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House Prices, Local Demand, and Retail Prices

Online Appendix

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A Identification Concerns and Instrumental Variables

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

Imagine that retail prices are affected by house prices, observable characteristics or shocks, X_m, and unobservable characteristics or shocks, D_m:

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

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 location, which would lead to an increase in house prices and a decrease in retail prices. Omitting controls for productivity from the OLS regression would therefore bias down our estimate of the true elasticity of house prices to retail prices.

The well-known idea of an instrumental variables research design is that if we can find a variable that predicts house price changes, but that is uncorrelated with D_m, we can obtain unbiased estimates of β. In Section 2.1 we introduced measures of the housing supply elasticity as instruments for the change in house prices. The idea of these instruments is that during the housing boom period, when there was a national housing demand shock, house prices in less elastic areas increased by more in response to this shock. During the reversal period, it was precisely those areas that experienced the biggest house price increases that then saw the largest house price declines. This suggests that $cov(SupplyElasticity_m, \Delta log(HousePrice)_m) \neq 0$ across both periods. We can verify this "inclusion restriction" by running the first-stage regression A1 (also regression 1 in the paper).

$$\Delta log(HousePrice)_m = \rho SupplyElasticity_m + \delta X_m + \epsilon_m. \tag{A1}$$

Put differently, even though our supply elasticity instrument is time-invariant, our empirical specification, which splits the sample into the boom and bust, means that predicted house price movements vary over these periods. 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 in both the boom and the bust periods.

It is worth re-emphasizing that this inclusion restriction is driven by the *interaction* between the

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instrument and a national housing demand shock – this is a difference from more traditional instrumental variables strategies, where the instrument is correlated with the endogenous variable independently of the presence of other shocks. In other words, the supply elasticitity instrument generates variation in house prices which is otherwise orthogonal to local shocks because local supply elasticity changes the propagation of aggregate shocks into local house prices. However, this does not change the causal interpretation of the resulting estimates. The identifying assumption, or the "exclusion restriction," does not depend on this distinction, and requires that the instrument is uncorrelated with any unobserved shocks that affect both house prices and retail prices, D_m.

$$Cov(SupplyElasticity_m, D_m) = 0. \tag{A2}$$

The exclusion restriction is inherently untestable: if we observed D_m we would control for it directly by including it in X_m, thereby avoiding the omitted variables problem. For example, in Section 2.3.5 we argue that changes in competition could potentially bias both our OLS and IV specifications. In particular, suppose that:

$$\Delta log(RetailPrice)_m = \beta \Delta log(HousePrice)_m + \gamma \Delta log(establishments) + \varepsilon_m,$$

so that retail prices are driven by both local house prices and changes in the local level of competition. If we did not control for $\Delta log(establishments)_m$, the IV exclusion restriction would require that:

$$Cov(SupplyElasticity_m, \Delta log(establishments)_m) = 0,$$

which might be violated, since locations where it is difficult to build housing might also face more restrictions on new retail entrants. Thus, if $\Delta log(establishments)_m$ was unobserved, then this would be problematic for our estimates. Fortunately, we can directly measure $\Delta log(establishments)_m$ using county business patterns data to control for this bias. That is, running the IV regression

$$\begin{aligned}
\Delta log(RetailPrice)_m &= \beta \Delta log(\widehat{HousePrice})_m + \gamma \Delta log(establishments) + \varepsilon_m \\
\Delta log(HousePrice)_m &= \rho SupplyElasticity_m + \delta \Delta log(establishments) + \epsilon_m
\end{aligned}$$

can produce an unbiased estimate of β, even if supply elasticity is correlated with local retail entry. We also control for many other possible observable confounders in our regression, including changes in income, and changes in demographics. Indeed, while we believe that effects on local competition are the most potent challenge to the exclusion restriction, the fact that controlling for many observable characteristics in Tables I and III does not affect the estimated coefficient for β lends credibility to the validity of the instrument. Furthermore, in Section 2.2 we present an alternative identification strategy using the interaction of house price changes with homeownership rates. To also explain these results, the unobserved shock D_m would have to differentially affect house prices in zip codes with different homeownership rates.

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B Price-Setting Behavior - High Frequency Results

In Section 2.1 we presented our baseline results using "long-difference" specifications in which we estimate the effect of changes in house prices over longer periods on changes in retail prices over the same period. We next provide more temporally disaggregated results. We document a strong relationship between house prices and retail prices at quarterly frequencies, suggesting that our results are relevant even for high-frequency business cycle analysis. In regression A3, the unit of observation is an MSA-quarter, and the key dependent variable is the log of the retail price level in that quarter.

$$log(RetailPrice)_{m,q} = \beta log(HousePrice)_{m,q} + \gamma X_{m,q} + \xi_m + \delta_q + \varepsilon_{m,q} \tag{A3}$$

Columns 1 and 2 of Appendix Table A6 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.[1] The estimated elasticity is 5%, which suggests that much of the long-run response of retail prices to house prices occurs at relatively high frequencies.

