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 prices to changes in house prices, with elasticities of 15%-20% across housing booms and busts. We provide evidence that our results are driven by changes in markups rather than by changes in local costs. We argue that this markup variation arises when increases in housing wealth reduce households' demand elasticity, and firms raise markups in response. Consistent with this channel, price effects are larger in zip codes with many homeowners, and non-existent in zip codes with mostly renters. 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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1 Introduction

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 (see Nekarda and Ramey, 2013, for a review). However, this approach requires strong assumptions, both to identify aggregate demand shocks and to measure marginal cost and markups; it also makes it hard to isolate the channel that explains any observed relationships.

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 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 approximately 15%-20%. This elasticity is both highly significant and economically large: for example, during the 2001-2006 housing boom, moving from the 10th percentile to the 90th percentile of MSA house price growth implies an increase in relative retail prices of 7-10%. This compares to an overall 90-10 difference in retail prices of 12%, and suggests that the local demand shocks we identify can account for a significant fraction of the overall regional variation in retail price changes 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

[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 increases with house prices and can remain agnostic about the channel that drives this relationship.

[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.

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 large increases in retail prices, while in zip codes with the lowest homeownership rates, house price increases actually lead to declines in retail prices (although these declines are not always statistically significant).[3]

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, and so their wholesale cost is 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 is driving the relationship between local house prices and retail prices.

Second, we directly consider two cost channels that might nevertheless explain our results: local wages might rise in response to increased local demand, or local retail rents may increase. Since wages are a small fraction of overall marginal cost for the stores in our data, explaining our result through a wage channel would require extremely large responses of local wages to local demand. Consistent with this, we find that controlling for local wages does not change our estimates. We also match our data with information on local retail rents, and find that they have no effect on our estimates.

Third, our identification strategies make supply shocks an unlikely explanation. Our first empirical estimates instrument for changes in house prices using measures of the elasticity of housing supply; it is unclear why supply-side shocks should be particularly strong in regions with lower housing supply elasticity.[4] More importantly, the estimated response of retail prices to house prices is also significantly larger in zip codes with higher homeownership rates. The local homeownership rate should be irrelevant for this response if retail price movements just reflect the pass-through of higher local costs. In contrast, higher house prices induce larger demand shocks in areas with a high concentration of homeowners, so there is a natural interaction through this channel.

[3] These negative effects in locations with a large number of renters are predicted in our framework if house prices are capitalized into local rents or some renters plan to purchase in the future.

[4] We provide detailed supporting evidence on this point in our empirical results.

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, purchase fewer goods with a coupon, and reduce the fraction of spending on generics and on items that are on sale. 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.[5]

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 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 cost, 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 imply that even with no pricing frictions, markups 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 modeled as

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

"structural", exogenous shocks, which are policy invariant. In contrast, our evidence suggests that these desired markups will respond endogenously to changes in monetary policy.

Indeed, recent work by Huo and Ríos-Rull (2013) and Kaplan and Menzio (2013) shows 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. While contemporaneous work finds support for cyclical changes in household shopping behavior (e.g., Nevo and Wong, 2014), we believe we are the first paper to document an interaction between household and firm behavior over the business cycle. We show that cyclical changes in household shopping behavior strongly affect firms' pricing decisions in equilibrium, so that the data indeed provide support for some of the interactions proposed in these theories. This 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 which vary with household wealth in the cross-section, and Manova and Zhang (2012) provide evidence 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].

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.[7] These papers identify movements in the overall markup and often interpret their results as evidence in favor 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 empirical evidence also relates directly to the large literature studying housing wealth effects. 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.,

[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

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

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 very important direction: we are able to decompose 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 relevant 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 aggregate real effects. Without price data, they explore various scenarios but must make strong assumptions about counterfactuals in order to make any concrete predictions.

Beyond these macro implications, our results also have a variety of implications for labor and urban economics. We leave a detailed discussion to the body of the paper but note here that the response of local retail prices to local house prices is a key parameter for understanding insurance against local shocks as well as spatial sorting patterns.

Related Literature: To our knowledge, Coibion, Gorodnichenko and Hong (2014) are the first researchers to look at geographic variation in price-setting. They use the same scanner data as we do to find that prices do not respond to local unemployment rates. Beraja, Hurst and Ospina (2014) use a broader set of scanner data that is only available beginning in 2006, and find 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. In addition, even large increases in unemployment affect only a small fraction of the population directly, while house price changes impact many more households. Hence, variation in house prices provides greater econometric power to identify demand shocks. If unemployment variation is not large enough to have much effect on firms' desired markups, this might explain the previous conflicting findings.[8] We also use more disaggregated geographic data, and our second identification strategy relies crucially on this sub-metro area variation. Finally, by jointly analyzing household shopping behavior and firm price setting across a large number of markets, we are able to identify the channel that explains the relationship between house prices and retail prices.

Several other papers study the implications of changes in demand using alternative identification

[8]The conflicting findings could also reflect the presence of time-varying confounding shocks, since supply and demand shocks will have opposite implications for the correlation between retail prices and unemployment.

procedures.[9] Warner and Barsky (1995) and Chevalier, Kashyap and Rossi (2003) document the response of retail prices to predictable seasonal changes in demand that are unrelated to households' wealth. Consistent with our findings, these papers show that markups are lower during times of the year when households purchase larger quantities of a given good and therefore shop more intensely for lower prices. Gicheva, Hastings and Villas-Boas (2010) show that households' grocery store spending switches towards sale-items when gas prices rise. Gagnon and Lopez-Salido (2014) find no retail price response to supermarket strikes, weather shocks, or migration driven by hurricane Katrina. However, their demand shocks are unlikely to change the demand elasticity faced by a particular store or the store's marginal cost; it is then not surprising that there is no effect on prices.

An important implication is that not all "micro" demand shocks are the same, and if the goal is to inform business cycle models, one should look for demand shocks that mimic the effects of the business cycle. There is evidence that the effects of the demand shocks we identify are comparable to aggregate business cycle shocks on important dimensions such as their impact on household shopping behavior. For example, Aguiar, Hurst and Karabarbounis (2013) show that there is an increase in time-use 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). We find similar large changes in shopping behavior in response to house-price-induced demand shocks.

The rest of the paper proceeds as follows: Section 2 describes our data. Section 3 describes the price-setting and shopping behavior results. Section 4 discusses implications of our findings for business cycle models and for interpreting the results from research that exploits sub-national house price variation. We also discuss additional implications of our work. Section 5 concludes.

2 Data Description

To conduct the empirical analysis we combine a number of datasets. 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.

[9] A growing literature explores the cyclicality of price-setting behavior using CPI micro data, but does not try to isolate the response to demand shocks. Vavra (2014) uses U.S. CPI data to document that the frequency and size of price adjustment is countercyclical. Kryvtsov and Vincent (2014) show that the frequency of sales is also countercyclical. We show that our results hold both for posted and regular prices.

2.1 Retail Price Data

Retail pricing data are provided by IRI Worldwide, and have weekly store-level information for chain grocery and drug stores from 2001 to 2011.[10] The dataset includes store-week-UPC sales and quantity data for 31 product categories, which represent roughly 15% of household spending in the Consumer Expenditure Survey.[11] We 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.[12] 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.

