 vary only in the cross-section, we also conduct an instrumental variables version of regression A4. To do this, we follow Bartik's (1991) intuition and instrument for $log(HousePrice)_{m,q}$ with the product of the MSA-level housing supply elasticity and the U.S.-wide house price level as measured by the seasonally-adjusted purchase-only OFHEO house price index. While changes in aggregate housing demand (for example due to changes in interest rates) will move U.S.-wide house prices, the local house price response will depend on the local elasticity of housing supply. The exclusion restriction requires that changes in U.S.-wide house prices interacted with local supply elasticity affect local retail prices only through their effect on local house prices. Columns 3 and 4 of Appendix Table A5 present the results from the IV regression, using the housing supply elasticity measures provided by Saiz (2010) and Gyourko, Saiz and Summers (2008), respectively. Just as in the long-difference specifications, the estimated elasticities in this IV regression are highly significant and about twice as large as in the OLS regressions.

Columns 5-8 of Appendix Table A5 show results from the quarterly zip code-level analysis in regression A5.

$$\begin{aligned} log(RetailPrice)_{z,q} =& \beta log(HousePrice)_{z,q} + \delta log(HousePrice)_{z,q} \times HomeownershipRate_z + \\ & \gamma X_{m,q} + \xi_z + \delta_q + \varepsilon_{q,z} \end{aligned} \tag{A5}$$

Columns 5 and 6 show the relationship between house prices and retail prices with and without additional control variables. As before, comparing these numbers to columns 1 and 2, we find smaller elasticities at the zip code level than at the MSA level. The main specifications of interest at the zip code level are shown in columns 7 and 8, where we include the interaction of the zip code homeownership rate with house prices. The evidence confirms that increases in house prices translate into higher retail prices primarily in zip codes with high homeownership rates.

[58] Quarter fixed effects imply that we are identifying off of cross-sectional differences across MSAs rather than movements across time, so that general increases in the price level do not contaminate our results. Using first-difference specifications requires stronger assumptions, but also delivers a significant positive relationship.

C Price Index Construction – Robustness

In Section 1.1 we provide a description of our benchmark price index construction. Here we expand on that description and discuss what features of the data can drive changes in our price index. More importantly, we discuss alternative price index construction methods, and show that our benchmark results are essentially unchanged under alternative methods. To construct our benchmark price index, we first construct a category-level price index:[59]

$$\frac{P_{l,c,t+1}}{P_{l,c,t}} = \prod_{i} \left(\frac{P_{i,l,c,t+1}}{P_{i,l,c,t}} \right)^{\omega_{i,l,c,y(t)}}.$$

We then construct an overall location-specific price index by weighting these category price indices by the revenue share of a particular category, $\omega_{l,c,y(t)} = \frac{\sum_{i \in c} TS_{i,l,c,y(t)}}{\sum_{i} TS_{i,l,y(t)}}$:[60]

$$\frac{P_{l,t+1}}{P_{l,t}} = \prod_{c} \left(\frac{P_{l,c,t+1}}{P_{l,c,t}} \right)^{\omega_{l,c,y(t)}}.$$

In this benchmark specification revenue shares are updated annually and vary across locations. We choose this specification for our benchmark because it most closely reflects the inflation rate for the products that are actually being purchased in a particular location at a specific time. Furthermore, it also follows the construction of regional CPI price indices by the BLS.

What does this specification imply for the sources of price index variation? First, permanent differences in product availability, quality or price across locations will not show up as any variation in our price indices, since all variation is driven by price relatives across time. To see this most clearly, assume that all products in city 1 are high quality, high price items, but that prices do not change across time, and that all products in city 2 are low quality, low price items, which also do not change prices across time. Since only location-specific price relatives contribute to location-specific price index changes, the price index in both cities in period 0 is normalized to 1, and the price index remains equal to 1 for all future dates. That is, permanent differences across location are essentially absorbed into a fixed effect that is differenced out of all of our empirical exercises. Similarly, product switching towards high quality, high price items also results in no change in the measured price index as long as these prices are not increasing differentially. This point is important to remember when comparing

[59] To limit the influence of outliers, we winsorize individual price relatives at ± 1 log points.

[60] We winsorize the top and bottom percentile of category price-relatives; our results are robust to other specifications.

our evidence in Section 2.5, which showed that there are important changes in shopping behavior in response to house price movements, with the evidence below, which shows that using alternative expenditure weights does not affect the relationship between house prices and retail prices.

Only two sources of variation can generate movements in retail price indices across locations. First, holding revenue weights constant, individual posted prices can increase. If ω is constant and posted prices in a location rise, then that location's relative price index will increase. This is the primary source of price variation that we are interested in. However, in our benchmark specification, prices can also change for second reason. If some items have high inflation and some items have low inflation, the relative price level in a location will rise across time if households in that location substitute more towards the high inflation goods than households in other locations. (If households in all locations substitute towards higher inflation goods, each price index will rise more but there will be no change in relative prices across locations). While we want to capture these substitution driven price index changes in our benchmark, since they will be relevant for households' cost of living as well as for understanding aggregate inflation, the two sources of variation have different interpretations in models. That is, location-specific price indices can rise either because firms increase prices or because household substitute towards items which have more rapid inflation.

To address this, we have constructed price indices under two alternative assumptions. First, we have constructed a pure fixed-basket Laspeyres Index. That is, instead of constructing price indices using $\omega_{i,l,c,y(t)}$, we instead use a consumption basket in each location which is fixed at initial-period weights: $\omega_{i,l,c,y(t)} = \omega_{i,l,c,y(0)}$. In this case, changes across time in household shopping behavior, by construction, will have no effect on price indices across time. Table A9 recomputes our baseline results for this alternative specification, and shows that our results are essentially unchanged. Thus, product-switching behavior does not mechanically drive our location-specific price effects. Prices for a fixed basket of goods are actually rising faster in the high-house-price-growth areas.

