Online Appendix For

Housing Wealth Effects: The Long View

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A Data

A.1 Data Construction Details

Our main data sources are the Quarterly Census of Employment and Wages (QCEW) from 1975 to 2017, the County Business Patterns (CBP) from 1975 to 2016, the 1970-2010 post-Censal county population estimates, the 2010-2017 inter-Censal county population estimates, and the Freddie Mac House Price Indices. We also merge in data from a number of other data sources for controls. Throughout, we use consistent CBSA definitions and assign each CBSA to a census region based on the majority of its population in the 2000 Census.

The QCEW data provides county-level data for each SIC industry from 1975-2000 and for each NAICS industry from 1990-2017 and is publicly available on the BLS website. The QCEW is sometimes missing data due to BLS disclosure requirements, as described by the BLS at: https://www.bls.gov/osmr/pdf/st040100.pdf. This missing data problem primarily affects smaller and more narrowly-defined industries in smaller counties where there are few enough employers within an industry that the BLS's disclosure criteria are not met. In our baseline analysis, we only use data for counties within a CBSA that have no missing data for the industry in question (either retail or manufacturing) over the whole sample. In practice, this means that we drop a few small outlying counties for a few CBSAs along with the entire Dover, Delaware CBSA. We show in Appendix D.1.3 that dropping these counties for the entire sample does not affect the results because they are so small.

The QCEW reports monthly employment and aggregate wages by industry. We take quarterly averages and use employment for retail or manufacturing employment. We also use the total wage bill for all employees. We merge in county-level population as estimated annually by the Census, linearly interpolating to quarters. We then drop counties with missing data at any point in the sample, aggregate both employment in each industry and population to the CBSA level, and calculate log changes in employment per capita. As mentioned in the text, the QCEW begins in 1975 but there are data issues the first two years. The log changes we use thus begin in 1978 with the difference between 1978 and 1977 log employment per capita. We create average wages by dividing total wages by total employment. We then clean the data by removing observations where we observe an unusually-large jump in employment or employment per capita over a single quarter. We do so because this likely reflects changes such as county realignments or a large employer being recategorized across industries rather than an actual changes in employment. Appendix D.1.3 shows our results are not sensitive to this data cleaning.

The shift from SIC to NAICS changed the definitions of retail, manufacturing, construction, and real estate employment. For instance, wholesale employment was included as part of retail under SIC but separated into its own sector under NAICS. This causes discrete jumps in sectoral employment. However, for the 1990-2000 period where the BLS provides both SIC and NAICS data, the two series are almost the same in log changes. We thus splice together the SIC and NAICS data in Q1 1993. We choose Q1 1993 because this is the first date for which we can splice together one year and three year log changes. In Appendix D.1.3, we show our results are robust to splice date

We also use a number of additional variables in constructing controls. We merge in 2-digit industry shares from the CBP. We use the CBP rather than the QCEW for this because whereas the QCEW simply omits data when the BLS cannot disclose data, the Census provides employment ranges for industries that do not satisfy this, which include some 2-digit industries. Because the CBP data do not provide an overlapping period for SIC and NAICS, we deal with the SIC to NAICS transition by harmonizing all of the data to consistent 2-digit industry classifications using an algorithm developed by Acemoglu et al. (2016). We then aggregate to the CBSA level and create 2-digit industry shares. Because the CBP data are only available through 2016, we assume that industry shares are constant from 2016 to 2017. Our results are not dependent on this one year. Since the CBP data are annual, we linearly interpolate to quarters to get a 2-digit industry share series by quarter. In a robustness check, we also use the annual CBP data rather than the quarterly QCEW data for the analysis.

We then merge our data set with the Freddie Mac House Price Index. We convert it to a real index using the GDP deflator downloaded from FRED. In a robustness check, we use a proprietary house price index from CoreLogic which uses only transactions but has less time coverage.

We then merge in the Gilchrist and Zakrajsek (2012) excess bond premium. GZ give two different time series on their website. We use the "ebp_oa" measure. This is the excess bond premium which subtracts a fitted value for "distance to default" from options. We also merge in the real 30-year mortgage rate, which we create by taking the average 30-year fixed mortgage rate from FRED and adjusting for inflation using 1-year inflation measured by the BEA's GDP deflator.

We create the regional log changes in employment per capita and house prices, and subtract off the log change in the GDP deflator to get real house prices. We do so by using the average log change in employment per capita or house prices weighting by 2000 population and leaving out each individual CBSA from the calculation of the aggregate.

Finally, we merge in the Saiz housing supply elasticities. We do so by matching the central city with Saiz, who uses older MSA definitions. In some cases, two CBSAs are assigned to the same Saiz MSA. All results are robust to dropping the second match.

A.2 Regional Home Price Indexes

Figure A.1 shows time series plots of the annual log change in housing prices for the United States as a whole and each of the four Census regions we use as the regional aggregates for our empirical approach. One can see that prior to the 2000s boom and bust, the national house price index exhibited relative small fluctuations. However, there were regional house price cycles. In particular, there was a small bust in the Midwest in the early 1980s, a boom and busts in the Northeast from the mid-1980s to the mid-1990s, and a boom and bust in the West in the early 1990s. We use this variation to help us identify the housing wealth effect prior to 2000.

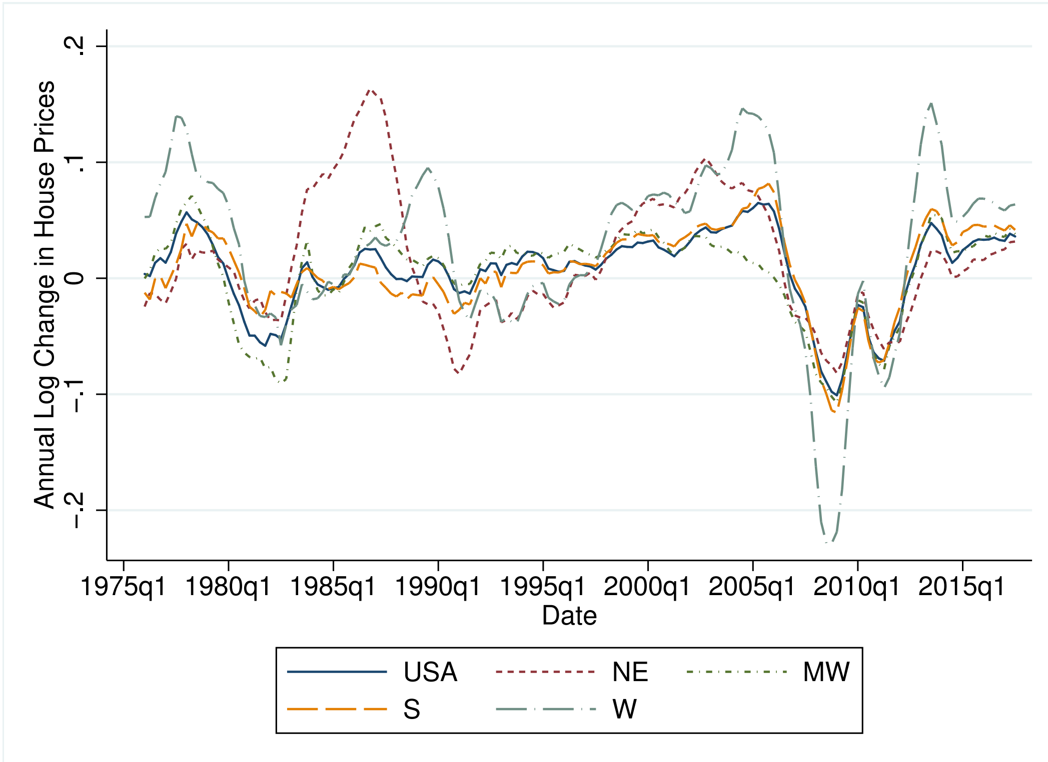

Figure A.1: Regional House Price Indexes

Note: This figure presents regional house price indexes for each of the Census regions we use in our analysis.