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).[13] 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.[14]

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

[10] 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.

[11] These product categories include Beer, Carbonated Beverages, Coffee, Cold Cereal, Deodorant, Diapers, Facial Tissue, Photography Supplies, Frankfurters, Frozen Dinners, Frozen Pizza, Household Cleaners, Cigarettes, Mustard & Ketchup, Mayonnaise, Laundry Detergent, Margarine & Butter, Milk, Paper Towels, Peanut Butter, Razors, Blades, Salty Snacks, Shampoo, Soup, Spaghetti Sauce, Sugar Substitutes, Toilet Tissue, Toothbrushes, Toothpaste, and Yogurt. There is a correlation of roughly 0.6 between "food at home" and "all-items ex-shelter" in more aggregate BLS regional data, which suggests our results are generalizable to a larger set of goods.

[12] 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. See also Coibion, Gorodnichenko and Hong (2014) and Gagnon and Lopez-Salido (2014) for applications of the data to macroeconomic questions.

[13] Here it is important to note that we only look at the prices of an identical item (UPC-store pair) across time, so that changes in quality or problems associated with comparing non-identical products are not relevant for our results.

[14] 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 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 also 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.[15] In particular, we construct a geometric-weight price index with a consumption basket which is chained annually.[16] 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.[17] 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:[18]

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

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

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

[15] Since we are interested in extrapolating our results to inform aggregate inflation, we abstract from the effects of local variety on price indices explored in Handbury (2012).

[16] We observe revenues at high frequencies, which allows us to construct this chained index. 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 additional discussion. The CPI construction is similar but is a Laspeyres Index using a basket of goods that is only updated every five years.

[17] It is also important to note that quality changes will typically be associated with changes in UPCs, and we explicitly track prices of identical UPCs across time.

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

Panel A of Figure I shows that our price index qualitatively reproduces the behavior of the BLS food-at-home CPI.[19] While they do not match precisely, this is 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 dataset. 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.[20] To the extent that there are discrepancies, we believe that sampling error is smaller for our data than for the published metro area CPIs.[21] 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 sample.

2.2 Shopping Data

We use Homescan data from AC Nielsen to measure household-level shopping behavior.[22] The dataset 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

[19] The baseline price index construction described above implies that price indices are all normalized to 1 in period $t = 0$, so computation of the location-specific price index requires only knowledge of how individual items in a given location change prices across time. We do not require any information on how the same product is priced across locations at a given point in time. This is because we are interested in how prices change with house prices at business cycle frequencies and are not interested in identifying all potential dimensions of permanent price level differences across locations. Since our empirical specification is in changes rather than in levels, we avoid the various measurement complications and biases discussed in Handbury and Weinstein (2014). It is nevertheless worth noting that our result that retail prices move substantially with house prices might seem to conflict with the conclusion in Handbury and Weinstein (2014) that retail prices do not vary across locations after correcting for a number of biases. However, a few methodological differences actually mean that their cross-sectional "level" result is consistent with our time-series "change" result. In particular, their paper uses household level data and removes demographic effects such as income as well as retailer fixed effects from their geographic price indices. They do not directly add controls for house prices, and this implies that any correlation between retail prices and house prices that is also related to household demographics or retailer location will be absorbed in their controls. That is, they are measuring a conceptually distinct object and our variation of interest is mostly absorbed by the controls in their empirical specifications. It is also worth noting that we perfectly replicate their conclusion that retail prices do not grow with city size. Just as in their paper, we find a mild negative relationship between population and retail prices. This is to be expected as both our results and theirs remove the effects of UPC heterogeneity.

[20] For most regions, the increase in the CPI is modestly larger than the increase in the IRI index. This likely reflects standard substitution bias, since we use a chained index while the BLS uses a fixed basket.

[21] On average, metro area food-at-home price indices are constructed by the BLS from roughly two thousand price observations each quarter. The number of price observations in the IRI data is an order of magnitude larger, with more than 50,000 price observations per MSA per quarter. In addition, the IRI data covers a broader array of markets than the BLS data, and it is available at a more disaggregated level.

[22] 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.

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 dataset includes store identifiers, these codes are anonymized so that researchers cannot identify 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 and enter the number of units purchased. 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 and number of units purchased, households also record whether a product was purchased while "on sale", or using a coupon.[23] 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 and prizes to elicit accurate reporting and to reduce panel attrition. In addition, Nielsen expends a great deal of effort ensuring the quality and representativeness of their data. Projection weights are provided to make the sample representative of the overall U.S. population.[24] 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.[25]

2.3 Other Data

In addition to the IRI and Nielsen data, we use a number of other datasets in our analysis. We obtain house price indices at both the zip code level and the MSA level from CoreLogic, which computes

[23] 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.

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

[25] In addition to the UPC-data described above, the Nielsen data contain information on a set of "magnet" goods, such as produce and raw meat, that do not contain barcodes. We exclude these products from our analysis because they are only available for a small and non-representative set of households.

repeat sales price indices from individual transactions data.[26] In addition to house price data, we also use information on effective retail rents from 2000-2014 for 45 MSAs. These 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).[27] 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 instruments for house prices come from Gyourko, Saiz and Summers (2008) and Saiz (2010).

3 Empirical Analysis

In this section we provide an overview of our empirical strategy for identifying the impact of house prices on retail prices. We use two 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-

[26]Our empirical patterns persist when using house price indices from Zillow to measure house price changes, but the Zillow price indices are only available for a smaller set of locations.

[27]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 result, since geographic variation in homeownership rates is quite distinct from geographical variation in 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 Appendix A, we provide an extended discussion and supporting evidence for our identification assumptions.

In addition to documenting a causal link between house prices and retail prices, we provide evidence on the underlying economic mechanism that drives this relationship. In general, an increase in retail prices could reflect an increase in marginal cost or an increase in markups. We argue that our results primarily reflect markup variation by first showing that changes in observable costs do not drive our result. We then present direct evidence that households become less price sensitive after their housing wealth increases, which increases firms' optimal markups. Just as suggested by our retail price results, we also show that this change in household price sensitivity interacts strongly with individual homeownership status.

3.1 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 boom and bust. 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.[28]

Figure III shows the more disaggregated correlation between MSA-level house price growth and retail price growth over the periods 2001-2006 (Panel A) and 2007-2011 (Panel B). 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 in column 1 of Table I. However, while suggestive, these raw correlations do not establish causality, since there might be a third factor, such as time-varying productivity, that could simultaneously move both house prices and retail prices. If

[28] 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 actually 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.

we cannot directly control for this third factor in the OLS regression, we will obtain a biased estimate of the elasticity of retail prices to house prices.

Our first approach to dealing with this possible omitted variable bias is to exploit an instrumental variable (IV) 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 is less elastically supplied. During the housing bust, it is then precisely those areas where house prices rose the most that see the largest declines in house prices (Glaeser, 2013).

We use two measures of housing supply elasticity as instruments: 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 expanded. 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 boom, and larger house price drops during the housing bust.[29]

The exclusion restriction requires that housing supply elasticity does not affect retail prices except 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,

[29] Unsurprisingly, the power of the instrument is significantly stronger during the housing boom than during the housing bust. The first-stage F-statistic 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. That supply elasticity has predictive power during the bust is because it reflects ex-post unraveling of the differential house price bubble. See Appendix A for additional discussion.