However, it could still be the case that households in high-house-price-growth locations simply happen to purchase more items that exhibit higher inflation. For example, if there are two products, one with permanently high inflation and one with permanently low inflation, it may be the case that households in the high-house-price-growth location always purchase the high-inflation item and households in the low-house-price-growth location always purchase the low inflation-item. This would show up as a change in relative prices across time in both our benchmark and in the fixed basket specification, even though household behavior and firm behavior do not change across time. To address this concern, we construct a version of the price index using common national revenue weights. That is, $\omega_{i,l,c,y(t)} = \omega_{l,c,y(t)}$, so that all locations place the same weight on each item in the price index. In this case, differences in households' shopping baskets across location are ignored when constructing price indices, so differences in these shopping baskets or in shopping behavior cannot explain our results. Table A10 recomputes our baseline results for this specification, and again shows that it does not affect our results.

In addition to these robustness checks, we have also experimented with constructing price indices at higher and lower time-frequencies, using different product mixes, excluding temporary price changes, and using alternative treatments of missing price observations which occur in weeks with no sales. None of these alternatives substantively affected our results. Thus, our broad conclusion is that while various features of weighting or measurement of price indices could potentially be important for our results, these details ultimately have little quantitative importance. Together, the various alternative price indices we have constructed strongly support our interpretation of the retail price-house price link: When house prices rise, firms actually raise prices in response.

D Business Cycle Modeling

In Section 3 of the paper we discussed a number of ways in which demand shocks can affect markups. First, if demand shocks lead to changes in marginal costs, then if retail prices are sticky and firms cannot immediately raise prices to keep their markup constant, this will lead to a decline in total markups. In our empirical setting we found no evidence for changes to marginal costs in response to changes in house prices; therefore, this channel does not seem to be at work on our setting. Second, if higher demand shocks led to a decline in the search effort expended by households, then the demand elasticity faced by firms would decline. This effective increase in market power leads to higher desired markups. Our estimated response of retail prices to house prices confounds two effects. In particular, in the presence of sticky prices, changes in the desired "flexible price" markups cannot be immediately realized, because not all firms can immediately increase their prices to the new, desired level. In this appendix we argue that the price response we document therefore represents a lower bound on the response of flexible price desired markups.

To highlight the different sources of variation, let us decompose the actual markup into those markups set by flexible-price firms and those set by firms subject to some pricing frictions: $\mu_t = \overline{\mu} + f\mu_t^{flex} + (1-f)\mu_t^{sticky}$. Fraction f of firms set prices fully flexibly while the remaining firms are subject to some pricing frictions. The first term in the sum, $\overline{\mu} = \frac{\overline{\theta}}{\overline{\theta}-1}$, is the steady-state markup. Let $\mu_t^{flex} = \frac{\theta_t}{\theta_t-1} - \frac{\overline{\theta}}{\overline{\theta}-1}$ be the flexible price deviation in the markup from steady-state. If the elasticity of substitution, θ, is constant, then the contribution of μ_t^{flex} to total markups will be zero; with flexible prices, deviations from steady-state markup occur when the elasticity of substitution changes.[61] Finally, let $\mu_t^{sticky} = \frac{P_t^{sticky}}{\Psi_t} - \frac{\overline{\theta}}{\overline{\theta}-1}$ be the contribution of sticky prices to the total markup. The average price chosen by firms subject to pricing frictions, P_t^{sticky}, will in turn be a mix of prices that are currently fixed and prices that reset in the current period. In the presence of pricing frictions, these reset prices will be increasing in expected marginal cost and in expected flexible price desired markups. If Ψ_t does not respond to local increases in demand, then μ_t^{sticky} will only rise if there is an increase in flexible price markups. Thus, if marginal cost is constant, our empirical evidence can only be rationalized through an increase in μ_t^{flex}.

Using this notation, we can show that the price response we document represents a lower bound on the response of flexible price markups. To see this, we can look at the response of the price level to a local change in demand D_l in a standard New Keynesian setup. Let f be the fraction of firms with flexible prices in the economy. Assume that the remaining firms are Calvo price setters with probability of adjustment $(1-\alpha)$ and choose price P^* when adjusting. Then

$$\begin{aligned}\frac{\partial \log P}{\partial \log D_l} &= f\frac{\partial \log P^{flex}}{\partial \log D_l} + (1-f)(1-\alpha)\frac{\partial \log P^*}{\partial \log D_l} \\ &= f\frac{\partial \log\left[\mu^{flex}\Psi\right]}{\partial \log D_l} + (1-f)(1-\alpha)\sum_{t=0}^{\infty}\phi_t \frac{\partial E\log\left[\mu_t^{flex}\Psi_t\right]}{\partial \log D_l},\end{aligned}$$

where ϕ_t is a standard kernel that weights future marginal costs according to firms' discount rates together with the probability of future price adjustment. $\partial E\left[\mu_t^{flex}\Psi_t\right]$ is the expected response of flex price markups and marginal cost to the demand shock for today and all future periods. Now if goods are not produced locally, an increase in local demand should have no effect on marginal cost: $\frac{\partial \Psi_t}{\partial D_l} = 0$ $\forall t$ and we get

$$\frac{\partial \log P}{\partial \log D_l} = f\frac{\partial \log \mu^{flex}}{\partial \log D_l} + (1-f)(1-\alpha)\sum_{t=0}^{\infty}\phi_t \frac{\partial E\log \mu_t^{flex}}{\partial \log D_l}.$$

[61] Note that variation in flexible price markups can occur through various other structural channels that map into this parameter. In addition to time-variation in the elasticity of demand, variation in the importance of fixed costs or other factors affecting competitive structure can affect flexible price markups.