While our instruments for house price changes in Section 2.1 vary only in the cross-section, and, as discussed in Appendix A, only work if they differentially propagate a shock, we also conduct an instrumental variables version of regression A3. 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 to this national shock 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 A6 present the results from the IV regression, using the housing supply elasticity measures provided by Saiz (2010) and Gyourko, Saiz and Summers (2010), 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 A6 show results from the quarterly zip code-level analysis in regression A4.

$$\begin{aligned} log(RetailPrice)_{z,q} = &\beta log(HousePrice)_{z,q} + \delta log(HousePrice)_{z,q} \times HomeownershipRate_z + \\ &\gamma X_{m,q} + \xi_z + \delta_q + \varepsilon_{z,q} \end{aligned} \tag{A4}$$

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

[1] 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.

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Discussion While our findings using this "high frequency" specification are highly consistent with the "long difference" estimates, there are four reasons why we prefer the latter as our primary estimates. First, the high frequency measures of house price and retail price changes are likely to contain substantially more measurement error than the long difference measures. Higher measurement error in retail prices will increase standard errors; higher measurement error in house prices is even more problematic, and will bias our results due to attenuation bias. Second, we expect there to be some time lag between the actual house price changes, and households' response to them, for example because it takes time to learn about the house price changes, and because it takes time to extract home equity. Similarly, sticky prices might induce a delay between firms observing a change in their demand elasticity, and their ability to adjust prices (see Appendix Section D.2 for a discussion of how the presence of sticky prices might affect the interpretation of our results). The long difference specification allows us to estimate the full response, rather than a partial response. Fourth, most of our control variables are only reported at the annual level, and so the long difference specifications allow us to control, for example, for changes in income, unemployment, and wages at the same frequency as we are measuring price changes. Fourth, the long difference estimates correspond to the specifications in many of the seminal papers that have estimated nominal consumption responses to house price changes during the Great Recession (e.g., Mian and Sufi, 2011, 2014). By aligning our specifications with those of the existing literature, our estimates can be used to evaluate to what extent the consumption responses in those papers capture increases in real consumption, and to what extent they capture increases in prices.

C Price Index Construction – Robustness

In Section 1.1, we provide a description of our benchmark price index construction. In this Appendix, 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. The analysis begins with individual weekly price observations, which are themselves weekly unit values computed as the ratio of total sales to total units sold for a given UPC in a given store in a given week. In our baseline results, we move from this weekly data to a quarterly sample by only retaining price observations for the last week of each quarter.[2]

To construct our benchmark price indices, we first construct category-level price indices which

[2]While this potentially induces some additional measurement error, it substantially reduces the computational burden in the index construction and also eliminates the possibility that price changes arise from within quarter intertemporal substitution by households rather than from posted price changes by retailers. However, we have verified that we obtain nearly identical results when instead using all prices within a quarter. We have also implemented the exact methodology suggested by Gagnon, Lopez-Salido and Sockin (2016) which makes various alternative choices about missing prices and winsorizing and again arrive at nearly identical results.

aggregate up these individual price observations.[3] :

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

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

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

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 variation in our price indices, since all variation is driven by price relatives across time. To see this most clearly, assume that all products in city 1 are high quality, high price items, but that prices do not change across time, and that all products in city 2 are low quality, low price items, which also do not change prices across time. Since only location-specific price relatives contribute to location-specific price index changes, the price index in both cities in period 0 is normalized to 1, and the price index remains equal to 1 for all future dates. That is, permanent differences across location are essentially absorbed into a fixed effect that is differenced out of all of our empirical exercises. Similarly, product switching towards high quality, high price items also results in no change in the measured price index as long as these prices are not increasing differentially. This point is important to remember when comparing our evidence in Section 2.4, which showed that there are important changes in shopping behavior in response to house price movements, with the evidence below, which shows that using alternative expenditure weights does not affect the relationship between house prices and retail prices.

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

[3] In order to limit the influence of outliers we winsorize individual price relatives at ± 1 log point, we winsorize the top and bottom 1% of category price relatives and we winsorize local quarterly price changes at ± 0.025 log points. In addition, since zip codes vary substantially in the number of price observations, we weight all zip code level regressions by the total number of items measured in that zip code. None of these choices have important effects on our results.

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

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

In addition to these robustness checks, we have also experimented with constructing price indices at higher and lower time-frequencies, using different product mixes, excluding temporary price changes, including goods that were observed more than once but not in consecutive quarters, 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 Additional Discussion on Implications

In this section, we provide a more detailed discussion of the implications of our empirical results. We divide this discussion into two parts. In the first part, we discuss implications that arise from procyclical "flexible price" natural 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.

D.1 Implications of Markup Variation for Business Cycle Modeling

In many business cycle models, firm markups play an important role in determining the real response to expansionary monetary policy (Goodfriend and King, 1997). For example, many New Keynesian models assume that firm i produces differentiated products, and faces demand of the form:

$$c_t^i = \left(\frac{P_t^i}{P_t}\right)^{-\theta} C_t,$$

where θ is the elasticity of substitution,

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

is a consumption aggregate, P_t^i gives the firms' own nominal prices, 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. This markup varies with the price elasticity faced by the firm:

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

In the discussion in the paper we have called $\frac{\theta}{\theta-1}$ the "natural markup." The average realized markup in the economy is, in turn, crucial for determining real output. If we define the average markup as the ratio of the price level to marginal cost, $\mu_t = \frac{P_t}{\Psi_t}$, the cost-minimizing solution for labor input, given demand for a firm's product, must satisfy:

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

Substituting from the above definition then gives that

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

so a higher average realized markup corresponds to a higher marginal productivity of labor, and a real reduction in output.