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 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 the measures of housing supply elasticity and income growth in our sample: during the housing boom, the correlation of the Saiz (2010) instrument with income growth is 0.040, the correlation of the Wharton Regulation Index with income growth is -0.007. These correlations are -0.224 and 0.054, respectively, for the housing bust, and never statistically significant. One channel that could violate the instruments' exclusion restriction is that changes in the degree of local competition might correlated with the elasticity of housing supply, if the regulatory or geographic environment hinders the opening of new retail stores. In Section 3.2 we address this concern, and show that differential changes in competition cannot 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 control variables, which we add to the specifications in Section 3.2; the robustness of our results to the addition of control variables for local labor market conditions, retail rents, and changes in income and demographics will further alleviate concerns about whether our instruments satisfy the exclusion restriction. Appendix Table A1 provides summary statistics on the dependent variable and controls.

We first present results using the housing supply elasticity from Saiz (2010) as an instrument for house price changes. Column 2 of Table I presents the estimate from the second-stage regression. It implies that the elasticity of retail prices to house prices is about 12-13% during our sample. These elasticities are about 2 times as large as the estimates from the OLS regression presented in column 1 of Table I. This is consistent with the presence of local productivity shocks, which should 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. Column 3 of Table I shows the instrumental variables estimation using the Wharton Regulation Index (Gyourko, Saiz and Summers, 2008) 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.

3.2 Changes in Markups or Passthrough of Changes in Marginal Cost?

By definition, a change in retail prices can be decomposed into a change in marginal cost and a change in markups. While either explanation for the observed elasticity of retail prices to house prices would be interesting, in this section we provide several pieces of evidence that the relationship between house prices and retail prices is driven to large extent by markup variation. First, we argue that the vast majority of 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. We then directly control, to the extent possible, for shocks such as wages and retail rents that could affect the retailers' non-inventory marginal cost.[30] Finally, we present evidence that the elasticity interacts strongly with local homeownership rates and that individual household shopping behavior changes with house prices. Most cost-based stories for our pricing patterns would be independent of local homeownership status.

Share of Local Marginal Cost For the typical grocery store, the cost of goods sold makes up approximately 75% of total costs.[31] 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

[30] Controlling for more observable characteristics in the instrumental variables regression also reduces concerns about omitted variables that might be correlated with the instrument.

[31] 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.

goods sold.[32] For this reason, a change in a retailer's local demand is unlikely to be correlated with its marginal cost, which implies that the increase in retail prices we observe mostly reflects higher markups.[33] Nevertheless, we next attempt to directly rule out two cost-passthrough channels: labor costs and retail rents.

Labor Costs First, we address whether changes in labor market conditions could induce changes in the relatively small labor component of retailer's marginal cost. 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 component of marginal cost. To analyze such a channel, columns 4-6 of Table I control for changes in the unemployment rate and changes in average weekly wages. We also control for the change in the employment shares of the construction sector, the non-tradable sector and the retail food sector. This ensures that our results are not driven by industry-specific shocks. Consistent with a markup channel, the estimated elasticity of retail prices to house prices is constant across these specifications.[34]

In addition to shedding light on the mechanism that explains the response of retail prices to house prices, the controls in columns 5 and 6 of Table I serve a second purpose: although using an instrument for house price growth reduces endogeneity concerns, there is always a worry that an instrument may not perfectly satisfy the exclusion restriction. Controlling for various observables therefore reduces concerns that other economic variables might be explaining our estimates.

Retail Rents We next explore whether a pass-through of higher commercial rents can explain the retail price response to house prices.[35] We obtained annual effective retail rent data from REIS for 45

[32] In commodity flows survey data, the median MSA ships only 24% of its food and beverage shipments by total value less than 50 miles. 76% are shipped further than 50 miles, and are therefore not locally produced. However, this 24% figure overstates the contribution of local costs to total cost of goods sold, since the survey measures gross values rather than value added. Local distributors are important for grocery stores, but they represent a small share of value added in the grocery production chain. Industry input-output tables from the BEA imply that the intermediate's 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 relevant net intermediate input costs for food and beverage stores being determined within MSA.

[33] Since all of our evidence is cross-sectional, it is important to note that we are always measuring relative markups rather than absolute markups. Any aggregate changes in marginal cost are differenced out in our regressions.

[34] The positive coefficient on the unemployment change between 2001-2006 may seem surprising, but this could reflect important local supply shocks. It is also worth noting that if we instead control for the mean unemployment rate over the sample period, as suggested by early Phillips curve relationships, rather than the change in unemployment, then the coefficient becomes negative. This does not change the coefficient on house prices (see column 1 Table II).

[35] Whether retail rents should be considered a fixed cost or a variable cost in the running of a retail business probably depends on the time horizon considered. 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 cost remained constant, this pass-through channel would still represent an increase in markups.

MSAs. Appendix Figure A1 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 boom, and 0.08 in the bust. This relatively low passthrough of house prices to retail rents is consistent with the long duration of retail lease contracts, and the significant movement of the price-rent ratio in residential housing over this period (see, for example, Sinai, 2013). As a first quantitative exercise, even if an unrealistic 20% of marginal costs were made up of retail rents, these estimates suggest that passthrough of retail rents could explain at most 20% of the elasticity of retail prices to house prices.[36]

To test the extent to which the (relatively small) changes in retail rents can explain our results, Table III includes this 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 in retail rents increases the estimated response of retail prices to changes in house prices.[37] As further evidence, in column 2 of Table II 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.[38] In these markets retail rents are likely to make up a much larger fraction of total costs; therefore, if the passthrough 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 the same in this sample.

Finally, when we turn to our second identification scheme, we will show that the response of house prices to retail prices grows with local homeownership rates. An increase in local rents should affect a firm's costs in the same way whether the firm is located in an area with very many or very few homeowners. Thus, an explanation for our price pattern which relies primarily on pass-through of local land prices into commercial rent and retail prices will struggle to explain our homeownership interaction.

[36] If 20% of markups are retail rents, then a doubling of house prices will increase markups by 20% $\times 20\% = 4\%. This is about 20\% of the total increase in retail prices.$

[37] 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

[38] See http://www.colliers.com/en-us/~/media/files/marketresearch/global/2012globalreports/globalretailhighlightsmidyear2012.ashx. The six cities are Boston, MA; Chicago, IL, New York, NY; Los Angeles, NY; San Franisco, CA; Washington, DC.

Local Component in Cost of Goods We next provide some additional evidence that our empirical results are not driven by changes in local costs of goods sold. While we argued above that local wholesale costs should not be quantitatively important in general, there are certain products which 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 3 of Table II, 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 passthrough of local marginal cost is unlikely to explain our findings.

Entry and Exclusion Restriction As discussed in Section 3.1, 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 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 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.

We first test the correlation between our instruments and measures of entry in the retail sector. We measure entry by considering changes in the number of retail establishments, as provided by the County Business Patterns, normalized by the population. Our measures of supply elasticity are uncorrelated with the level of entry during both the housing boom and the housing bust. In addition, in column 4 of Table II we directly control for the change in the number of retail establishments per inhabitant (in addition to the share of retail employment that is already controlled for in all regressions reported in Table II). If anything, the estimated elasticity is slightly larger.[39] 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 an effect on entry in the retail sector.