Finally, note that $\sum_{t=0}^{\infty}\phi_t \frac{\partial E \log \mu_t^{flex}}{\partial \log D_l} \leq \frac{\partial \log \mu^{flex}}{\partial \log D_l}$, with equality holding only when the effect of the demand shock on flex price markups is permanent. This then implies that

$$\frac{\partial \log \mu^{flex}}{\partial \log D_l} \geq \frac{\frac{\partial \log P}{\partial \log D_l}}{f + (1-f)(1-\alpha)}.$$

This simple inequality provides a back-of-the-envelope way to convert the observed response of prices to local demand shocks into implied changes in flexible price markups. For example, assume that the demand shock is permanent, that 10% of grocery store prices are fully flexible, and that the quarterly frequency of adjustment is roughly 33% for the remaining items. This implies that

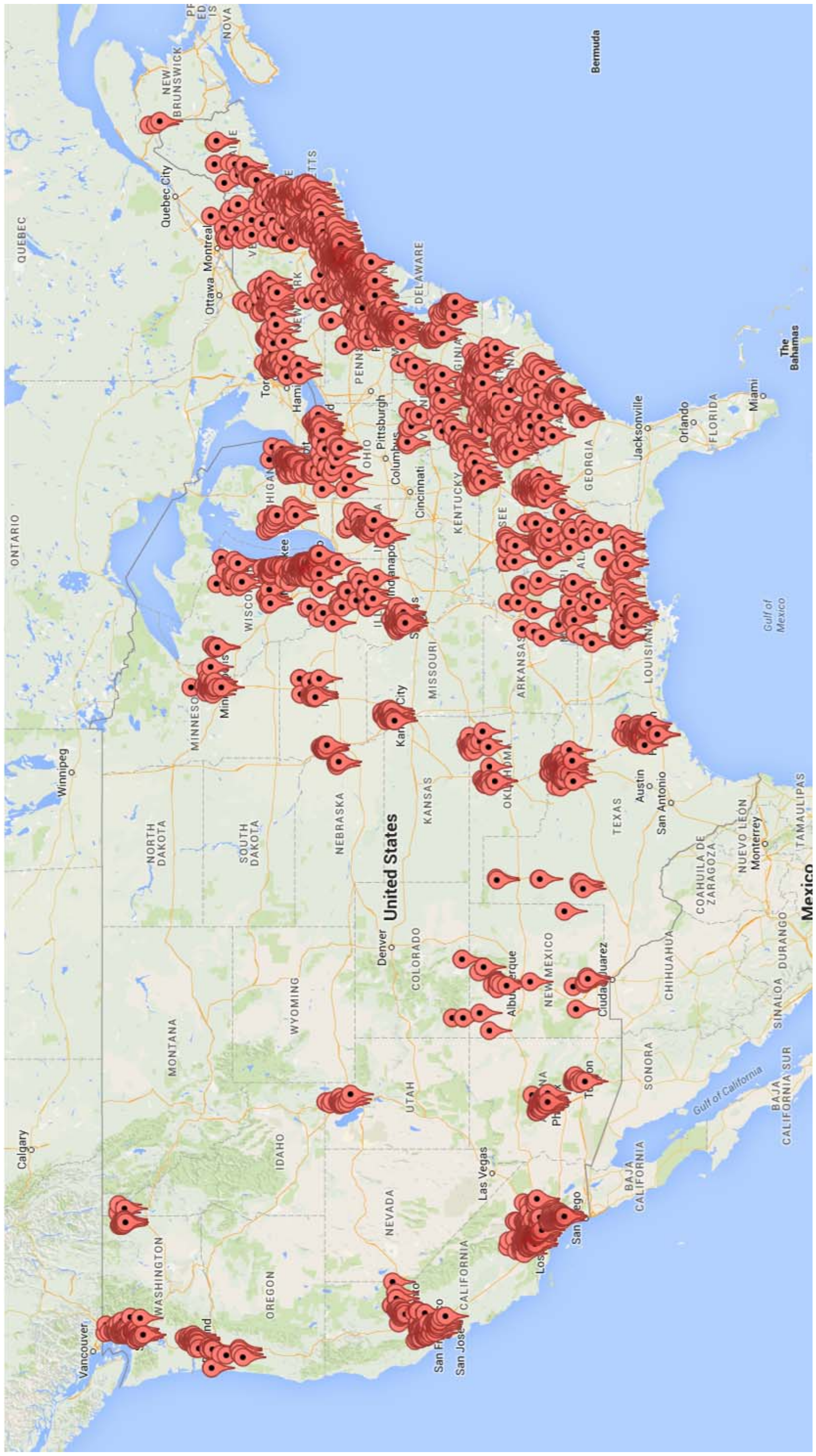
$$\begin{aligned}
\frac{\partial \log \mu^{flex}}{\partial \log D_l} &= \frac{\frac{\partial P}{\partial \log D_l}}{[0.1 + 0.9(0.33)]} \\
&\simeq 2.5 \frac{\partial \log P}{\partial \log D_l}.
\end{aligned}$$

In this scenario, the 15% elasticity of retail prices to house prices that we observe implies almost a 40% elasticity of flex-price markups. If these local demand shocks are less than permanent, then this multiplier would become even larger, since firms hit by the Calvo fairy today would optimally respond less strongly to a temporary change in desired markups so that the same observed price response requires a larger underlying change in flex-price markups today.