A.3 Cross-City Evidence on Retail Employment vs. Consumption

In this section, we provide additional evidence on the relationship between retail employment and consumer expenditures beyond the aggregate evidence presented in the main text by studying the relationship between city-level consumption and retail employment using data for 17 cities for which the BLS publishes city-level average consumption per person using data from the Consumer Expenditure Survey (CEX). Since the aggregated city-level CEX data are available in two-year averages back to 1986 (e.g., 1986-1987, 1987-1988, and so on), so we construct both the left-hand side and right-hand side variables as 2-year log differences of the 2-year averaged data.

We estimate:

$$\Delta \log \bar{C}_{i,t} = \xi_i + \zeta_t + \beta \Delta \log \bar{Y}_{i,t} + \varepsilon_{i,t}, \tag{1}$$

where $\Delta \log \bar{C}_{i,t}$ is the 2-year log change of 2-year averaged consumption, $\Delta \log \bar{Y}_{i,t}$ is the 2-year log change of 2-year averaged retail employment, and ξ_i and ζ_t are city and time fixed effects respectively. We estimate the equation over the sample period 1986-2014. To construct real consumption, we deflate consumer expenditures from the CEX by the city-level CPI, which is available from the

Table A.1: City-Level Consumption vs. Retail Employment Regressions

	1	2	3	4
	OLS Total Cons	IV Total Cons	OLS Ex Imputed Rent	IV Ex Imputed Rent
Retail Emp. Per Capita Growth	0.454** (0.164)	0.947** (0.297)	0.512* (0.210)	0.967** (0.378)
CBSA FE	X	X	X	X
Time FE	X	X	X	X
N	423	408	423	408

Note: This table shows regressions of the elasticity of growth total consumption and total consumption excluding imputed rent to retail employment growth estimated from equation (1). The analysis uses 17 CBSAS for which the Consumer Expenditure Survey CBSA-level data is available. Because the aggregated city-level CEX data are available in two-year averages back to 1986 (e.g., 1986-1987, 1987-1988 and so on), so we construct both the left-hand side and right-hand side variables as 2-year log differences of the 2-year averaged data. Consumption is deflated by the city-level CPI. The IV specification instruments with 3-year log changes in house price growth as indicated in the text.

BLS for the cities we consider at an annual frequency.

Table A.1 presents the results of this analysis. We estimate this equation both with total consumption as the dependent variable (columns 1-2) and total consumption excluding imputed rent (columns 3-4). We first present estimates using OLS. The OLS estimates (column 1 and 3) show that a 1% increase in retail employment is associated with 0.45-0.55% in total consumption both including and excluding imputed rent.

However, an important issue in estimating this equation is that the right-hand side variable (retail employment growth) is measured with substantial error. Hence, the OLS estimate is likely to be biased downward due to attenuation bias. To account for this, we also present results based on an IV estimation strategy where we instrument for retail employment using city-level house price growth. Since the consumption and employment growth rates are for 2-year log changes in 2-year averages (i.e., the growth rate between say 1997/1996 and 1994/1995) we make use of 3-year log changes in house prices as the instrument (i.e., the house price growth from 1994 to 1997 in this example). It is important to recognize that we are not making any assumption here about whether variation in the house price index is endogenous or exogenous to business cycle shocks — only that the measurement error in house price indexes is likely to be orthogonal to the measurement error in employment growth (because the two statistics are calculated from entirely separate samples).

Our IV estimates of the relationship between retail employment and consumer expenditures are presented in in columns 2 and 4. A 1% increase in city-level retail employment is associated with roughly a 1% increase in city-level consumption both including and excluding imputed rent. Both the CEX and retail employment presumably provide a noisy measure of true consumer expenditures.

Hence, it is not surprising that the IV estimates are higher than their OLS counterparts (as we would expect in the presence of attenuation bias). However, the IV analysis suggests that once we account for measurement error, retail employment per capita varies roughly one-for-one with real consumption. This is consistent with the aggregate evidence we present in the text on the time series behavior of aggregate consumption measures and retail employment.

The IV coefficient presented in the table above gives the ratio of the covariances of retail employment with house prices, and consumer expenditures with house prices, and hence provides insight into the relative responsiveness of consumer expenditures relative to retail employment associated with house price shocks. The comovement between retail employment and consumer expenditures may differ in response to other sources of variation, aside from house price shocks. However, since our analysis focuses on how retail employment responds to changes in house prices, we believe that the IV estimates we report above are the "right" kind of variation to focus on for the purpose of our analysis.

B Calibration and Numerical Methods

B.1 Model Income Process

We use an income process that captures salient features of the earnings dynamics reported in Guvenen et al. (2016), hereafter GKOS. Specifically we model log annual income as $\log y = \ell + z + \xi$, where ℓ is a deterministic life-cycle component, z is a persistent shock that follows an AR(1) process, and ξ is a transitory shock. The deterministic life-cycle is from Figure 3 of the July 2015 version of GKOS. GKOS model the transitory income shocks as a "non-employment shock" and we mimic this specification. The transitory component ξ is zero with some probability and is equal to $\log(1 - x)$ with complementary probability, where x is drawn from an exponential distribution on the interval $(0, 1)$. We use data from the 2002 March CPS on hours worked in the prior year to estimate the parameter of the exponential distribution to be 2.25 and the probability that ξ is zero to be 0.75 using maximum likelihood. We fix the persistence of the AR(1) component z to 0.97 because a near unit-root persistence is needed to match the near linear growth of the cross-sectional earnings variance over the life-cycle. The innovations to z are drawn from a mixture of two normals, which allows us to capture the leptokurtic nature of earnings growth rates reported by GKOS. We fix the mean of the mixture components to zero and estimate the mixture probability and the standard deviations using a simulated method of moments procedure. Table A.2 lists the target moments and

Table A.2: Data Targets and Model Moments for Income Risk

Moment	Data	Model
St. dev. of 1-year log earnings growth	0.51	0.63
St. dev. of 5-year log earnings growth	0.78	0.76
Growth of cross-sectional variance of log earnings over the life-cycle	0.66	0.66
Fraction of 1-year log earnings growth in [-1.0,1.0]	0.94	0.92
Fraction of 1-year log earnings growth in [-0.1,0.1]	0.53	0.52
Fraction of 5-year log earnings growth in [-1.0,1.0]	0.90	0.89
Fraction of 5-year log earnings growth in [-0.1,0.1]	0.27	0.27

the model-implied values. All empirical moments are taken from GKOS. The resulting parameters of the innovations to z are a first component with a mixture weight of 0.984 and a standard deviation of 0.071 and a second mixture component with a weight of 0.016 and a standard deviation of 1.60.