In practice, there are at least two reasons why average realized markups can change with the business cycle. First, the presence of sticky prices may prevent some firms from adjusting prices when their marginal costs move, holding firms' desired or natural markups constant. In other words, average markups can move because the "markup gap" between the realized and the natural markup moves. In the traditional New Keynesian mechanism, θ (and thus the natural markup) is typically held con-

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stant and countercyclical realized markups arise entirely from the countercyclical forces generated by sticky-price-induced markup gaps. In this paper we identify a separate channel that puts procyclical pressure on natural markups, and thereby on realized markups: during boom periods, households become less price-sensitive, θ_t falls, and firms' natural markups increase. Holding marginal cost constant, this then leads actual markups to increase as long as prices are not completely fixed.

While a vast literature studies the implications of sticky prices, comparatively little attention has been devoted to studying cyclicality of the elasticity of demand. In practice, both sticky-price forces and price-sensitivity forces are likely at work in determining aggregate markups over the business cycle, and their relative importance depends, for example, on the particular shocks considered, or the specific time period studied. For example, since we show that marginal cost does not respond to our *local* house-price-induced demand shocks, this means that actual realized markups are procyclical in response to *these shocks*. However, it is important to note that aggregate demand shocks may generate substantially more upward pressure on marginal cost than the local shocks we study. If marginal costs do rise with aggregate demand, and if prices are at least partially sticky, then the New Keynesian sticky price channel will put downward pressure on markups.[4] The actual realized markup will then depend on the importance of the movement in the natural markup relative to that of the markup gap.

The presence of these competing forces also has implications for the large literature using aggregate time-series data to measure the cyclicality of markups. Nekarda and Ramey (2013) review that literature. While looking at time-series variation in total markups might be the right approach for measuring the total effects of a policy change, if one is interested in isolating the effects of sticky prices in order to test New Keynesian models, one needs to hold price elasticity θ_t fixed. If firms' natural markups are constant, then realized markups only move due to sticky prices, but once θ_t changes across time, then this no longer holds. If price flexibility also varies across time, as suggested by Vavra (2014), then so will the decomposition of the total markup into a "natural markup effect" and a "sticky price effect." Time-variation in the strength of these effects could thus potentially reconcile conflicting evidence on the response of total markups to demand shocks. For example, Gali, Gertler and Lopez-Salido (2007) find that markups fall in response to expansionary monetary policy. However, using an identical methodology, Nekarda and Ramey (2013) show that this result changes when using revised data for the last few years of the sample.

Thus, the countercyclical shopping intensity channel we identify need not imply that the sticky price effect is unimportant, and it does not imply that the total aggregate markup is procyclical; however, this shopping intensity channel nevertheless has important implications for the conduct of monetary policy. To see this, consider the DSGE models in Smets and Wouters (2007), Christiano, Motto and Rostagno (2010), and Justiniano, Primiceri and Tambalotti (2010). These models allow for exogenous "cost-push" shocks to the natural 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 natural markup are interpreted as exogenous "structural" shocks, and as such they do not respond to policy. In contrast, we provide evidence for endogenous natural markups: during booms, households become less price-sensitive, and firms raise

[4] While, in our setting, the local house price shocks do not appear to have a significant effect on local marginal costs, many business cycle shocks are likely to move both θ and marginal costs together.

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markups in response. This is an important distinction, because our results imply that natural markups will work against the traditional expansionary effects of stimulus policy. Expansionary monetary policy may lower markups through a traditional New Keynesian channel, which will in turn drive up output. However, as output begins to rise, households will become less price-sensitive, which puts upward pressure on markups. Treating movements in the natural 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. Our empirical evidence suggests that more attention should be paid to modeling the effects of cyclical price-sensitivity on the economy.

D.2 Implications of Sticky Prices for our Interpretation

If prices are fully flexible, then our retail price responses provide a direct measure of the change in θ. However, in the presence of sticky prices, changes in the "flexible price" natural markups arising from variation in θ cannot be immediately realized, because not all firms can immediately increase their prices to the new, desired level. In this case, our elasticities represent a lower bound on the response of flexible price natural markups.

Our benchmark analysis focuses on multi-year differences where sticky prices are unlikely to be important. In Appendix B we show that elasticities remain large and significant at quarterly frequencies but are reduced by roughly one-third relative to our long-difference analysis. We now show that this is consistent with the presence of some shorter-run pricing frictions. To highlight different sources of variation, we decompose the actual realized markup into those markups set by fully flexible-price firms (natural markups) 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. 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 deviation of the natural markup from the steady-state markup. 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 natural 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}.

Now consider 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. $\frac{\partial E \log\left[\mu_t^{flex}\Psi_t\right]}{\partial \log D_l}$ is the expected response of flex price markups and marginal cost to the demand shock for today and all future periods. If goods are not produced locally, local demand should have no effect on marginal cost: $\frac{\partial \Psi_t}{\partial D_l} = 0 \; \forall t$ and we get

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

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 25% of grocery store prices are fully flexible, and that the quarterly frequency of adjustment is roughly 40% for the remaining items, as in our IRI data. This implies that

$$\frac{\partial \log \mu^{flex}}{\partial \log D_l} = \frac{\frac{\partial P}{\partial \log D_l}}{[0.25 + 0.75(0.40)]} \simeq 2.5\frac{\partial \log P}{\partial \log D_l}. \tag{A5}$$

In this scenario, the long-run elasticity with fully-flexible prices should be roughly 80% larger than the quarterly elasticity with sticky-prices, which is in line with our empirical estimates.