While we previously argued that assuming a constant marginal cost is sensible in our empirical context, the above formula can also be used to assess the plausibility of marginal cost movements for explaining our empirical results. If there was no change in μ^{flex}, and instead all results were driven by variation in marginal cost, then we would need an elasticity of marginal cost of 40% in response to housing wealth shocks. If 90% of the marginal cost is cost of goods sold, which if anything have a mild negative demand elasticity due to volume contracts with wholesalers, this means that an elasticity of local wages or other components of marginal cost of more than 400% would be required to explain our price responses. This is an implausibly large elasticity, especially since there is no relationship between average local wage growth and local housing wealth shocks.

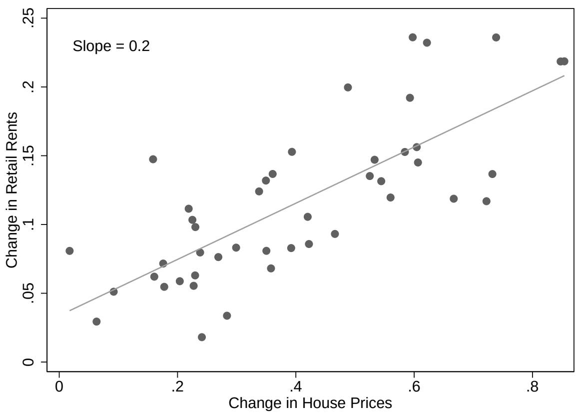
Appendix Figures

Figure A1: Location of Retail Stores in IRI Sample

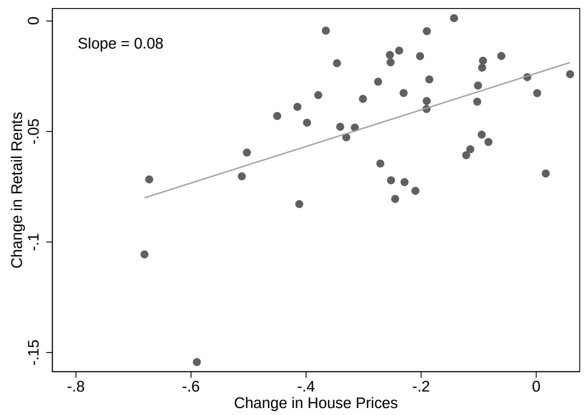


Note: Figure shows the location of the zip codes in which we observe stores in the IRI sample.

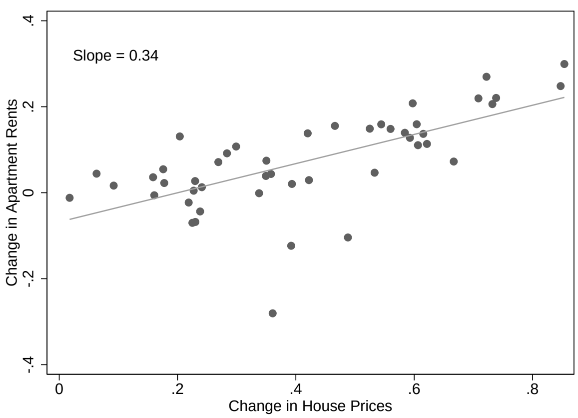
Figure A2: Changes in Apartment and Retail Rents vs. Changes in House Prices



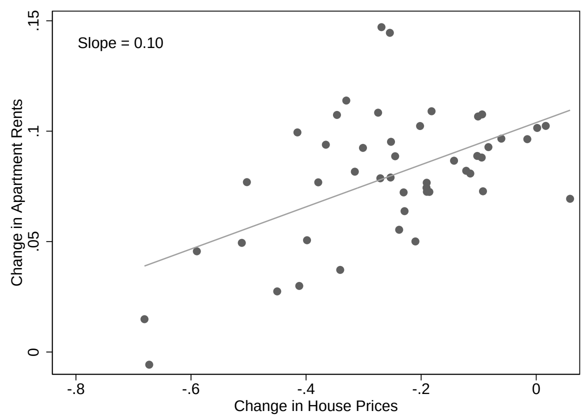
(A) Retail Rents: 2001-2006



(B) Retail Rents: 2007-2011



(C) Apartment Rents: 2001-2006



(D) Apartment Rents: 2007-2011

Note: Figure shows changes in house prices and changes in retail rents (Panels A and B) and apartment rents (Panels C and D) for the periods periods 2001-2006 (Panels A and C) and 2007-2011 (Panels B and D).

Appendix Tables

Table A1: Summary Statistics, MSA Level "Long Differences"

Panel A: Time Period: 2001 - 2006

	Mean	St. Dev.	P25	P50	P75	N
Δ Retail Prices (% as decimal)	0.080	0.045	0.052	0.079	0.111	125
Δ House Prices (% as decimal)	0.366	0.186	0.227	0.349	0.514	125
Δ Unemployment Rate (% as decimal)	0.138	0.216	-0.007	0.143	0.310	125
Δ Wage (% as decimal)	0.231	0.071	0.195	0.218	0.256	125
Δ Share Grocery Retail Employment (absolute)	-0.005	0.015	-0.011	-0.004	0.002	125
Δ Share Nontradable Employment (absolute)	-0.008	0.030	-0.027	-0.007	0.013	125
Δ Share Construction Employment (absolute)	0.092	0.037	0.066	0.086	0.113	125
Δ Retail Rent (% as decimal)	0.116	0.057	0.076	0.111	0.147	45
Δ Retail Establishments per 1000 people	0.081	1.086	-0.074	-0.031	0.014	123
Δ Share population with at least highschool (absolute)	0.032	0.017	0.019	0.030	0.041	125
Δ Share population with at least bachelor (absolute)	0.025	0.015	0.016	0.025	0.034	125