[39]Similar results obtain when we directly control for the change in the absolute number of retail establishments, without normalizing by MSA population.

Demographic Changes 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.[40] In column 5 of Table II we control for changes in income as well as changes in the fraction of population that has completed at least highschool and at least a bachelor degree. In column 6 we control for population growth over our sample period. Our estimates are unaffected by the addition of these control variables. Consistent with this, Section 3.4 shows that individual household shopping behavior does indeed change in response to house price movements.

Robustness Checks While we believe that using the broadest price index possible is the appropriate benchmark, a large literature has explored the implications of sales for monetary policy.[41] In column 7 of Table II, we show that results are extremely similar when excluding temporary "sales" prices from our price index.

Finally, we want to ensure that our results are not driven by extreme outliers. In column 8 we exclude the MSAs with the largest and smallest 5% house price growth; in column 9 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.

3.3 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 a number of important advantages of these MSA-level estimates relative to estimates using house price and retail price measures at a more disaggregated level such as a zip code: first, nearly all of the grocery spending for a household should occur within an MSA, 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. Third, our housing supply elasticity instruments for house price changes 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

[40] 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.

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

homeownership rates across zip codes allows us to explore a separate, complementary identification strategy.[42] 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 and relax borrowing constraints for homeowners, they have no such effects on renters.[43] If house prices are capitalized into apartment rents or if renters plan to purchase in the future, then house price increases actually represent negative wealth shocks for renters.[44] Thus, if the positive relationship between retail prices and house prices is indeed 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, Figure IV shows the average retail price level for zip codes in the top and bottom quartile of house price growth between 2001 and 2011. Panel A focuses on zip codes in the bottom quarter of homeownership rates (average of 46%), Panel B on zip codes in the top quarter of homeownership rates (average of 86%). In both panels, those zip codes with larger house price increases have higher retail price growth. However, as one would expect if house price increases lead to wealth shocks, the differential price growth is much larger in 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.[45]

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

The results of this regression are presented in Table IV. Columns 1 and 5 show the elasticity between house prices and retail prices without controlling for other covariates for the periods 2001-2006 and

[42] Variation in homeownership rates at the zip code level is an order of magnitude larger than variation at the MSA level.

[43] 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.

[44] We use the effective apartment rent data from REIS to analyze the relationship between house prices and rents. 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 A1). The less-than-full passthrough of house price movements to rents is highly consistent with the fact that house price movements over this period conincided with significant swings in the price-rent ratio (see, for example, Sinai, 2013).

[45] 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.

2007-2011, respectively. The estimated elasticities are approximately 50% of the size of the MSA-level OLS estimates presented in Appendix 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 IV 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. To illustrate the significance of the interaction, Figure V plots the elasticity of retail prices to house prices for each level of the homeownership rate.

These results significantly strengthen the argument for a causal impact of house prices on retail prices. In particular, any omitted variable that might be correlated with our housing supply elasticity instruments in Section 3.1, 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 rates might capture some other characteristics of those neighborhoods. For example, high homeownership zip codes might have higher population density, and therefore have inhabitants that do more of their shopping within the zip code. This could explain the larger measured response of local house prices to local retail prices in those areas without relying on differential wealth effects. Similarly, high homeownership zip codes could primarily house older citizens who might be more responsive to house price changes for reasons other than the effect those house price changes have on their wealth or access to credit. To see whether these factors can explain our findings, columns 4 and 8 of Table IV 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.

Overall, these results provide important evidence for the impact of wealth-driven demand shocks on retail prices. Alternative stories, such as higher house prices leading to higher costs but constant markups, have difficulty explaining why changes in house prices have larger impacts on retail prices in zip codes with high homeownership rates.

3.4 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]

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

We measure local house prices at the quarter $\times$ zip code level.[49] We include quarter fixed effects to control for any aggregate time-trends. Importantly, we also control for household fixed effects. This keeps any household-specific determinants of shopping intensity, such as the disutility from compar-

[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 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 A4 and A6 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 sample only starts in 2004, which is halfway through the housing boom. More importantly, long-difference specifications rely on us observing the household for long time periods. Due to sample attrition and 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.

[49] Appendix Tables A5 and A6 show that our results are robust to measuring house prices at the quarter $\times$ MSA level.

ing prices or the baseline preference for generic goods, constant. The main parameter of interest is β_3, which captures how the shopping behavior of homeowners changes as house prices increase. The parameter β_1 is informative for changes in the shopping behavior of renters.

Columns 1 and 2 of Table V 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.

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 VI presents results from a version of regression 4 in which the unit of observation is a shopping outcome for each household $\times$ quarter $\times$ product category.[51] As before, we include household fixed effects but we now augment the specification with additional product category $\times$ quarter fixed effects.

Columns 1 and 2 show that, for homeowners, higher house prices lead to higher total expendi-

[50] Interestingly, the coefficient on homeowner, β_2, suggests that homeowners have lower nominal purchases and are more price sensitive than renters, which might at first seem counterintuitive. However, it is important to recall that due to the household fixed effects, this coefficient is only identified by households that change tenure while in the sample. It suggests that following the purchase of a house, households become more price sensitive, perhaps because of significantly tightened credit constraints or expenditures for a mortgage. Importantly, when removing household fixed effects, we find that, on average, homeowners have higher expenditures and appear less price sensitive than renters. When we drop all households that switch homeownership status during our sample, the estimated coefficients for β_1 and β_3 remain unaffected.

[51] The individual product categories are health & beauty care, dry grocery, frozen food, dairy, deli, packaged meat, fresh produce, non-food grocery, alcoholic beverages and general merchandize.

tures within each product category; higher house prices lead to lower expenditures for renters, though the effect is not statistically significant. The effect is of a similar magnitude as the estimates in Table V. Importantly, 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 all 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 Table A5 we therefore 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.

3.5 Discussion of Magnitude

We next discuss the implications of our house price effects for overall aggregate inflation during our sample period as well as for explaining geographic variation in inflation rates. First, how much did the aggregate house price boom and bust contribute to aggregate inflation over the same time period? Using CoreLogic data, average house prices increased by 36.5% from 2001-2006.[52] Our IV specifications in the housing boom imply elasticities of retail prices to house price movements of 12-23%. Thus, our empirical estimates imply that house price movements caused retail prices to rise by 4.4%-8.4%

[52] Note that Case-Shiller Indices find a larger increase in average house prices, but more heavily weights certain locations with more prominent housing booms. The 36.5% increase in house prices in our sample is quite similar to the 42% increase observed in the more broad OFHEO house price index.

between 2001-2006. Over the same time period, the total increase in the CPI food-at-home index was 13.2%, and the increase in the broad CPI was 14.5%.

Thus, house price movements generate significant but plausible changes in retail prices. Here, it is important to note that in addition to movements in house prices there are many factors such as oil prices and trade patterns that will influence aggregate inflation. This means that the fact that the aggregate price level did not precisely mirror the housing boom and bust does not mean that house-price induced demand shocks are not driving important changes in retail prices. Indeed, Beraja, Hurst and Ospina (2014) argue that the presence of offsetting aggregate shocks is crucial for understanding aggregate inflation. For example, during the housing boom, increasing import penetration from China likely held down overall retail price increases despite upward pressure from house price movements. 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, for a discussion of aggregate inflation dynamics during the Great Recession).