The data used by GKOS is on earnings before taxes. We use the "log" tax function estimated by Guner et al. (2014) for all households to approximate the US tax system including state and local taxes, which states that the average tax rate is $0.135 + 0.062(Y/\bar{Y})$.

B.2 Model Calibration

We next describe the procedure we use to calibrate the discount factor, β; the strength of the preference for housing, ω; the strength of the bequest motive, B_0; the degree to which a bequest is a luxury, B_1; the rent-price ratio, δ; the mortgage origination cost, ψ^M; and the transaction cost for selling a house, ψ^{Sell}. The method we use is to minimize a quadratic objective function. We begin by describing our empirical targets, then discuss the objective function, and conclude with an assessment of the model's fit.

The broad overview of our empirical targets is provided in the main text and here we provide some additional information. Starting with the 2001 SCF, we first compute home-value-to-income for households with heads aged 25 to 60. We compute this ratio as the value of real estate held to household income. Next we compute LTV as the sum of all housing debt relative to the value of all real estate. Liquid assets are defined as the sum of liquid accounts ("liq" in the SCF extracts sums checking, savings, and money market accounts), directly held mutual funds, stocks, and bonds less revolving debt. Following Kaplan et al. (2014), liquid account holdings are scaled by 1.05 to reflect cash holdings. We normalize the model and the data such that median income among 40 year-olds is 1.0. To compute life-cycle profiles, we use rolling 5-year windows by age (i.e., moments at age 30 include heads 28 to 32 years old).

Table A.3: Reasons for Moving in 2001 March CPS

Reason for moving	Frequency	Included
Did not move	93.63	
Change in marital status	0.38	no
To establish own household	0.41	no
Other family reason	0.57	no
New job or job transfer	0.63	no
To look for work or lost job	0.03	no
To be closer to work/easier	0.14	no
Retired	0.11	no
Other job-related reason	0.06	no
Wanted to own home, not rent	1.9	yes
Wanted new or better house	1.12	yes
Wanted better neighborhood	0.23	no
Cheaper housing	0.11	yes
Other housing reason	0.44	no
Attend/leave college	0.04	no
Change of climate	0.08	no
Health reasons	0.06	no

We now turn to our refinancing frequency target. Deng et al. (2000) estimate a statistical model of refinancing behavior that controls for the difference between existing mortgage interest rates and the market rate. Their model allows for unobserved heterogeneity with three household types who differ in the propensity to refinance. Their Figure 2 reports the time-varying refinancing rate for each of the groups. Using this information and the relative sizes of the groups estimated by Deng et al., we simulate a population of households and compute the fraction of mortgages refinanced each year, which yields 9.3 percent.

Lastly, we target a 3.2 percent moving rate for owner occupiers based on March 2001 CPS data. Overall, 6.3 percent of owner occupiers reported living in a different house one year earlier. The CPS asks for the reason for the move and many of the movers report moving for reasons that are outside of the scope of our model. Table A.3 lists the reasons-for-moving responses, whether we included or excluded these moves, and the frequency of these responses.

Our quadratic objective function is constructed as follows. For life-cycle profile targets we average the squared difference between the model and the data over the life-cycle. We normalize the weight of the LTV target to 1.0. The weights for liquid assets, home-to-income, homeownership rate, the aggregate refinancing rate, and the aggregate moving rate are 0.25, 1, 25, 500, and 250, respectively.

Figures A.2 to A.5 show the model's fit to the marginal distributions of liquid assets, LTV,

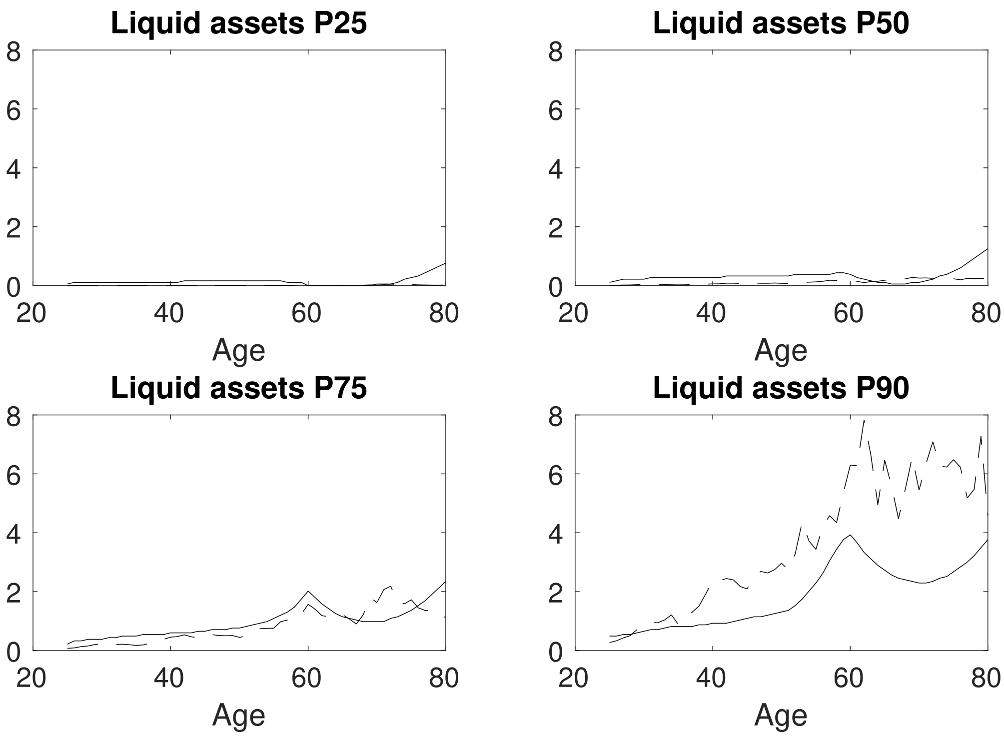

Figure A.2: Liquid Asset Holdings By Age for Model (Solid) and Data (Dashed)

Note: Data refer to SCF 2001.

housing values and homeownership across ages. The calibrated model predicts a refinancing rate of 8.6% and a moving rate of 2.9%.