Panel B: Time Period: 2007 - 2011

	Mean	St. Dev.	P25	P50	P75	N
Δ Retail Prices (% as decimal)	0.137	0.030	0.116	0.137	0.160	126
Δ House Prices (% as decimal)	-0.202	0.150	-0.274	-0.190	-0.094	126
Δ Unemployment (% as decimal)	0.507	0.216	0.377	0.520	0.658	126
Δ Wage (% as decimal)	0.111	0.057	0.090	0.114	0.138	126
Δ Share Grocery Retail Employment (absolute)	0.003	0.011	-0.001	0.002	0.006	126
Δ Share Nontradable Employment (absolute)	0.012	0.023	0	0.011	0.024	126
Δ Share Construction Employment (absolute)	-0.029	0.024	-0.044	-0.025	-0.014	126
Δ Retail Rent (% as decimal)	-0.045	0.029	-0.061	-0.039	-0.024	45
Δ Retail Establishments per 1000 people	-0.039	0.052	-0.065	-0.035	-0.013	124
Δ Share population with at least highschool (absolute)	0.033	0.018	0.019	0.030	0.041	126
Δ Share population with at least bachelor (absolute)	0.025	0.015	0.016	0.025	0.034	126

Note: Table shows summary statistics for the key dependent and independent variables in regression 2 over the periods 2001-2006 (Panel A) and 2007-2011 (Panel B).

Table A2: Instrumental Variables Regression – First Stage

	Time Period: 2001-2006				Time Period: 2007-2011			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Saiz Elasticity Measure	-0.099***	-0.088***			0.055***	0.048***		
	(0.015)	(0.016)			(0.012)	(0.012)		
Wharton Regulation Index			0.124***	0.126***			-0.071***	-0.088***
			(0.017)	(0.016)			(0.017)	(0.015)
Controls		✓		✓		✓		✓
R-squared	0.284	0.315	0.252	0.357	0.130	0.260	0.120	0.334
N	112	112	112	112	112	112	112	112

Note: Table shows results from the first-stage instrumental variable regression 1. The unit of observation is an MSA, the dependent variable is house price growth over 2001-2006 in columns 1 - 4, and house price growth over 2007-2011 in columns 5 - 8. In even columns we also control for the same control variables as in columns 4 - 6 of Table I. For the Saiz Elasticity Measure, higher values signal an MSA with more elastic housing supply. For the Wharton Regulation Index, lower values signal an MSA with more elastic housing supply. Robust standard errors are presented in parantheses. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table A3: ENTRY – Δ GROCERY RETAIL ESTABLISHMENTS PER 1000 PEOPLE

	PANEL A: TIME PERIOD: 2001-2006				
	(1)	(2)	(3)	(4)	(5)
Δ House Prices	0.070*			0.152**	0.059
	(0.037)			(0.069)	(0.052)
Saiz Elasticity		-0.014**		0.008	
		(0.006)		(0.013)	
Wharton Regulation			0.017		0.018
			(0.011)		(0.026)
Δ House Prices × Saiz Elasticity				-0.058	
				(0.040)	
Δ House Prices × Wharton Regulation					-0.020
					(0.054)
N	121	109	109	109	109

	PANEL B: TIME PERIOD: 2007 - 2011				
	(1)	(2)	(3)	(4)	(5)
Δ House Prices	-0.027			-0.004	-0.023
	(0.027)			(0.089)	(0.046)
Saiz Elasticity		0.005		0.004	
		(0.005)		(0.011)	
Wharton Regulation			0.002		0.004
			(0.007)		(0.016)
Δ House Prices × Saiz Elasticity				-0.019	
				(0.043)	
Δ House Prices × Wharton Regulation					0.019
					(0.071)
N	124	111	111	111	111

Note: Table shows results from an OLS regression, where the dependent variable is change in the number of retail establishments per 1,000 inhabitants over the periods 2001-2006 (Panel A) and 2007-2011 (Panel B). The unit of observation is an MSA. Robust standard errors are presented in parantheses. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table A4: Instrumental Variables Analysis - Robustness Checks

	Dependent Variable: Δ Retail Prices					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Instrument with Saiz Supply Elasticity; 2001 - 2006						
Δ House Prices	0.164**	0.171***	0.174***	0.188***	0.169***	0.195***
	(0.083)	(0.066)	(0.057)	(0.064)	(0.064)	(0.069)
Panel B: Instrument with Saiz Supply Elasticity; 2007 - 2011						
Δ House Prices	0.160***	0.142*	0.101**	0.145***	0.159***	0.139*
	(0.053)	(0.077)	(0.051)	(0.053)	(0.054)	(0.074)
Panel C: Instrument with Wharton Regulation Index; 2001 - 2006						
Δ House Prices	0.261***	0.274***	0.223***	0.235***	0.266***	0.274***
	(0.066)	(0.063)	(0.045)	(0.052)	(0.063)	(0.057)
Panel D: Instrument with Wharton Regulation Index; 2007 - 2011						
Δ House Prices	0.167***	0.196**	0.147***	0.161***	0.185**	0.181**
	(0.045)	(0.091)	(0.043)	(0.041)	(0.072)	(0.071)
Controls	✓	✓	✓	✓	✓	✓
Robustness	Coast Dummy	4 Census Region Fixed Effects	9 Census Division Fixed Effects	Exclude sales	Exclude outliers in house price changes	Drop bubble states (CA, AZ, FL)