In addition to their aggregate implications, relative house price movements across locations also explain a significant fraction of the dispersion in retail price movements across locations. During the housing boom, moving from the 10th percentile of MSA house price growth to the 90th percentile of MSA house price growth implies an increase in relative retail prices of 7-10%. This compares to an overall 90-10 difference in retail prices of 12%. The same calculation in the housing bust implies that house prices 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 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 prices can be explained through this channel.

4 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.

4.1 Implications of Markup Variation

4.1.1 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; Leahy, 2011). For example, in New Keynesian models, firms produce differentiated products and have some pricing power for their variety.[53] Firm i faces demand with elasticity of substitution θ and nominal price P_t^i:[54]

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

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, $\mu_t = \frac{P_t}{\Psi_t}$, one can show the role of markups for production decisions. 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

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

[54] This demand function could be derived from a two-stage budgeting model in the spirit of Dixit and Stiglitz (1977), where households first decide on total consumption, and then on the allocation of that consumption across the varieties.

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 lead to countercyclical markups in response to demand shocks.

In this paper we identify an entirely separate channel which puts procyclical pressure on markups, even in an economy with flexible prices. In particular, we argue that 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 business cycles. 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. Expansion-

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

[56] Our evidence on firm price setting and markups comes from a set of non-durable retail goods. As such, one needs to be cautious to directly generalize to aggregate markups. In particular, our channel of declining price sensitivity and higher markups in booms for non-durable goods could interact with the "composition of demand" channel in Bils (1989) and Galı (1994). In those models, buyers of durable goods are more price sensitive. Since those goods constitute a larger share of total purchases during booms, the price elasticity for the average good increases in booms, putting downward pressure on markups. Similarly, in the model of Edmond and Veldkamp (2009) countercyclical income dispersion leads to countercyclical variation in total markups. Our channel is complementary: total markups do not only change because the composition of goods or households is changing, but also because the price elasticity for each good and household varies. Nevertheless, Panel C of Figure I shows that the cross-sectional variation in the "food at home" CPI produced by the BLS moves closely with the broader CPI. This suggests that house-price-induced demand shocks will also affect the pricing of goods that are not part of our sample.

ary 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.

4.1.2 Aggregate Time-Series Movements of Markups

Our results also contribute to a large literature that uses 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 getting at the sticky-price specific channel, one needs to hold price elasticity θ_t fixed. If firms' desired markups are constant, then measuring μ_t variation is equivalent to measuring variation due to sticky prices, but once θ_t changes across time, then this no longer holds.

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. Movements in markups over the business cycle may reflect movements in desired markups for firms that change prices 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 shocks. 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.

4.2 Implications of Price Variation

4.2.1 Housing Wealth Effect, and Aggregate Implications

We also contribute to a 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 housing 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, we find that house-price-induced demand shocks lead to higher retail prices, negating 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. As we have argued throughout the paper, we believe that marginal cost does not respond to these local housing wealth shocks. In contrast, when there is an aggregate increase in demand, marginal cost will increase, 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.

[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. Nevertheless, since our data focuses on grocery store spending and shows that there are strong effects of housing wealth even on these relatively low-cost goods, it suggests that housing wealth effects are operating through channels beyond pure effects on household borrowing and relaxing collateral constraints. It is unlikely that changes in households' grocery store spending behavior is being driven by extraction of home equity, given the large fixed costs of refinancing. It is substantially more likely that our evidence is being driven by more direct housing wealth effects on consumption.

4.2.2 Implications for Urban and Labor Economics

The response of local retail prices to local house prices can also help to 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, in order to encourage some households to move to less productive regions with lower wages. 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 directly informs the calibration of these equilibrium models.

A related literature considers the extent to which local price changes provide an insurance mechanism against local shocks. For example, Notowidigdo (2011) argues that house prices decline after negative labor market shocks, which can have an important role in helping households smooth consumption by reducing the total cost of housing. 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.

5 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 house prices when homeownership rates are high. We also find evidence of differential shopping effects 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 household and firms' local house price expectations. While we concentrated on constructing price indices for identical items in a fixed set of stores, there are also interesting questions about how store entry and product quality respond 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 housing 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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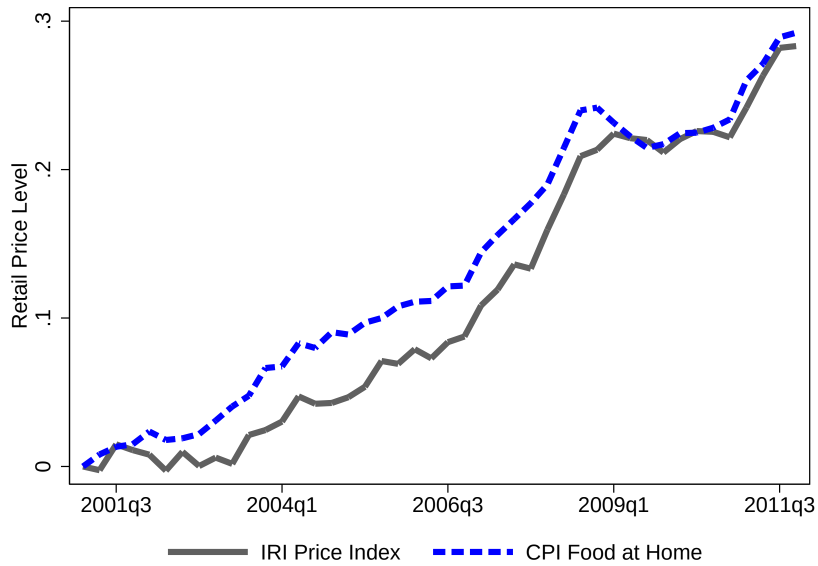
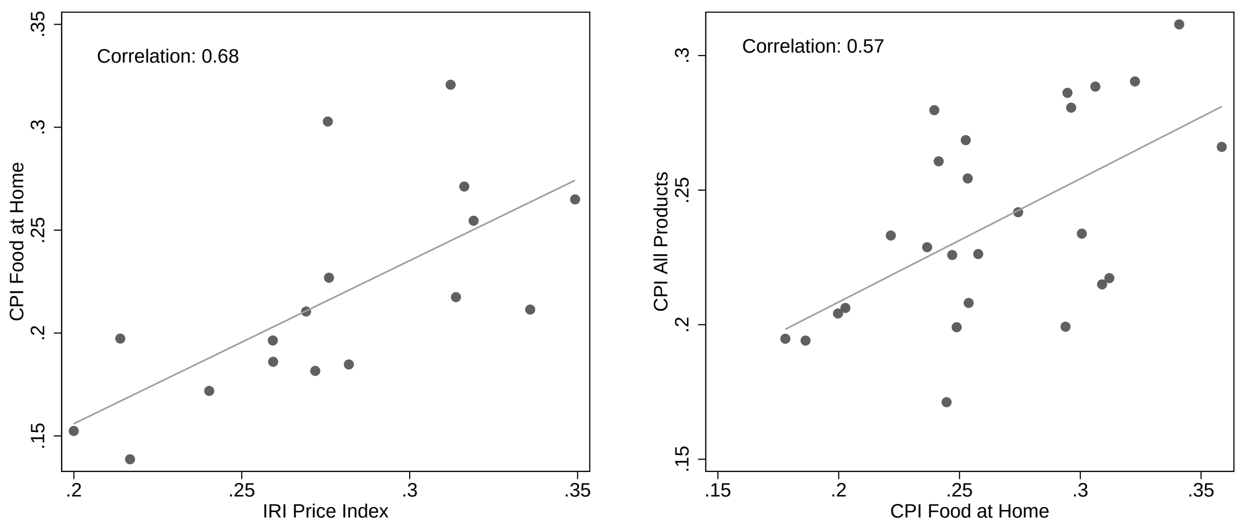
Figure I: Price Index vs. BLS