B.3 Constructing the Distribution of Idiosyncratic States from SCF and CoreLogic Data

Our model has five state variables: cash on hand, mortgage debt, housing position, persistent income, and age. We create analogous variables using each wave of the SCF from 1983 to 2016. We equate cash on hand to liquid assets plus annual wage income, where liquid assets are defined as in Section B.2. Wage income is set to X5702 + X5704 + X5716 + X5718 + X5720 + X5722 in the SCF, which is the sum of income from wages/salaries, sole proprietorships, unemployment insurance and workers' compensation, child support and alimony, welfare assistance, Social Security, or other pensions. We remove taxes from income in the same manner as described in Section B.1. Mortgage debt is set to the sum of all loans backed by housing, which includes home equity lines of credit when this information is available (1989 and onwards). Our model is written in terms of physical units of housing, $h_{i,t}$, that trade at price p_t per unit. We define a unit of housing as a dollar of housing in 2001. We deflate the value of housing based on the evolution of the FHFA national price

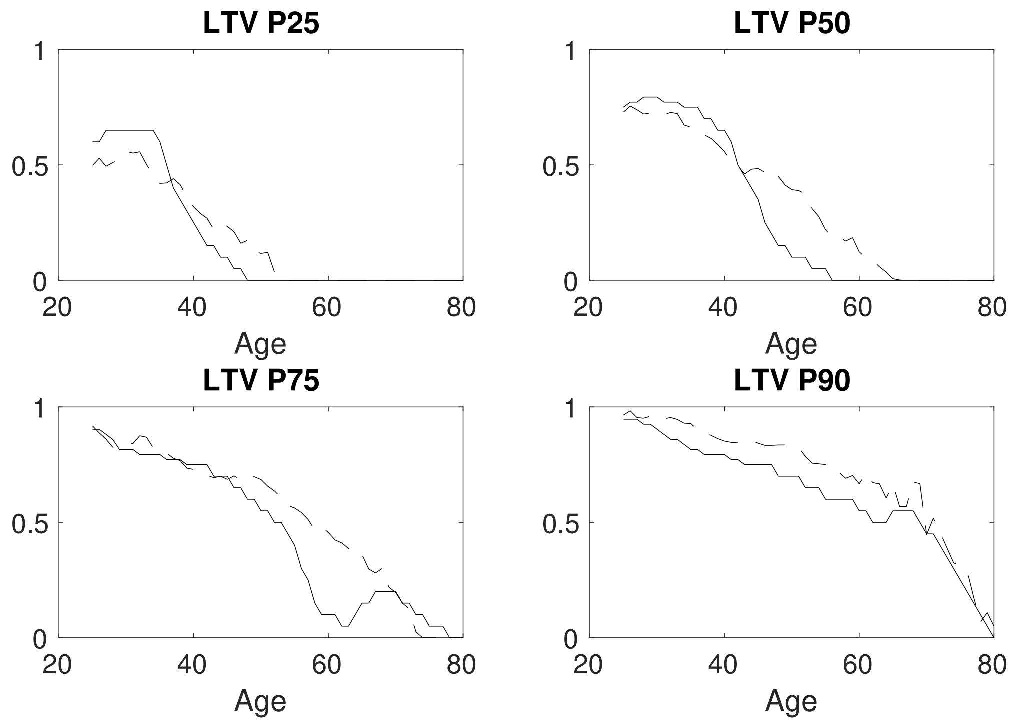

Figure A.3: LTV By Age for Model (Solid) and Data (Dashed)

Note: Data refer to SCF 2001.

index relative to the trend of disposable income per capita. We use disposable income from the BEA's Personal Income and Outlays release and divide by the civilian non-institutional population reported by the BLS. We smooth the log of the quarterly series with the HP filter with coefficient 1600 and time aggregate to annual observations. Normalizing by disposable income per capita is a simple way of adjusting for changes in nominal income and rendering the price index roughly stationary. We set the persistent income state based on wage income. Finally, age is simply the age of the household head. We create a product grid on the state space and allocate the mass of the SCF observations to the grid points in a manner that preserves the means of variables.

We are concerned that the SCF may understate the decline in home values during the Great Recession. The Flow of Funds reports a 24% drop in the nominal value of owner occupied real estate between 2007 and 2010. Similarly, the FHFA expanded data house price index reports a 21% drop in house prices, which is similar to other repeat sales house price indices. By contrast, the drop in the SCF is only 14%. It may be that households are slow to recognize (or admit) that the value of their homes has fallen, leading to systematic misreporting in the SCF during the housing bust in the Great Recession.

To address this concern, we use data from CoreLogic's Homeowner Equity Reports to adjust the

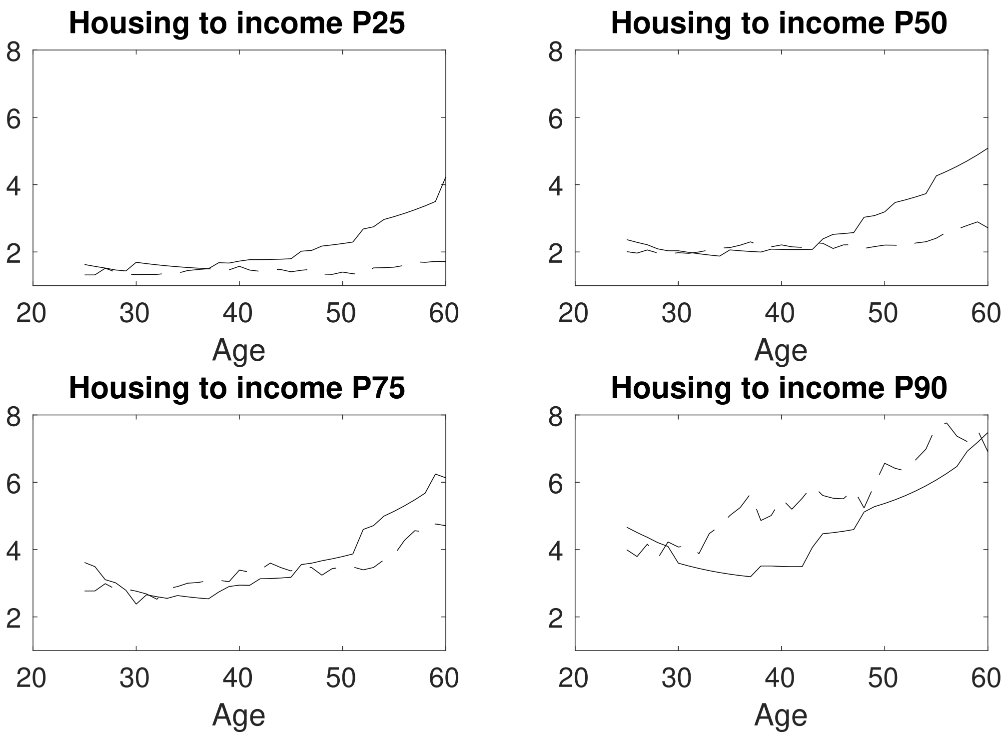

Figure A.4: Housing-to-Income By Age for Model (Solid) and Data (Dashed)
Note: Data refer to SCF 2001. Sample restricted to working-age homeowners.