Note: Table shows results from regression 2. The unit of observation is an MSA, the dependent variable is the change in retail prices in 2001-2006 (Panels A and C) and 2007-2011 (Panels B and D). In Panels A and B we instrument for the change in house prices using the housing supply elasticity measure provided by Saiz (2010); in Panels C and D we instrument for house price changes using the Wharton Regulation Index described in Gyourko, Saiz and Summers (2008). All specifications control for changes in the unemployment rate, changes in wages, and changes in the employment share in the construction, non-tradable, and grocery retail sector. Column 1 includes a coast dummy. Column 2 includes fixed effects for four census regions. Column 3 includes fixed effects for nine census divisions. Column 4 excludes sales prices in the construction of the retail price index. Column 5 excludes those MSAs with the 5% largest and smallest house price changes over the period. Column 6 excludes observations from the "bubble states" Arizona, California and Florida. Robust standard errors in parenthesis. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table A5: Quarter-by-Quarter Analysis

	MSA Level				Zip Code Level			
	OLS		IV (Saiz)	IV (Wharton)	OLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(House Prices)	0.047***	0.054***	0.114***	0.155***	0.015***	0.017**	-0.019*	-0.018
	(0.011)	(0.011)	(0.029)	(0.034)	(0.005)	(0.005)	(0.011)	(0.011)
Unemployment Rate		0.016**	0.027***	0.035***		0.005		0.004
		(0.007)	(0.010)	(0.010)		(0.004)		(0.004)
Average Weekly Wage		-0.004	-0.011	-0.022		0.004		0.003
		(0.024)	(0.026)	(0.026)		(0.008)		(0.008)
Share Grocery Retail Employment		-0.073*	-0.111**	-0.133**		0.003		-0.002
		(0.040)	(0.051)	(0.054)		(0.089)		(0.089)
Share Nontradable Employment		-0.156**	-0.172***	-0.170**		0.008		0.015
		(0.061)	(0.064)	(0.067)		(0.054)		(0.054)
Share Construction Employment		0.175	0.175*	0.147		-0.019		-0.032
		(0.108)	(0.104)	(0.106)		(0.030)		(0.031)
log(House Prices)							0.052***	0.053***
× Homeownership Rate							(0.017)	(0.017)
Fixed Effects	Q, MSA	Q, MSA	Q, MSA	Q, MSA	Q, Zip	Q, Zip	Q, Zip	Q, Zip
N	5,546	5,546	4,959	4,959	43,914	43,914	43,914	43,914

Note: Table shows results from regression A4. The unit of observation is an MSA-quarter in columns 1 - 4, and a zip code-quarter in columns 5 - 8. The dependent variable is the log of retail prices. Columns 3 and 4 present results from an instrumental variables regression; we instrument for log(House Prices) with the interaction of the MSA-specific housing supply elasticity measures provided by Saiz (2010) and Gyourko, Saiz and Summers (2008), respectively, with the seasonally-adjusted OFHEO national house price index. Standard errors are clustered at the MSA level in columns 1 - 4, and the zip code level in columns 5 - 8. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table A6: Effect of House Prices on Shopping Behavior - Zip Code House Prices, Zip Code Homeownership Rates

DEPENDENT VARIABLE:	LOG(EXPENDITURE)		SHARE "DEAL"		SHARE GENERIC		SHARE COUPON	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(House Price)	-0.006	-0.018	0.008	0.016**	-0.004	-0.001	0.005	0.001
	(0.019)	(0.020)	(0.006)	(0.007)	(0.003)	(0.004)	(0.003)	(0.004)
Homeownership Rate	-0.182	-0.226	0.098**	0.111**	0.011	0.020	0.060**	0.048*
	(0.135)	(0.139)	(0.045)	(0.046)	(0.024)	(0.025)	(0.024)	(0.025)
log(House Price)	0.062**	0.074***	-0.021**	-0.023**	-0.004	-0.006	-0.012***	-0.009*
× Homeownership Rate	(0.027)	(0.027)	(0.009)	(0.009)	(0.005)	(0.005)	(0.005)	(0.005)
Unemployment Rate		-0.008		0.128***		0.034**		-0.046***
		(0.080)		(0.025)		(0.015)		(0.014)
Average Weekly Wage		0.021*		0.002		-0.003		0
		(0.013)		(0.004)		(0.002)		(0.002)
Share Grocery Retail		-0.245***		0.070**		0.020		0.029*
Employment		(0.091)		(0.029)		(0.018)		(0.017)
Share Nontradable		0.138***		-0.018		-0.008		0.007
Employment		(0.047)		(0.015)		(0.009)		(0.008)
Share Construction		0.129***		-0.053***		0.004		-0.025***
Employment		(0.049)		(0.015)		(0.009)		(0.008)
Quarter Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Household Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
R-squared	0.716	0.716	0.866	0.866	0.728	0.730	0.761	0.761
$\bar{y}$	6.678	6.681	0.283	0.283	0.174	0.174	0.079	0.079
N	955,251	913,926	955,251	913,926	955,251	913,926	955,251	913,926

Note: Table shows results from regression 4. The unit of observation is a household-quarter, the sample is 2004 to 2011. The dependent variables are the log of total household expenditure (columns 1 and 2), the expenditure share of products that are on sale (columns 3 and 4), the expenditure share of generic products (columns 5 and 6), and the expenditure share of products purchased with a coupon (columns 7 and 8). House prices are measured at the zip code level. All specifications include household and quarter fixed effects. In columns 2, 4, 6 and 8 we also include additional control variables at the zip code × quarter level. Instead of the household's predicted homeownership rate, as in Table VI, we include the zip code level homeownership rate in this Table. Standard errors are clustered at the zip code × quarter level. Each observation is weighted by the household sampling weight. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table A7: Shopping Behavior - MSA House Prices