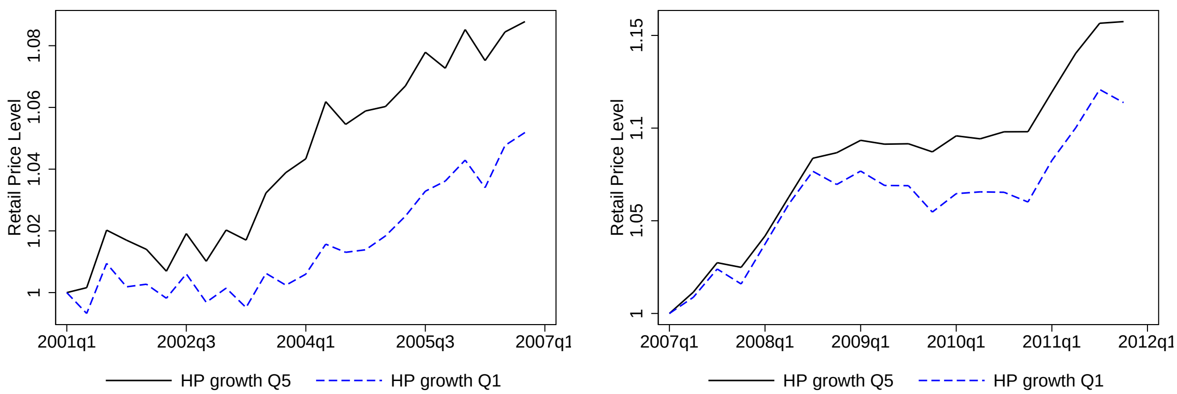
(A) Compared to "Food at Home" CPI



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

Note: Figure shows a comparison of our price indices produced with IRI data to inflation measures 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 between "food at home" prices and "all product" prices from the BLS.

Figure II: Retail Price Level (MSA) Time Series

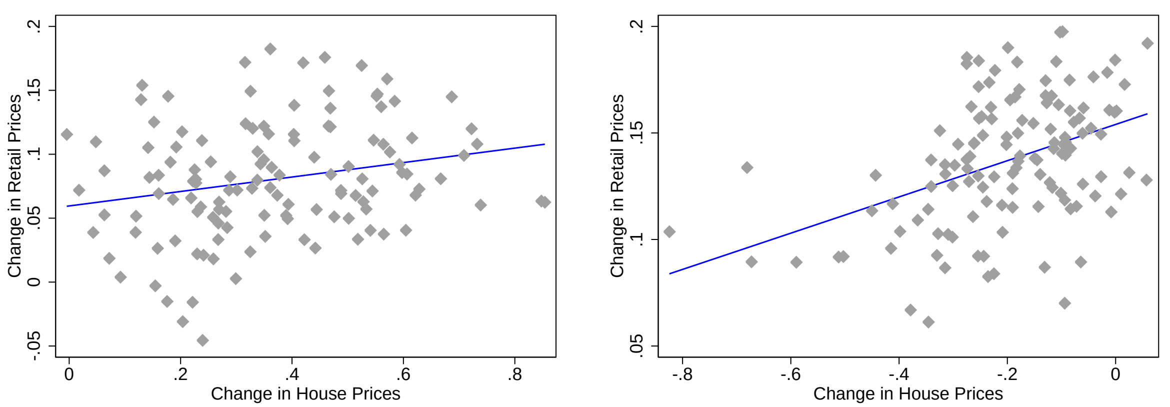


(A) Time Period: 2001-2006

(B) Time Period: 2007-2011

Note: Figure 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)

Figure III: Raw Correlation: House Prices and Retail Prices (MSAs)

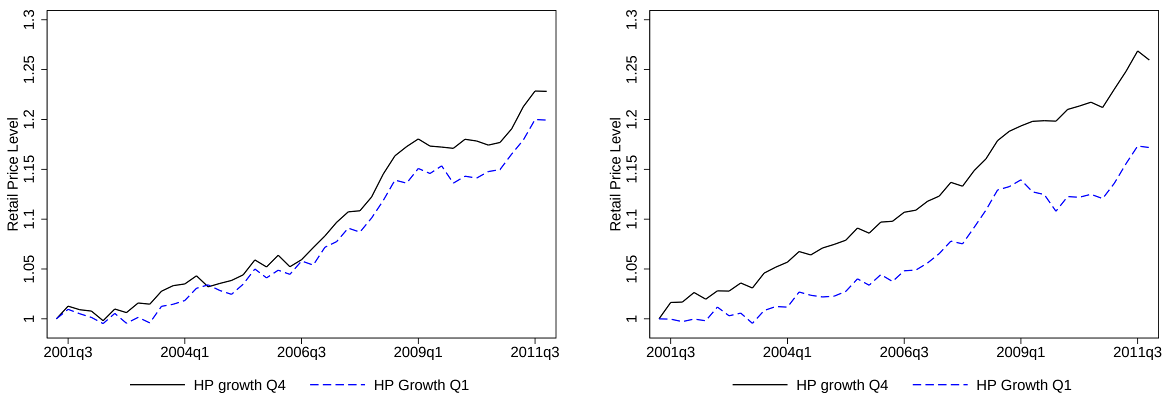


(A) Time Period: 2001-2006

(B) Time Period: 2007-2011

Note: Figure shows MSA-level correlation between changes in house prices and changes in retail prices for the period 2001-2006 (Panel A), and the period 2007-2011 (Panel B), as well as the line of best fit.