SCF home values to match CoreLogic's estimated distribution of equity. Since 2007, CoreLogic has reported the CDF of the nationwide LTV distribution at a given set of percentiles. For example, the fraction of households with a mortgage with LTVs less than 50%, 50% to 55%, 55% to 60%, and so on. Starting from this information we construct a marginal distribution of LTVs. To do so we use the SCF to determine the share of all households with a mortgage and the conditional distribution of LTVs within the 0 to 50% bin. We then linearly interpolate the CoreLogic CDF within these 5 percentage point intervals. In making this adjustment, we maintain the order of households in the LTV distribution, but change the values of the LTVs to match the marginal from CoreLogic.

For our calculations, we need to know the mortgage balance and home value separately. We assume that the SCF values for mortgage balances are correct and adjust the home values to match the LTV calculated in the previous step. This reflects the fact that most households can easily look up their mortgage balance during their SCF interview but cannot easily establish their house's market value. Similarly, we assume that the share of homeowners with a mortgage in the SCF is correct in making our adjustment.

As described in footnote 31, CoreLogic changed its methodology in 2010 to better account for

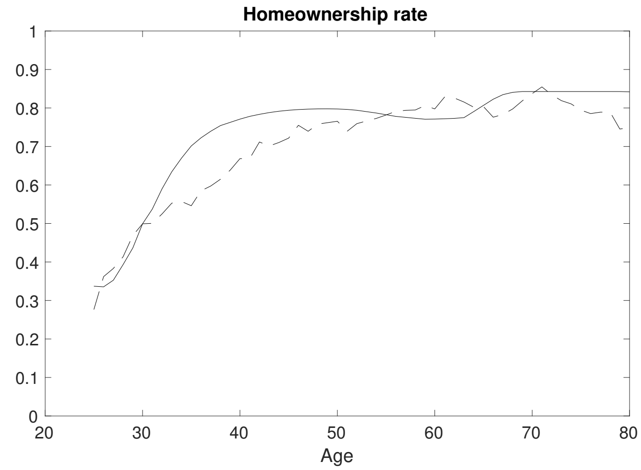

Figure A.5: Homeownership By Age for Model (Solid) and Data (Dashed)
Note: Data refer to SCF 2001.

loan amortization and HELOC draw-down. This led to a reduction in their estimated share of homeowners who are underwater on their loan. We use the old methodology for 2007 when the new methodology is not available and the new methodology for 2013 when the old methodology is not available. For 2010, when both are available, we report results with both, although our baseline results use the new methodology.

B.4 Value Functions and Model Solution

The household's problem can be written as follows. If a household buys a home it solves:

$$V^H(w,z,a,p) = \max_{c,m',h',A'} \left\{u(c,h') + \beta \mathbb{E}\left[V(\ell',h',m',z',a+1,p)\right]\right\},$$

where w is wealth defined below, z is the persistent income shock, a is age, c is consumption, h is housing, m is mortgage debt, A is liquid savings, ℓ is liquid cash on hand. This maximization problem is subject to the LTV constraint:

$$c + A' - \left(1-\psi^{\text{m}}\right) m' + (1+\psi^{\text{Buy}})ph' = w,$$

where w is defined as:

$$w = (1-\psi^{\text{Sell}})ph - R_m m + \ell.$$

In this Appendix we will use the notation $R_m m$ to denote the balance due on the outstanding mortgage including interest, but this is shorthand for a more complicated expression that reflects the fact that the interest rate on the mortgage depends on the LTV at origination. Liquid cash on hand is determined according to:

$$\ell = R_A A + y.$$

Households that previously rented have $h = m = 0$. If a household refinances its mortgage it solves:

$$V^m(f, h, z, a, p) = \max_{c,m',A'} \left\{ u(c, h) + \beta \mathbb{E}\left[V(\ell', h, m', z', a+1, p) \right] \right\},$$

subject to the LTV constraint and:

$$c + A' - (1 - \psi^m)\, m' = f,$$

where f is financial wealth defined as:

$$f = \ell - R_m m.$$

If an owner-occupier household neither refinances nor sells its house it solves:

$$V^0(\ell, h, m, z, a, p) = \max_{c,A'} \left\{ u(c, h) + \beta \mathbb{E}\left[V(\ell', h, G(a)m, z', a+1, p) \right] \right\},$$

subject to:

$$c + A' - m' = \ell - R_m m.$$

A renter solves:

$$V^R(w, z, a, p) = \max_{c,h',A'} \left\{ u(c, h') + \beta \mathbb{E}\left[V(\ell', 0, 0, z', a+1, p) \right] \right\}.$$

Entering the next period, the household has a discrete choice over the adjustment costs:

$$\begin{aligned} V(\ell, h, m, z, a) = \max \Big\{ & V^H \Big(((1 - \psi_H^{\text{Sell}})ph - R_m m + \ell, z, a, p \Big), \\ & V^m \left(\ell - R_m m, h, z, a, p \right), \\ & V^0(\ell, h, m, z, a, p), \\ & V^R \Big((1 - \psi_H^{\text{sell}} - \delta)ph + \ell - R_m m, z, a, p \Big), \\ & V^D \left(z, a, p \right) \Big\}, \end{aligned}$$

with V^m and V^0 unavailable to households that previously rented. $V^D(z, a, p) = V^R(\epsilon, z, a, p) - \phi$ is an option to default on the mortgage, which leaves the household as a renter with a small liquid asset position ϵ and incurs a utility cost ϕ. Defaults play very little role in our analysis (we set $\phi = 4$ and homeowners are loathe to default), but it is useful to allow this option for homeowners without alternatives.

We solve the household's problem using value function iteration. In solving the model we place a grid on LTV as opposed to mortgage debt. We also specify grids for wealth, financial wealth, liquid wealth, and income. We allow the household to make continuous choices of consumption, liquid savings, and mortgage debt, but we restrict housing to discrete values. The output of each iteration of our Bellman equation is the value on the grid points for (ℓ, h, m, z, a). The most obvious way of solving this problem is to solve for the optimal actions for each of the discrete adjustment options for each combination (ℓ, h, m, z, a). A more efficient approach makes use of the fact that, for example, all households with a certain level of wealth who buy a house will make the same choice so we can solve the problem on the more compact space of (W, z, a) and then interpolate the value onto (ℓ, h, m, z, a). This works well for the value functions but there is a small complication for the decision rules because the housing quantity choice is discrete and so we cannot easily interpolate the decision rules onto (ℓ, h, m, z, a). To find the decision rules, we cannot avoid solving the problem for the specific combinations of (ℓ, h, m, z, a), but we only need to do this for the households who choose to buy a new house or rent a house and are therefore making a choice over h'. For households who refinance, h' is fixed so there is no problem interpolating the decision rules.

C Empirical Approach

C.1 Empirical Approach in a Structural Simultaneous Equations Framework

This Appendix explains our identification strategy using a simple simultaneous equations econometric framework. It derives the equation we use to create our instrument structurally and formalizes our identification assumption.