Dependent Variable:	log(Expenditure)		Share "Deal"		Share Generic		Share Coupon	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(House Price)	-0.029*		0.013**		-0.001		0.004	
	(0.017)		(0.006)		(0.003)		(0.003)	
$\mathbb{1}_{Homeowner}$	-0.252***	-0.189***	0.131***	0.087***	0.033**	0.004	0.087***	0.085***
	(0.079)	(0.046)	(0.028)	(0.016)	(0.014)	(0.009)	(0.014)	(0.009)
log(House Price)	0.058***	0.046***	-0.025***	-0.016***	-0.006**	-0.001	-0.016***	-0.016***
$\times \mathbb{1}_{Homeowner}$	(0.016)	(0.009)	(0.006)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
Unemployment Rate	0.057		0.104***		0.022		-0.047***	
	(0.086)		(0.027)		(0.016)		(0.015)	
Average Weekly Wage	0.008		0.004		-0.000		0.002	
	(0.014)		(0.004)		(0.002)		(0.002)	
Share Grocery Retail	-0.309***		0.084***		0.015		0.041**	
Employment	(0.097)		(0.031)		(0.019)		(0.018)	
Share Nontradable	0.117**		-0.026		-0.008		0.005	
Employment	(0.050)		(0.016)		(0.009)		(0.009)	
Share Construction	0.182***		-0.040**		0.009		-0.015	
Employment	(0.052)		(0.017)		(0.009)		(0.009)	
Household Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Quarter Fixed Effects	✓	·	✓	·	✓	·	✓	·
Quarter $\times$ MSA Fixed Effects	·	✓	·	✓	·	✓	·	✓
R-squared	0.716	0.736	0.868	0.877	0.732	0.750	0.765	0.778
$\bar{y}$	6.699	6.715	0.281	0.291	0.175	0.180	0.079	0.084
N	811,038	849,103	811,038	849,103	811,038	849,103	811,038	849,103

Note: Table shows results from regression 4. The unit of observation is a household-quarter, the sample is 2004 to 2011. The dependent variables are the log of total household expenditure (columns 1 and 2), the expenditure share of products that are on sale (columns 3 and 4), the expenditure share of generic products (columns 5 and 6), and the expenditure share of products purchased with a coupon (columns 7 and 8). House prices are measured at the MSA level. All specifications include household fixed effects. In odd columns we include quarter fixed effects, in even columns we include quarter $\times$ MSA fixed effects. Each observation is weighted by the household sampling weight. Standard errors are clustered at the MSA $\times$ quarter level. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table A8: Homescan Results - MSA House Prices, Zip Code Homeownership Rates

DEPENDENT VARIABLE:	LOG(EXPENDITURE)		SHARE "DEAL"		SHARE GENERIC		SHARE COUPON	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(House Price)	-0.041*		0.013*		-0.002		-0.005	
	(0.022)		(0.007)		(0.004)		(0.004)	
Homeownership Rate	-0.414***	-0.454***	0.141***	0.054*	0.027	-0.033	0.094***	0.074***
	(0.148)	(0.091)	(0.049)	(0.031)	(0.026)	(0.028)	(0.027)	(0.018)
log(House Price) × Homeownership Rate	0.111***	0.114***	-0.029***	-0.015**	-0.007	0.005	-0.019***	-0.017***
	(0.029)	(0.018)	(0.010)	(0.006)	(0.005)	(0.004)	(0.005)	(0.004)
Unemployment Rate	-0.016		0.091***		0.031**		-0.056***	
	(0.081)		(0.025)		(0.015)		(0.015)	
Average Weekly Wage	0.022*		0.001		-0.002		0.002	
	(0.013)		(0.004)		(0.002)		(0.002)	
Share Grocery Retail Employment	-0.255***		0.081***		0.026		0.038**	
	(0.090)		(0.029)		(0.018)		(0.019)	
Share Nontradable Employment	0.136***		-0.021		-0.010		0.009	
	(0.047)		(0.015)		(0.009)		(0.009)	
Share Construction Employment	0.130***		-0.036**		0.009		-0.011	
	(0.049)		(0.015)		(0.009)		(0.009)	
Household Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Quarter Fixed Effects	✓	·	✓	·	✓	·	✓	·
Quarter × MSA Fixed Effects	·	✓	·	✓	·	✓	·	✓
R-squared	0.716	0.732	0.867	0.889	0.730	0.747	0.766	0.773
$\bar{y}$	6.680	6.694	0.283	0.292	0.174	0.179	0.079	0.084
N	924,068	966,605	924,068	966,605	924,068	832,386	794,909	832,386

Note: Table shows results from regression 4. The unit of observation is a household-quarter, the sample is 2004 to 2011. The dependent variables are the log of total household expenditure (columns 1 and 2), the expenditure share of products that are on sale (columns 3 and 4), the expenditure share of generic products (columns 5 and 6), and the expenditure share of products purchased with a coupon (columns 7 and 8). House prices are measured at the MSA level. All specifications include household fixed effects. In odd columns we include quarter fixed effects, in even columns we include quarter × MSA fixed effects. Each observation is weighted by the household sampling weight. Instead of the household's predicted homeownership rate, as in Table VI, we include the zip code-level homeownership rate in this Table. Standard errors are clustered at the zip code × quarter level. Each observation is weighted by the household sampling weight. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).