Figure IV: Retail Price Level (Zip Code) Time Series

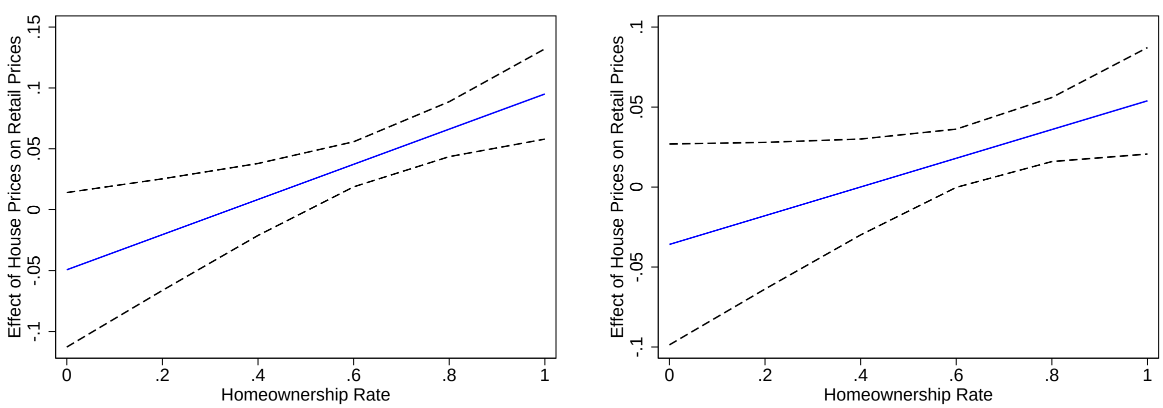


(A) Q1 of Homeownership Rate

(B) Q4 of Homeownership Rate

Note: Figure 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 in the US between 2001 and 2011. Panel A shows results of zip codes in the bottom quartile of homeownership rate, Panel B shows results of zip codes in the top quartile of homeownership rate.

Figure V: Retail Price Elasticity by Homeownership Rate



(A) Time Period: 2001-2006

(B) Time Period: 2007-2011

Note: Figure shows the estimated elasticity of retail prices to house prices by zip code level homeownership rates for the period 2001-2006 (Panel A), and the period 2007-2011 (Panel B). The dashed black lines correspond to the 95% confidence interval.

Table I: Retail Prices vs. House Prices

	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	0.057***	0.129***	0.224***	0.068***	0.153***	0.230***
	(0.020)	(0.042)	(0.048)	(0.023)	(0.058)	(0.048)
Δ Unemployment				0.039**	0.070**	0.095***
				(0.018)	(0.029)	(0.026)
Δ Wage				0.039	-0.005	0.038
				(0.055)	(0.061)	(0.060)
Δ Share Retail Empl.				-0.068	0.039	0.012
				(0.360)	(0.130)	(0.114)
Δ Share Nontradable Empl.				0.073	-0.130	-0.082
				(0.182)	(0.174)	(0.175)
Δ Share Construction Empl.				-0.060	0.219	0.132
				(0.098)	(0.391)	(0.376)
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	0.085***	0.124***	0.147***	0.086***	0.146***	0.157***
	(0.015)	(0.041)	(0.048)	(0.018)	(0.043)	(0.049)
Δ Unemployment				0.000	0.019	0.017
				(0.011)	(0.014)	(0.015)
Δ Wage				-0.030	-0.063	-0.060
				(0.044)	(0.049)	(0.048)
Δ Share Retail Empl.				-0.090	-0.040	-0.024
				(0.264)	(0.147)	(0.135)
Δ Share Nontradable Empl.				0.086	-0.003	-0.000
				(0.139)	(0.172)	(0.169)
Δ Share Construction Empl.				0.050	-0.000	0.008
				(0.127)	(0.282)	(0.275)
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 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 (i) Saiz (2010) in columns 2 and 5, and (ii) 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 II: Instrumental Variables Analysis - Robustness Checks

	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	0.115**	0.159***	0.145**	0.151***	0.158**	0.157***	0.188***	0.169***	0.195***
	(0.048)	(0.059)	(0.065)	(0.058)	(0.056)	(0.058)	(0.064)	(0.064)	(0.069)
	Panel B: Instrument with Saiz Supply Elasticity; 2007 - 2011								
Δ House Prices	0.131***	0.127**	0.121**	0.134***	0.123***	0.164***	0.145***	0.159***	0.139*
	(0.040)	(0.049)	(0.059)	(0.045)	(0.044)	(0.052)	(0.053)	(0.054)	(0.074)
	Panel C: Instrument with Wharton Regulation Index; 2001 - 2006								
Δ House Prices	0.196***	0.224***	0.194***	0.223***	0.300***	0.262***	0.235***	0.266***	0.274***
	(0.045)	(0.048)	(0.052)	(0.047)	(0.058)	(0.053)	(0.052)	(0.063)	(0.057)
	Panel D: Instrument with Wharton Regulation Index; 2007 - 2011								
Δ House Prices	0.155***	0.152***	0.246***	0.154***	0.153***	0.184***	0.161***	0.185***	0.181***
	(0.042)	(0.045)	(0.054)	(0.042)	(0.043)	(0.051)	(0.041)	(0.072)	(0.071)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Robustness	Average unemployment rate	Drop high retail rent cities	Exclude liquids and perishable goods	Control for entry in retail sector	Control for demographic changes	Control for population growth	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 retail sector. Column 1 controls for the average unemployment rate over the sample, rather than for changes in the unemployment rate. Column 2 drops the 6 cities with the highest level of retail rents. Column 3 drops all product categories classified as "perishable" in Bronnenberg, Kruger and Mela (2008) as well as liquids from our construction of the local price index. Column 4 controls for changes in the number of retail establishments per 1,000 citizens. Column 5 controls for changes in income using data from the IRS, as well as changes in the share of citizens who have completed highschool and the share who have completed a bachelor degree. Column 6 controls for the population growth using data from the annual population estimates for Metropolitan Statistical Areas produced by the US Census. Column 7 excludes sales prices in the construction of the retail price index. Column 8 excludes those MSAs with the 5% largest and smallest house price changes over the period. Column 9 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 III: Controlling for Retail Rent

	Panel A: Time Period: 2001 - 2006								
	Dependent Variable: Δ Retail Prices								
	OLS			IV (Saiz)			IV (Wharton)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
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 IV: 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 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 V: 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 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 VI: 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)	0.040***	0.044***	-0.018***	-0.020***	-0.006**	-0.008***	-0.012***	-0.014***
$\times \mathbb{1}_{Homeowner}$	(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 Retail		0.125**		-0.053***		0.009		-0.021**
Employment		(0.051)		(0.015)		(0.009)		(0.009)
Share Nontradable		0.135***		-0.025*		0.005		0.003
Employment		(0.048)		(0.015)		(0.009)		(0.009)
Share Construction		-0.172*		0.100***		-0.024		0.054***
Employment		(0.094)		(0.029)		(0.017)		(0.017)
Product Category $\times$ 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 $\times$ 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 $\times$ 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 3.1 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.

$$\Delta log(RetailPrice)_m = \beta \Delta log(HousePrice)_m + \gamma X_m + \underbrace{\psi D_m + \omega_m}_{\varepsilon_m} \tag{5}$$

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 downward 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 3.1 we introduced measures of the housing supply elasticity as an instrument for the change in house prices. The idea of the instrument is that during the 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. In other word, the instrument only works if $cov(SupplyElasticity_m, \Delta log(HousePrice)_m) \neq 0$. This is verified by the first-stage regression below (also regression 1 in the paper).

$$\Delta log(HousePrice)_m = \rho SupplyElasticity_m + \delta X_m + \epsilon_m \tag{6}$$

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.

$$Cov(SupplyElasticity_m, D_m) = 0 \tag{7}$$

The exclusion restriction is inherently untestable: if we observed D_m we would control for it directly by including it in X_m, avoiding the omitted variables problem. However, the fact that controlling for many observable characteristics in Tables I and II does not affect the estimated coefficient for β lends credibility to the validity of the instrument. Furthermore, in Section 3.3 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. In addition, the geographic variation in homeownership rates is quite distinct from variation in supply elasticity in the data.