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 little relationship between average local wage growth and local housing wealth shocks.

D.3 Industrial Organization of Wholesale-Retail Relationships

Our paper also provides novel evidence on the organization of wholesaler-retailer relationships, as well as on the nature of local competition. Much of the prior empirical literature studying retail pricing has emphasized the importance of wholesalers in pricing decisions, and has generally concluded that retailers play a largely passive role in the pricing process. For example, Nakamura and Zerom (2010) show that a large fraction of incomplete pass-through of coffee commodity prices into retail prices arises from incomplete pass-through into wholesale costs. Andreson et al. (2013) show that prices for a confidential retailer typically respond rapidly to changes in costs. Similarly, it is standard in New Keynesian models to introduce pricing power and sticky prices at the level of the intermediate goods producer rather than at the level of the final goods producer.

Our evidence shows that this passive view of retailers is incomplete. In particular, the prior literature focuses on the response to *cost* shocks. We show that the passive role of retailers does not hold when one instead focuses on the response to *demand* shocks. More concretely, suppose that retail prices are fully flexible and that retailers are subject to both wholesale cost shocks and shocks to the elasticity of demand. In this case, at each point in time, the retailer will set price

$$P_t = \frac{\theta_t}{\theta_t - 1} \alpha_t wc_t$$

where θ_t is the current standard CES elasticity of demand, α_t is an additional variable markup/ incomplete pass-through parameter, and wc_t is the current wholesale cost, which in turn is given by

$$wc_t = \gamma_t c_t$$

with commodity cost c_t and pass-through parameter γ_t. The existing literature has studied $\frac{dP}{dc_t}$ and concluded that at medium-run frequencies $\frac{dP}{dc_t} < 1$, and that this incomplete pass-through is largely driven by $\gamma_t < 1$ while $\alpha_t \approx 1$. Thus, retailers generally passively pass-through shocks to their costs in the medium-run. However, this does not imply that retailers are passive in general, and that one can fully understand final goods prices by focusing on wholesalers. Our evidence shows that retail prices respond strongly to changes in θ_t while wholesale prices do not. This implies that fully understanding the behavior of prices requires a comprehensive analysis of the interaction of retailers and their suppliers. Retailers are crucial in determining how prices and markups respond to changes in customer demand, while wholesalers play a crucial role in transmitting upstream cost shocks into final prices.

The fact that wc_t does not respond to changes in θ_t also suggests that local variation in profits is fully absorbed by retailers rather than split between wholesalers and retailers. That is, local declines in the elasticity of demand lead to higher markups and unit profits for the retailer but no increase in wholesale costs or unit profits for the wholesaler. This is an empirical result of our paper shown most explicitly in 2.3.1, rather than any assumption on our part, and it has interesting implications for the nature of retailer-wholesaler interactions. One potential explanation for this result is that retailers likely have much more information on their local customer demand than any wholesaler does. This may allow retailers to exploit their market power more fully without it being appropriated by increases in wholesale costs. This variation in retailer profits is highly consistent with our results in 2.3.5 that entry is mildly higher in locations where supply elasticity is low and markups rise more quickly. Seeing more entry in locations where it is ex-ante more difficult to enter only makes sense if these locations are also more profitable for retailers.

D.4 Implications of Price Variation

We next discuss a number of implications of our findings that do not depend on the decomposition of overall observed price changes into changes in markups and changes in marginal costs.

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D.4.1 Housing Wealth Effect, and Aggregate Implications

First, our analysis contributes to the literature that has analyzed the effects of house price changes on household consumption behavior (e.g., Case, Quigley and Shiller, 2011, and Carroll, Otsuka and Slacalek, 2011). From a theoretical perspective, it is unclear whether changes in house prices should induce significant wealth effects for homeowners. For example, Sinai and Souleles (2005) argue that while house price increases lead to higher housing asset values, these effects are undone by an increase in the houses' implicit rent so that consumption should be unchanged. However, Berger et al. (2016) show that if borrowing constraints and realistic substitution effects are added to this framework, then there can be substantial consumption response to house price movements. Our empirical results join a growing body of work consistent with this more general framework, as it shows that homeowners substantially change their behavior in response to house price changes.

More importantly, the fact that local prices are also responding to these local housing wealth shocks means that one should use caution when trying to learn from cross-sectional variation about the aggregate response of consumption to changes in housing wealth. Without local price indices, nominal spending responses cannot be decomposed into real consumption growth and inflation. Mian and Sufi (2014) 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, explaining at least some of the observed increases in nominal consumption. Most directly, our analysis of expenditures using the Homescan data suggests that about 30% of nominal expenditure increases reflect higher prices.