Table A9: Retail Prices vs. House Prices – Fixed Weights Across Time

	Panel A: Time Period: 2001 - 2006 Dependent Variable: Δ Retail Prices					
	OLS (1)	IV Saiz (2)	IV Wharton (3)	OLS (4)	IV Saiz (5)	IV Wharton (6)
Δ House Prices	0.056*** (0.021)	0.144*** (0.042)	0.239*** (0.052)	0.066*** (0.024)	0.167*** (0.057)	0.239*** (0.051)
Δ Share Grocery Retail Employment				-0.140 (0.365)	0.052 (0.384)	0.132 (0.398)
Δ Share Nontradable Employment				0.092 (0.185)	-0.047 (0.179)	-0.091 (0.179)
Δ Share Construction Employment				-0.103 (0.109)	-0.017 (0.129)	0.007 (0.145)
Δ Unemployment				0.041** (0.020)	0.080*** (0.031)	0.102*** (0.029)
Δ Wage				0.062 (0.059)	0.049 (0.063)	0.010 (0.065)
Number of Observations	125	112	112	125	112	112

	Panel B: Time Period: 2007 - 2011 Dependent Variable: Δ Retail Prices					
	OLS (1)	IV Saiz (2)	IV Wharton (3)	OLS (4)	IV Saiz (5)	IV Wharton (6)
Δ House Prices	0.079*** (0.014)	0.105** (0.042)	0.115*** (0.045)	0.081*** (0.017)	0.132*** (0.050)	0.136*** (0.039)
Δ Share Grocery Retail Employment				-0.052 (0.265)	0.105 (0.257)	0.102 (0.261)
Δ Share Nontradable Employment				0.046 (0.154)	-0.109 (0.158)	-0.110 (0.158)
Δ Share Construction Employment				-0.012 (0.131)	-0.105 (0.142)	-0.111 (0.142)
Δ Unemployment				-0.003 (0.013)	0.013 (0.015)	0.014 (0.013)
Δ Wage				-0.036 (0.046)	-0.067 (0.048)	-0.068 (0.048)
Number of observations	126	112	112	126	112	112

Note: Table shows results from the following OLS regression: $\Delta log(RetailPrice)_m = \beta \Delta log(HousePrice)_m + \gamma X_m + \varepsilon_z$ in columns 1 and 4, and from instrumental variables regression 2 in the other columns. The retail price index is constructed using regional expenditure weights that are fixed over time. The unit of observation is an MSA, the dependent variable is the change in retail prices in 2001-2006 (Panel A) and 2007-2011 (Panel B). We instrument for the change in house prices using measures of the housing supply elasticity provided by Saiz (2010) in columns 2 and 5, and the Wharton Regulation Index described in Gyourko, Saiz and Summers (2008) in columns 3 and 6. Robust standard errors in parenthesis. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).

Table A10: Retail Prices vs. House Prices – Fixed Weights Across Space

	Panel A: Time Period: 2001 - 2006 Dependent Variable: Δ Retail Prices					
	OLS (1)	IV Saiz (2)	IV Wharton (3)	OLS (4)	IV Saiz (5)	IV Wharton (6)
Δ House Prices	0.056^{**}	0.134^{***}	0.227^{***}	0.068^{***}	0.160^{**}	0.234^{***}
	(0.022)	(0.049)	(0.051)	(0.024)	(0.065)	(0.049)
Δ Share Grocery Retail Employment				-0.142	0.043	0.125
				(0.372)	(0.381)	(0.395)
Δ Share Nontradable Employment				0.154	-0.001	-0.047
				(0.197)	(0.181)	(0.184)
Δ Share Construction Employment				-0.076	0.003	0.028
				(0.101)	(0.120)	(0.134)
Δ Unemployment				0.037^{*}	0.072^{**}	0.095^{***}
				(0.019)	(0.030)	(0.027)
Δ Wage				0.017	0.012	-0.028
				(0.058)	(0.062)	(0.060)
Number of Observations	125	112	112	125	112	112

	Panel B: Time Period: 2007 - 2011 Dependent Variable: Δ Retail Prices					
	OLS (1)	IV Saiz (2)	IV Wharton (3)	OLS (4)	IV Saiz (5)	IV Wharton (6)
Δ House Prices	0.072^{***}	0.078^{*}	0.133^{***}	0.078^{***}	0.091^{*}	0.141^{***}
	(0.014)	(0.040)	(0.046)	(0.017)	(0.048)	(0.042)
Δ Share Grocery Retail Employment				-0.294	-0.046	-0.081
				(0.295)	(0.273)	(0.280)
Δ Share Nontradable Employment				0.129	-0.056	-0.068
				(0.183)	(0.165)	(0.169)
Δ Share Construction Employment				0.028	0.031	-0.044
				(0.120)	(0.132)	(0.136)
Δ Unemployment				0.004	0.013	0.024^{*}
				(0.012)	(0.015)	(0.014)
Δ Wage				-0.039	-0.042	-0.057
				(0.044)	(0.045)	(0.046)
Number of observations	126	112	112	126	112	112

Note: Table shows results from the following OLS regression: $\Delta log(RetailPrice)_m = \beta \Delta log(HousePrice)_m + \gamma X_m + \varepsilon_z$ in columns 1 and 4, and from instrumental variables regression 2 in the other columns. The retail price index is constructed using fixed national expenditure weights. The unit of observation is an MSA, the dependent variable is the change in retail prices in 2001-2006 (Panel A) and 2007-2011 (Panel B). We instrument for the change in house prices using measures of the housing supply elasticity provided by Saiz (2010) in columns 2 and 5, and the Wharton Regulation Index described in Gyourko, Saiz and Summers (2008) in columns 3 and 6. Robust standard errors in parenthesis. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).