Consider the following empirical model for the determination of retail employment and house prices:

$$\Delta y_{i,r,t} = \psi_i + \xi_{r,t} + \beta \Delta p_{i,r,t} + \alpha_i \mathcal{E}_{r,t} + \varepsilon_{i,r,t}, \tag{2}$$

$$\Delta p_{i,r,t} = \varphi_i + \zeta_{r,t} + \delta \Delta y_{i,r,t} + \gamma_i \mathcal{V}_{r,t} + \nu_{i,r,t}. \tag{3}$$

We allow for CBSA fixed effects (ψ_i and φ_i) and region-time fixed effects ($\xi_{r,t}$ and $\zeta_{r,t}$). $\mathcal{V}_{r,t}$ and $\nu_{i,r,t}$ denote regional and idiosyncratic shocks that affect house prices, respectively. $\mathcal{E}_{r,t}$ and $\varepsilon_{i,r,t}$ denote regional and idiosyncratic shocks that affect retail employment, respectively. These shocks should be viewed as vectors of more primitive shocks and may be correlated with each other (e.g., $\mathcal{V}_{r,t}$ and $\mathcal{E}_{r,t}$ may be correlated). Measurement error in retail employment and house prices would show up in this model as a correlation between $\nu_{i,r,t}$ and $\varepsilon_{i,r,t}$. The model allows for heterogeneity in sensitivity to regional shocks across CBSAs within region (the i subscripts on α_i and γ_i). This feature will play an important role. Equation (2) is the analog of equation (1) in the main text and the coefficient of interest is β, the causal effect of house prices on retail employment measured as an elasticity, which we call the housing wealth elasticity.[1]

Equations (2) and (3) form a system of simultaneous equations. Changes in local house prices affect local retail employment through the $\beta\Delta p_{i,r,t}$ term in equation (2). However, changes in local employment also affect local house prices through the $\delta\Delta y_{i,r,t}$ term in equation (3). Since causation between local employment and house prices runs both ways, estimating equation (2) by OLS will yield a biased estimate of β. The classic approach to solving this problem is to look for a variable that shows up in equation (3) but not in equation (2) and to use this variable as an instrument for $\Delta p_{i,r,t}$ when estimating equation (2). In a panel data context, there is another related possibility for identification: differential sensitivity to aggregate shocks.

As we discuss in the main text, a simple implementation of this idea would be to estimate equation (3) and use $z_{i,r,t} = \hat{\gamma}_i \Delta P_{r,t}$ as an instrument for $\Delta p_{i,r,t}$ in equation (2). The simple procedure runs into problems if retail employment responds differentially to regional shocks through other channels than local house prices. Suppose for simplicity that there is no actual variation in the γ_is in equation (3), but that local retail employment does respond differentially to regional shocks through heterogeneity in α_is in equation (2). In this case, the differential response of local retail employment to regional shocks induces differential responses of local house prices to these same shocks through the $\delta\Delta y_{i,r,t}$ term in equation (3). Were we to estimate equation (3) in this case, we would estimate heterogeneous γ_is. The source of these estimated γ_is would, however, be the $\alpha_i \mathcal{E}_{r,t}$ term in equation (2). In this case, therefore, $z_{i,r,t} = \hat{\gamma}_i \Delta P_{r,t}$ would clearly be correlated with $\alpha_i \mathcal{E}_{r,t}$. Intuitively, the differential response of local house prices to regional house prices in this example arises from reverse causation and cannot be used to identify β.

[1] The empirical model also allows for heterogeneity in the sensitivity to idiosyncratic shocks. This feature is captured through heterogeneity in the variances of $\varepsilon_{i,r,t}$ and $\nu_{i,r,t}$ in the cross-section. Because this is less important for our empirical approach, the notation in equation (2) and (3) is not as explicit about this heterogeneity in sensitivity.

To address this problem, consider the following more sophisticated identification strategy. First aggregate equation (3) to the regional level. Since the cross-sectional average of $\nu_{i,r,t}$ is zero, this yields:

$$\Delta P_{r,t} = \zeta_{r,t} + \delta \Delta Y_{r,t} + \gamma_r \mathcal{V}_{r,t},$$

where $\Delta Y_{r,t}$ denotes the log annual change in regional retail employment, and γ_r denotes the regional average of γ_i. Next, use this equation to rewrite equation (3) as:

$$\Delta p_{i,r,t} = \varphi_i + \tilde{\zeta}_{r,t} + \delta \Delta y_{i,r,t} + \frac{\gamma_i}{\gamma_r} \Delta P_{r,t} - \frac{\gamma_i}{\gamma_r} \delta \Delta Y_{r,t} + \nu_{i,r,t}. \tag{4}$$

where $\tilde{\zeta}_{r,t} = \zeta_{r,t}(1 - 1/\gamma_r)$. Estimating this equation yields estimates of each city's relative sensitivity to regional house prices γ_i/γ_r, which we can again denote $\hat{\gamma}_i$. Finally, use $z_{i,r,t} = \hat{\gamma}_i \Delta P_{r,t}$ as an instrument in equation for $\Delta p_{i,r,t}$ in equation (2). The logic for this procedure is similar to the simpler procedure described above, but it has the advantage that it eliminates the reverse causality problem by directly controlling for $\Delta y_{i,r,t}$ and $\Delta Y_{r,t}$ in equation (4). A slightly more general version of equations (2) and (3) allows for heterogeneity in the response of house prices to local employment, which replaces δ in equation (3) with δ_i. In this case, the coefficient on $\Delta y_{i,r,t}$ in equation (4) is δ_i. Our empirical specification in the main text allows for this generalization as well as additional controls.

The identifying assumption implicit in the procedure described above is that $z_{i,r,t}$ is uncorrelated with $\alpha_i \mathcal{E}_{r,t} + \varepsilon_{i,r,t}$, the error term in equation (2). Because we control for $\Delta y_{i,r,t}$ and $\Delta Y_{i,r,t}$ in defining the instrument in equation (4), such a correlation cannot result from reverse causation. Furthermore, purely idiosyncratic variation ($\varepsilon_{i,r,t}$) will not be correlated with $z_{i,r,t}$ either in the time-series or cross-section. The remaining concern is that there is some component of $\alpha_i \mathcal{E}_{r,t}$ — call it $\alpha_i^j \mathcal{E}_{r,t}^j$ — that is correlated with $z_{r,t}$. To be a threat to identification, $\alpha_i^j \mathcal{E}_{r,t}^j$ must have two features. First, $\mathcal{E}_{r,t}^j$ must be correlated with regional house price cycles. Second, α_i^j must be correlated with $\hat{\gamma}_i$ in the cross-section. An assumption that is sufficient to rule out endogeneity of our instrument is therefore that $\alpha_i^j \perp \hat{\gamma}_i$, i.e., that the same CBSAs whose house price indexes are relatively more sensitive to regional house price cycles do not also have local employment that is differentially more sensitive to $\mathcal{E}_{r,t}^j$. With additional controls, these identifying assumptions must only hold conditional on the controls.