D.4.2 Implications for Urban and Labor Economics

The response of local retail prices to local house prices can also help inform important parameters in models of urban economics (e.g., Shapiro, 2006, and Albouy, 2009). In equilibrium models along the lines of Roback (1981), households and firms have to be indifferent between locating in different areas. Each area is endowed with its own productivity and consumption amenities. Wages must be higher in more productive locations, otherwise firms would want to move there. Housing costs also have to be higher in those more productive regions to discourage all households from moving there. Land prices capitalize consumption amenities, making it more expensive to live in more desirable regions. The utility consequences of a change in land prices depend on whether this change has an impact on the cost of traded and non-traded consumption goods. This affects the adjustment mechanism to local shocks, as well as the incidence of these shocks. Our causal estimate of the impact of house prices on retail prices therefore directly informs the calibration of these equilibrium models.

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

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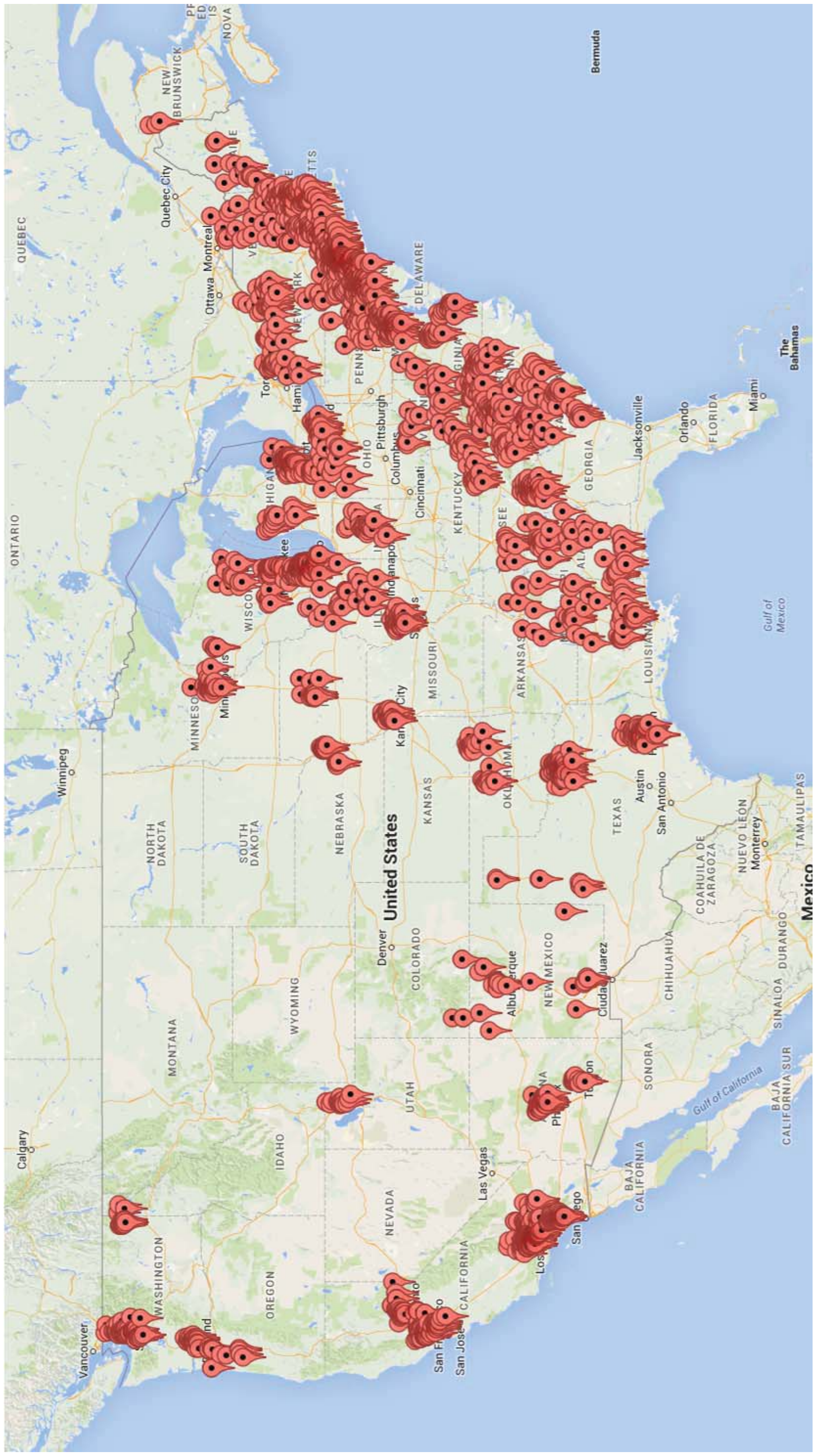
Appendix Figures

Figure A1: Location of Retail Stores in IRI Sample



Note: Figure shows the location of the zip codes in which we observe stores in the IRI sample. Most major U.S. population centers are covered, with notable omissions of Florida, Nevada, and Colorado.