B Price-Setting Behavior - High Frequency Results

In Section 3.1 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. In this Appendix we 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 8, the unit of observation is an MSA-quarter, and the key dependent variable is the log of the retail price level in that quarter.

$$log(RetailPrice)_{m,q} = \beta log(HousePrice)_{m,q} + \gamma X_{m,q} + \xi_m + \delta_q + \varepsilon_{m,q} \tag{8}$$

Columns 1 and 2 of Appendix Table A3 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 3.1 vary only in the cross-section, we also conduct an instrumental variables version of regression 8. 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 A3 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 A3 show results from the quarterly zip code level analysis in regression 9.

$$\begin{aligned} log(RetailPrice)_{z,q} = & \beta log(HousePrice)_{z,q} + \delta log(HousePrice)_{z,q} \times HomeOwnerShipRate_z + \\ & \gamma X_{m,q} + \xi_z + \delta_q + \varepsilon_{q,z} \end{aligned} \tag{9}$$

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

[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.

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.

C Price Index Construction – Robustness

In Section 2.1 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

[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.

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.

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 A7 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 A8 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 4 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, and therefore a decline in the demand elasticity faced by firms. 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: $\frac{\partial \Psi_t}{\partial D_l} = 0$

[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.

$\forall t$ and we get

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

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

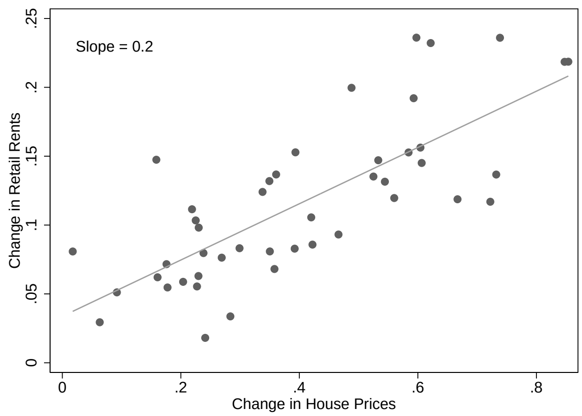
$$\begin{aligned} \frac{\partial \log \mu^{flex}}{\partial \log D_l} &= \frac{\frac{\partial P}{\partial \log D_l}}{[0.1 + 0.9(0.33)]} \\ &\simeq 2.5 \frac{\partial \log P}{\partial \log D_l}. \end{aligned}$$

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.

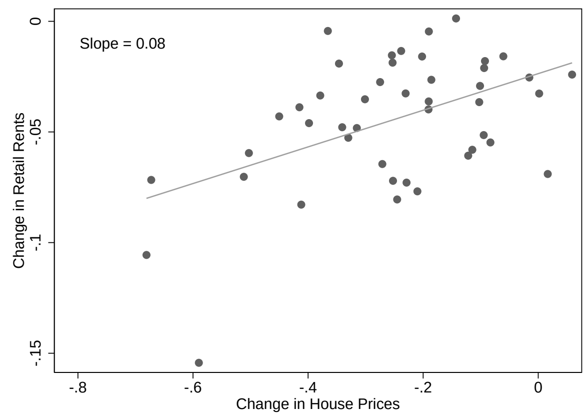
While we previously argued that assuming a constant marginal cost is sensible in our empirical context, the above formula can also be used to assess the plausibility of marginal cost movements for explaining our empirical results. If there was no change in μ^{flex}, and instead all results were driven 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

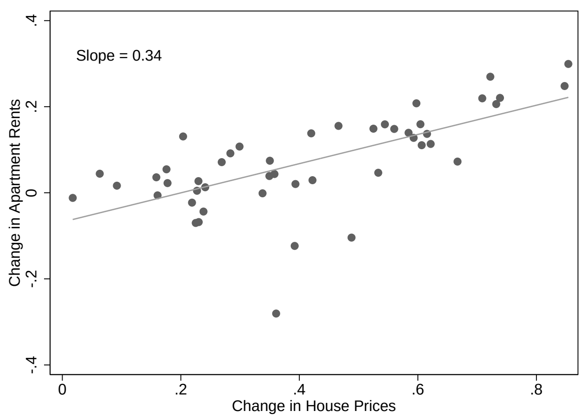
Figure A1: Changes in Apartment and Retail Rents vs. Changes in House Prices



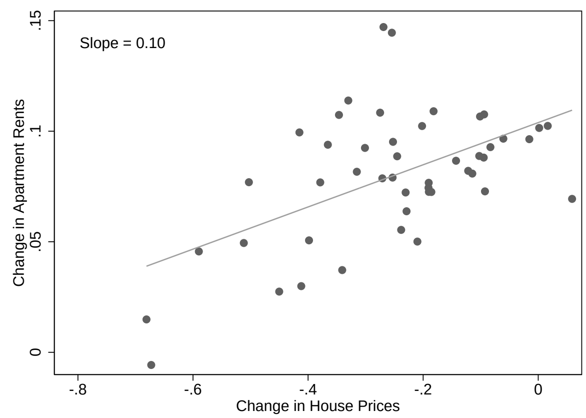
(A) Retail Rents: 2001-2006



(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 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 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 (Paenl 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: 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 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 8. 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 A4: 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 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 V, 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 A5: 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 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 × 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 × MSA fixed effects. Each observation is weighted by the household sampling weight. Standard errors are clustered at the MSA × quarter level. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table A6: 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 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 V, 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: 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***	0.144***	0.239***	0.066***	0.167***	0.239***
	(0.021)	(0.042)	(0.052)	(0.024)	(0.057)	(0.051)
Δ Share Retail Employment				-0.140	0.052	0.132
				(0.365)	(0.384)	(0.398)
Δ Share Nontradable Employment				0.092	-0.047	-0.091
				(0.185)	(0.179)	(0.179)
Δ Share Construction Employment				-0.103	-0.017	0.007
				(0.109)	(0.129)	(0.145)
Δ Unemployment				0.041**	0.080***	0.102***
				(0.020)	(0.031)	(0.029)
Δ Wage				0.062	0.049	0.010
				(0.059)	(0.063)	(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***	0.105**	0.115***	0.081***	0.132***	0.136***
	(0.014)	(0.042)	(0.045)	(0.017)	(0.050)	(0.039)
Δ Share Food Employment				-0.052	0.105	0.102
				(0.265)	(0.257)	(0.261)
Δ Share Nontradable Employment				0.046	-0.109	-0.110
				(0.154)	(0.158)	(0.158)
Δ Share Construction Employment				-0.012	-0.105	-0.111
				(0.131)	(0.142)	(0.142)
Δ Unemployment				-0.003	0.013	0.014
				(0.013)	(0.015)	(0.013)
Δ Wage				-0.036	-0.067	-0.068
				(0.046)	(0.048)	(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 (i) Saiz (2010) in columns 2 and 5, and (ii) 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 A8: 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 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 Food 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 (i) Saiz (2010) in columns 2 and 5, and (ii) the Wharton Regulation Index described in Gyourko, Saiz and Summers (2008) in columns 3 and 6. Robust standard errors in parenthesis. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).