Table A.4: Highest and Lowest γ_i CBSAs by Census Region (Pop > 500,000)

Region	Northeast	Midwest	South	West
Lowest	Pittsburgh, PA	Wichita, KS	Greensboro, NC	Albuquerque, NM
	Rochester, NY	Omaha, NE	Greenville, SC	Colorado Springs, CO
	Harrisburg, PA	Indianapolis, IN	Winston-Salem,NC	Salt Lake City, UT
	Buffalo, NY	Columbus, OH	Raleigh, NC	Denver, CO
	Scranton, PA	Youngstown, OH	Jackson, MS	Portland, OR
⋮				
	Worcester, MA	Toledo, OH	Jacksonville, FL	Fresno, CA
	Bridgeport, CT	Milwaukee, WI	Tampa, FL	Sacramento, CA
	New Haven, CT	Chicago, IL	Orlando, FL	Las Vegas, CA
	New York-Newark, NY	Minneapolis, MN	Sarasota, FL	Riverside, CA
Highest	Providence, RI	Detroit, MI	Miami, FL	Stockton, CA

Notes: The table shows the top five and bottom five CBSAs with a population over 500,000 in each census region sorted by γ_i. γ_i is estimated in a single pooled regression that does not leave out any years from 1975 to 2017.

C.2 Additional Details on γ_i Variation Across Cities

Table A.4 shows the top 5 and bottom 5 γ_i cities of over 500,000 population in 2000 for each Census region. As discussed in the main text, many cities that have similar values of the Saiz elasticity but have significantly different values of γ_i, such as Providence and Rochester.

D Empirical Robustness

D.1 Robustness of Rolling Windows Analysis

This section analyzes the robustness of the results, focusing on the rolling windows analysis in Figure 5. We focus on the robustness of the sensitivity instrument because it is most novel and provides for the tightest confidence intervals. The section is organized as follows:

D.1.1 Controls vs. No Controls

D.1.2 First Stage and Reduced Form

D.1.3 Alternate Specifications: First Stage and Reduce Form, 3-Year Differences, Population Weighting, Single Gamma, No Sand States, Pre-Period Gamma, Dropping Nearby Cities for Regional HPIs, Controlling For Distance to Region Centroid, 5-Year Windows, Alternative Controls and Variable Construction

D.1.4 Single Cross Section Results

D.1.5 Alternate House Price and Employment Data

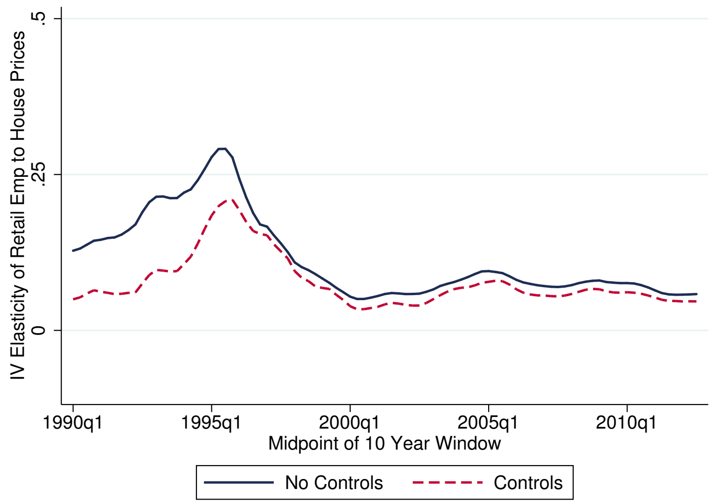

Figure A.6: Elasticity of Retail Employment Per Capita to House Prices: No Controls vs. Controls
Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a in red (dashed) and a version without the controls used in our baseline specification in blue. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer.

D.1.1 The Role of Controls in Our Baseline Specification.

In our baseline specification, we include a number of controls in the estimation of equations (1) and (4). These include controls for differential city-level exposure to regional retail employment, real 30-year mortgage rates, and Gilchirst and Zakrajek's (2012) measure of bond risk premia, controls for 2-digit industry shares with time-specific coefficients, and, in equation (4), the log change in average wages with city-specific coefficients.

To evaluate the role of these controls in our results, Figure A.6 shows the point estimates for the case with controls and the case with no controls (the standard errors do not change significantly). One can see that the elasticity is about 25% bigger without controls in the 10-year windows with their midpoints after 1996. For the 10-year windows with their midpoints prior to 1996, the controls do much more to reduce the estimated elasticity.

Which controls matter the most? In unreported results, we find that the industry shares have the largest effect, although the control for city-level exposure to regional retail employment does also explain a significant portion of the gap between the controls and no controls specifications in the 2000s.

Figure A.7: First Stage and Reduced Form

Note: Panel A plots the first stage and panel B plots the reduce form of Figure 5a. Each point indicates the coefficient for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. The instrumental variables estimator is described in Section 3. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer. The figure reports 95% confidence intervals constructed using two-way clustering by CBSA and time.

D.1.2 First Stage and Reduced Form

Figure A.7 shows the the first stage and the reduced form of the main results in Figure 5a. The instrument is somewhat stronger in the later period, but consistently has an F statistic above 100. The main time series pattern we observe in our IV regression is clearly evident in the reduced form.

D.1.3 Alternate Specifications

In this section, we evaluate several alternate specifications. All of the specifications yield a pattern of declining housing wealth elasticities in rolling windows since 1995. The most important form of variation across specifications is that several specifications yield lower estimates of the elasticities in 10-year windows with their midpoint in the early 1990s than in our baseline analysis (though with large standard errors). This is why we do not put too much emphasis on the 1980s and early 1990s results.

Figure A.8 shows three-year differences rather than one-year differences. The time pattern we find in our main figure remains the same, and the central elasticity is also similar. The main difference is that with three-year differences, the point estimate is slightly lower at the very end of the sample.

Figure A.9 shows results weighting by CBSA population in 2000 rather than unweighted. The time series pattern is similar to the pattern in our baseline analysis. However, the weighted elasticity is higher in the 10-year windows centered in the early 1990s and somewhat lower for the 10-year

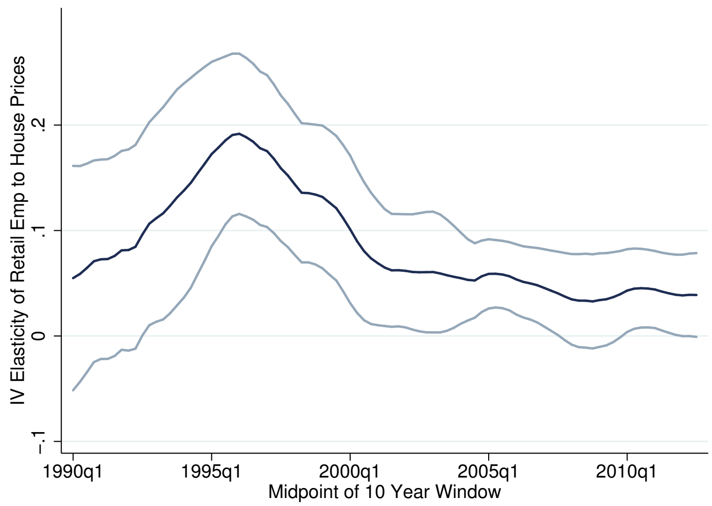

Figure A.8: Elasticity of Retail Employment Per Capita to House Prices: 3-Year Differences

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a using 3-year instead of 1-year log differences for all variables. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. The figure reports 95% confidence intervals in addition to point estimates for the elasticity. The standard errors are constructed using two-way clustering by CBSA and time.