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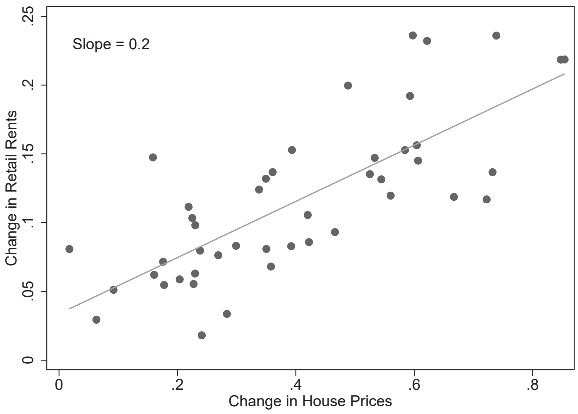
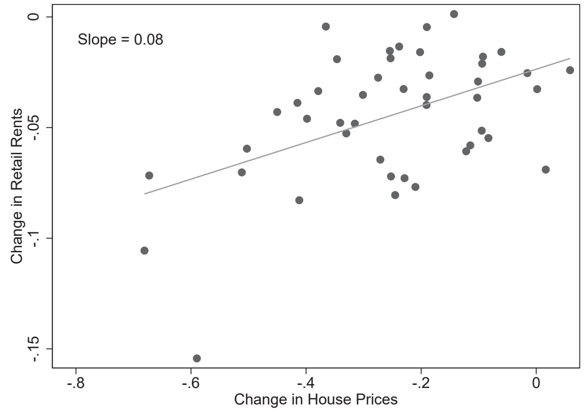
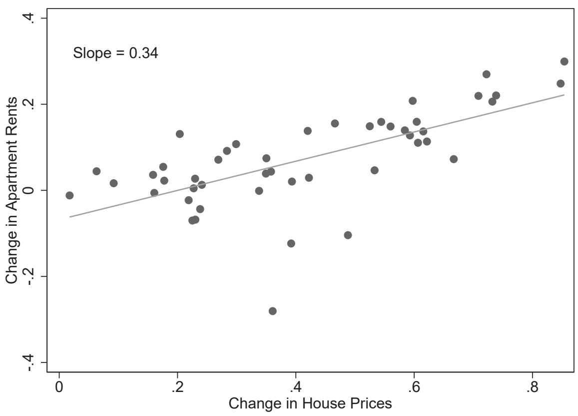
Figure A2: Changes in Apartment and Retail Rents vs. Changes in House Prices



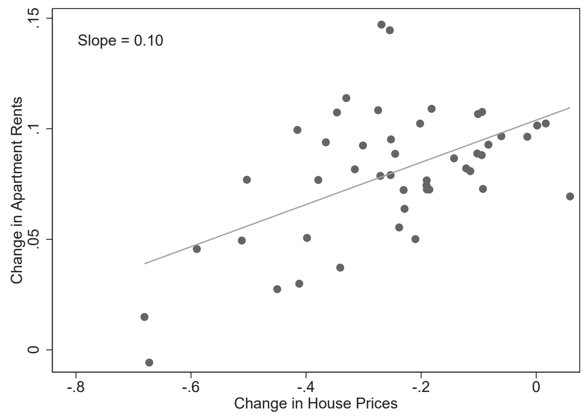
(A) Retail Rents: 2001-2006



(B) Retail Rents: 2007-2011



(C) Apartment Rents: 2001-2006



(D) Apartment Rents: 2007-2011

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

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Appendix Tables

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

PANEL A: TIME PERIOD: 2001 - 2006

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

PANEL B: TIME PERIOD: 2007 - 2011

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

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

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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*** (0.015)	-0.088*** (0.016)			0.055*** (0.012)	0.048*** (0.012)		
Wharton Regulation Index			0.124*** (0.017)	0.126*** (0.016)			-0.071*** (0.017)	-0.088*** (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 parentheses. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

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Table A3: Retail Prices vs. House Prices: Zip Code-Level Analysis (Robustness)

	Time Period: 2001-2006			Time Period: 2007-2011		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ House Prices	-0.170*	-0.169*	-0.124	-0.072	-0.082	-0.005
	(0.095)	(0.094)	(0.123)	(0.084)	(0.082)	(0.097)
Homeownership Rate	-0.120***	-0.113**	-0.100*	0.021	0.044	0.040
	(0.046)	(0.050)	(0.053)	(0.030)	(0.032)	(0.033)
Δ House Prices	0.222***	0.220**	0.197**	0.123*	0.157**	0.137*
× Homeownership Rate	(0.081)	(0.091)	(0.096)	(0.071)	(0.078)	(0.077)
Δ Unemployment	0.057***	0.057***	0.057***	-0.014*	-0.011	-0.013
	(0.013)	(0.013)	(0.013)	(0.008)	(0.008)	(0.008)
Δ Wage	0.047	0.051*	0.049*	0.007	0.003	0.008
	(0.029)	(0.030)	(0.030)	(0.025)	(0.025)	(0.025)
Δ Share Retail Employment	-0.241	-0.244	-0.251	0.078	0.098	0.142
	(0.295)	(0.297)	(0.298)	(0.216)	(0.213)	(0.219)
Δ Share Nontradable Employment	0.085	0.091	0.094	0.037	0.025	0.018
	(0.122)	(0.123)	(0.124)	(0.101)	(0.102)	(0.103)
Δ Share Construction Employment	-0.184***	-0.190**	-0.199**	0.114	0.127	0.110
	(0.071)	(0.076)	(0.078)	(0.076)	(0.078)	(0.079)
Population Density	0.001	0.001	0.001	-0.000	0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Δ House Prices	-0.002	-0.002	-0.002	0.002	0.003	0.005
× Population Density	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Share below 35 years	-0.002**	-0.002**	-0.002	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Δ House Prices	0.003*	0.003*	0.003	0.000	0.000	-0.000
× Share below 35 years	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Median Income		-0.000	-0.000		-0.000*	-0.000**
		(0.000)	(0.000)		(0.000)	(0.000)
Δ House Prices		0.000	-0.000		-0.000	-0.001*
× Median Income		(0.000)	(0.001)		(0.000)	(0.000)
Share High School or Less			0.000			-0.000
			(0.000)			(0.000)
Δ House Prices			-0.001			-0.001*
× Share High School or Less			(0.001)			(0.001)
Share White			-0.011			0.005
			(0.023)			(0.018)
Δ House Prices			0.028			0.067
× Share White			(0.046)			(0.042)
R-squared	0.084	0.082	0.078	0.046	0.049	0.059
N	708	708	708	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 - 3, and the change in retail prices in 2007-2011 in columns 4 - 6. Columns 1 and 4 correspond to columns 4 and 8 in Table II, respectively. Median income is measured in $1,000. Robust standard errors in parenthesis. Significance levels: * (p<0.10), ** (p<0.05), *** (p<0.01).

Table A4: Entry – Δ Grocery Retail Establishments per 1000 people

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

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

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

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Table A5: Instrumental Variables Analysis - Robustness Checks

	Dependent Variable: Δ Retail Prices						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Instrument with Saiz Supply Elasticity; 2001 - 2006							
Δ House Prices	0.164**	0.171***	0.174***	0.188***	0.169***	0.195***	0.170**
	(0.083)	(0.066)	(0.057)	(0.064)	(0.064)	(0.069)	(0.070)
Panel B: Instrument with Saiz Supply Elasticity; 2007 - 2011							
Δ House Prices	0.160***	0.142*	0.101**	0.145***	0.159***	0.139*	0.131***
	(0.053)	(0.077)	(0.051)	(0.053)	(0.054)	(0.074)	(0.045)
Panel C: Instrument with Wharton Regulation Index; 2001 - 2006							
Δ House Prices	0.261***	0.274***	0.223***	0.235***	0.266***	0.274***	0.267***
	(0.066)	(0.063)	(0.045)	(0.052)	(0.063)	(0.057)	(0.068)
Panel D: Instrument with Wharton Regulation Index; 2007 - 2011							
Δ House Prices	0.167***	0.196**	0.147***	0.161***	0.185**	0.181**	0.175***
	(0.045)	(0.091)	(0.043)	(0.041)	(0.072)	(0.071)	(0.045)
Controls	✓	✓	✓	✓	✓	✓	✓
Robustness	Coast Dummy	4 Census Region Fixed Effects	9 Census Division Fixed Effects	Exclude sales	Exclude outliers in house price changes	Drop bubble states (CA, AZ, FL)	Non-linear instruments in IV

Note: Table shows results from regression 2. The unit of observation is an MSA, the dependent variable is the change in retail prices in 2001-2006 (Panels A and C) and 2007-2011 (Panels B and D). In Panels A and B we instrument for the change in house prices using the housing supply elasticity measure provided by Saiz (2010); in Panels C and D we instrument for house price changes using the Wharton Regulation Index described in Gyourko, Saiz and Summers (2008). All specifications control for changes in the unemployment rate, changes in wages, and changes in the employment share in the construction, non-tradable, and grocery retail sector. Column 1 includes a coast dummy. Column 2 includes fixed effects for four census regions. Column 3 includes fixed effects for nine census divisions. Column 4 excludes sales prices in the construction of the retail price index. Column 5 excludes those MSAs with the 5% largest and smallest house price changes over the period. Column 6 excludes observations from the "bubble states" Arizona, California and Florida. Robust standard errors in parenthesis. In column 7 we use the measure of supply elasticity as well as the measure squared and cubed as instrumental variables. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

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Table A6: 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***	0.051***	0.108***	0.156***	0.010***	0.017**	-0.019*	-0.018
	(0.011)	(0.011)	(0.029)	(0.035)	(0.001)	(0.005)	(0.011)	(0.011)
Unemployment Rate		0.068	0.283**	0.443***		0.004		0.004
		(0.094)	(0.140)	(0.151)		(0.004)		(0.004)
Average Weekly Wage		-0.007	-0.010	-0.019		0.004		0.003
		(0.025)	(0.026)	(0.027)		(0.008)		(0.008)
Share Grocery Retail Employment		0.167	0.180*	0.157		0.003		-0.002
		(0.110)	(0.104)	(0.107)		(0.089)		(0.089)
Share Nontradable Employment		-0.151**	-0.171***	-0.172**		0.008		0.015
		(0.061)	(0.065)	(0.067)		(0.054)		(0.054)
Share Construction Employment		-0.080**	-0.116**	-0.139***		-0.019		-0.032
		(0.041)	(0.050)	(0.053)		(0.030)		(0.031)
log(House Prices) × Homeownership Rate							0.052***	0.053***
							(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 A3. 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: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Supplemental Material for: Johannes Stroebel, Joseph Vavra. 2019. "House Prices, Local Demand, and Retail Prices." Journal of Political Economy 127(3). DOI: 10.1086/701422.

Table A7: 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)	(9)	(10)	(11)	(12)
log(House Prices)	-0.025	-0.021	0.264***	-0.007	-0.002	0.030	0.004	0.003	0.014	0.002	0.000	0.006
	(0.017)	(0.017)	(0.079)	(0.005)	(0.005)	(0.023)	(0.003)	(0.003)	(0.015)	(0.003)	(0.003)	(0.013)
$\mathbb{1}_{Homeowner}$	-0.202**	-0.195**	-0.233***	0.063**	0.063**	0.053*	0.031**	0.030**	0.024	0.049***	0.048***	0.033**
	(0.083)	(0.083)	(0.086)	(0.027)	(0.027)	(0.028)	(0.015)	(0.015)	(0.016)	(0.013)	(0.013)	(0.013)
log(House Prices)	0.046***	0.044***	0.050***	-0.012**	-0.012**	-0.010*	-0.006*	-0.006*	-0.004	-0.009***	-0.009***	-0.006**
$\times\mathbb{1}_{Homeowner}$	(0.017)	(0.017)	(0.017)	(0.005)	(0.005)	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Unemployment Rate		0.253***	0.277***		0.098***	0.094***		-0.039**	-0.041**		-0.043***	-0.044***
		(0.090)	(0.090)		(0.024)	(0.024)		(0.016)	(0.016)		(0.013)	(0.013)
Average Weekly Wage		0.030*	0.032*		-0.012***	-0.013***		-0.001	-0.001		-0.004	-0.004*
		(0.017)	(0.017)		(0.004)	(0.004)		(0.003)	(0.003)		(0.002)	(0.002)
Share Grocery Retail		0.011	0.036		-0.058**	-0.062**		-0.019	-0.020		0.018	0.017
Employment		(0.092)	(0.092)		(0.025)	(0.025)		(0.017)	(0.017)		(0.013)	(0.013)
Share Nontradable		0.107**	0.098*		0.042***	0.042***		0.013	0.013		-0.001	-0.000
Employment		(0.052)	(0.052)		(0.014)	(0.014)		(0.010)	(0.009)		(0.007)	(0.007)
Share Construction		0.218***	0.214***		-0.007	-0.009		-0.022**	-0.023**		-0.013*	-0.012
Employment		(0.054)	(0.054)		(0.014)	(0.014)		(0.010)	(0.010)		(0.007)	(0.007)
Product Category $\times$ Quarter Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household $\times$ Zip Code FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R-squared	0.560	0.560	0.560	0.521	0.521	0.521	0.283	0.283	0.283	0.351	0.351	0.351
$\bar{y}$	3.902	3.902	3.902	0.265	0.265	0.265	0.185	0.185	0.185	0.070	0.070	0.070
N	7,520,617	7,520,617	7,520,617	7,520,617	7,520,617	7,520,617	7,520,617	7,520,617	7,520,617	7,520,617	7,520,617	7,520,617

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 1-3), the expenditure share of products that are on sale (columns 4-6), the expenditure share of generic products (columns 7-9), and the expenditure share of products purchased with a coupon (columns 10-12). House prices are measured at the zip code level. All specifications include household $\times$ zip code fixed effects and product category $\times$ quarter fixed effects. In columns 2, 3, 5, 6, 8, 9, 11, and 12 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).

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Table A8: Effect of House Prices on Shopping Behavior - Include MSA × Quarter Fixed Effects

Dependent Variable:	log(Expenditure)		Share "Deal"		Share Generic		Share Coupon	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(House Prices)	-0.017	0.371***	0.027***	0.060**	0.002	-0.024*	0.012***	0.028*
	(0.021)	(0.077)	(0.007)	(0.024)	(0.004)	(0.014)	(0.004)	(0.015)
$\mathbb{1}_{Homeowner}$	-0.188**	-0.224***	0.081***	0.063**	0.040***	0.037**	0.050***	0.029**
	(0.077)	(0.080)	(0.027)	(0.028)	(0.014)	(0.015)	(0.013)	(0.014)
log(House Prices) $\times \mathbb{1}_{Homeowner}$	0.043***	0.049***	-0.016***	-0.013**	-0.008***	-0.007**	-0.009***	-0.005*
	(0.015)	(0.016)	(0.005)	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)
MSA × Quarter Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Household × Zip Code Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Household Characteristics × log(House Price)	.	✓	.	✓	.	✓	.	✓
R-squared	0.728	0.729	0.876	0.876	0.747	0.748	0.776	0.776
$\bar{y}$	6.698	6.698	0.277	0.277	0.176	0.176	0.077	0.077
N	839,176	839,176	839,176	839,176	839,176	839,176	839,176	839,176

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 1-2), the expenditure share of products that are on sale (columns 3-4), the expenditure share of generic products (columns 5-6), and the expenditure share of products purchased with a coupon (columns 7-8). House prices are measured at the zip code level. All specifications include household × zip code fixed effects, and MSA × quarter fixed effects. In columns 2, 4, 6, and 8, we additionally control for the following household characteristics interacted with log house prices: household income, age of the head of the household, race of head of household, marital status of head of household, education level of head of household. Each observation is weighted by the household sampling weight. Standard errors are clustered at the zip code × quarter level. Significance levels: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

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

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

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

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

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

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

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

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