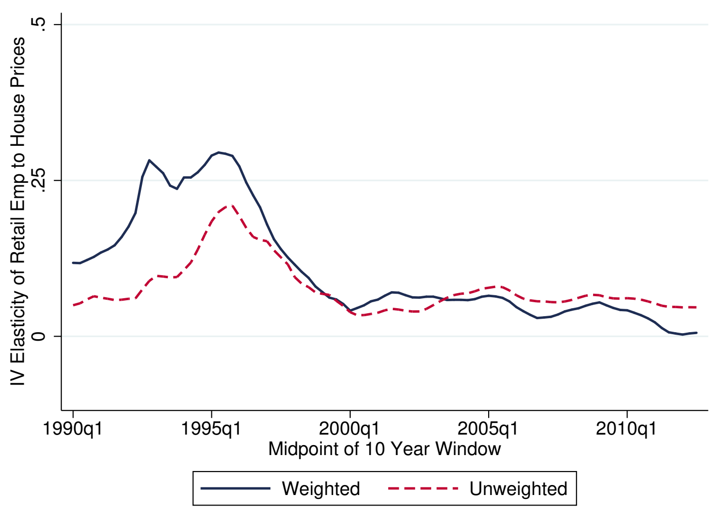

Figure A.9: Elasticity of Retail Employment Per Capita to House Prices: Population Weighting

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a in red (dashed) and a version with all regressions weighted by 2000 population in blue. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer.

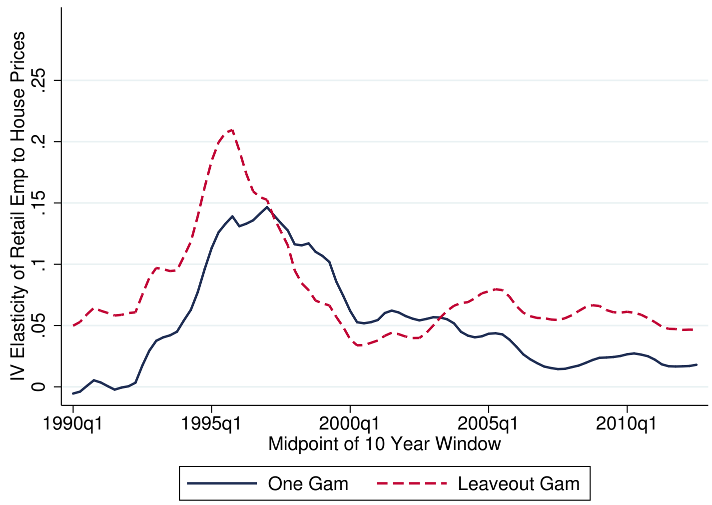

Figure A.10: Elasticity of Retail Employment Per Capita to House Prices: Single Gamma

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a in red (dashed). The specification in blue is the same except that γ_i is estimated for each CBSA by equation (4) once for all periods (including those in the 10-year window) rather than separately for each 10-year window leaving out that 10-year window. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer.

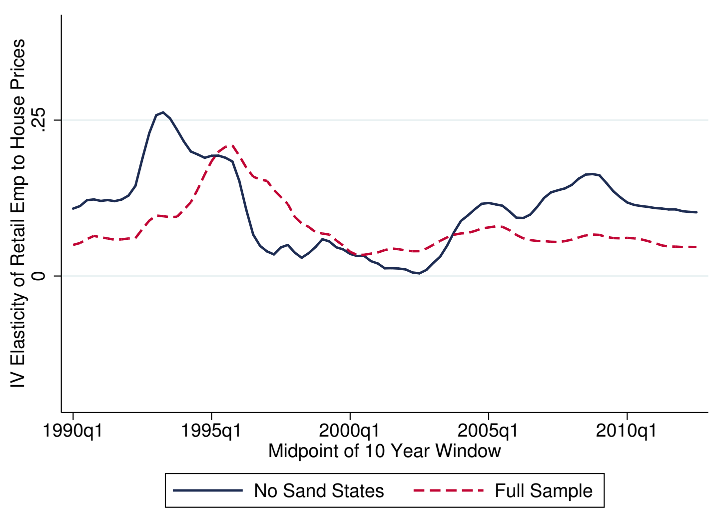

Figure A.11: Elasticity of Retail Employment Per Capita to House Prices: No Sand States

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a in red (dashed) and a version dropping the "sand states" of California, Nevada, Arizona, and Florida in blue. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer.

windows starting in the late 2000s and 2010s.

Figure A.10 shows results that estimate γ_i, the sensitivity of each city to regional house prices, once for all time periods rather than separately for each 10-year window leaving out the periods in that 10-year window. This specification does not therefore incorporate time-variation in γ_i across windows (which may be partly real and partly due to sampling error). Since the 10-year window with its midpoint in 1996, this specification yields a declining elasticity, in line with our baseline analysis. The elasticity is lower both in the early 1990s and in the late 2000s and 2010s than in our baseline analysis.

Figure A.11 shows results dropping the "sand states" of California, Arizona, Nevada, and Florida from the analysis. Critics of the Saiz instrument such as Davidoff (2017) often argue that much of the identification is driven by these states. This figure shows that the declining pattern of elasticities since the mid 1990s is not affected by these states. Indeed, the quantitative results since the mid 1990s are similar whether or not one includes these states.

Figure A.12 shows seven different robustness checks that do not change the results substantially (in light colors) together with the baseline specification (in dark blue). The first specification leaves the data "raw" rather than dropping counties within a CBSA with bad observations in the QCEW and cleaning the data to remove time periods with jumps as described in Appendix A.1. This has essentially no impact on the results. The second specification drops observations with particularly large population changes. Again, this has no impact on the results. The third specification uses a three-year buffer around the 10-year window in constructing the instrument using equation (4) and in constructing the controls for differential city-level exposure to regional retail employment, real 30-year mortgage rates, and the Gilchrist-Zakrajsek excess bond premium using equation (2). This has a slight effect on the results for a few 10-year windows in the mid-to-late 1990s, the mid 2000s, and the 2010s, but the difference is not significant economically or statistically and the main time series pattern remains. Finally, the last three specifications show results that change the date at which we splice together the SIC and NAICS retail employment data from 1993Q1 to 1991Q1, 1996Q1, and 2000Q1, respectively. How we splice together SIC and NAICS also has essentially no impact on the results.

Figure A.13 shows results for a version that uses only periods prior to the 10-year window for the instrument estimation rather than also using periods after the 10-year window. The results are similar, although the standard errors are generally larger earlier in the sample and the point estimates are slightly larger for the 10-year windows with midpoints in the early 1990s.

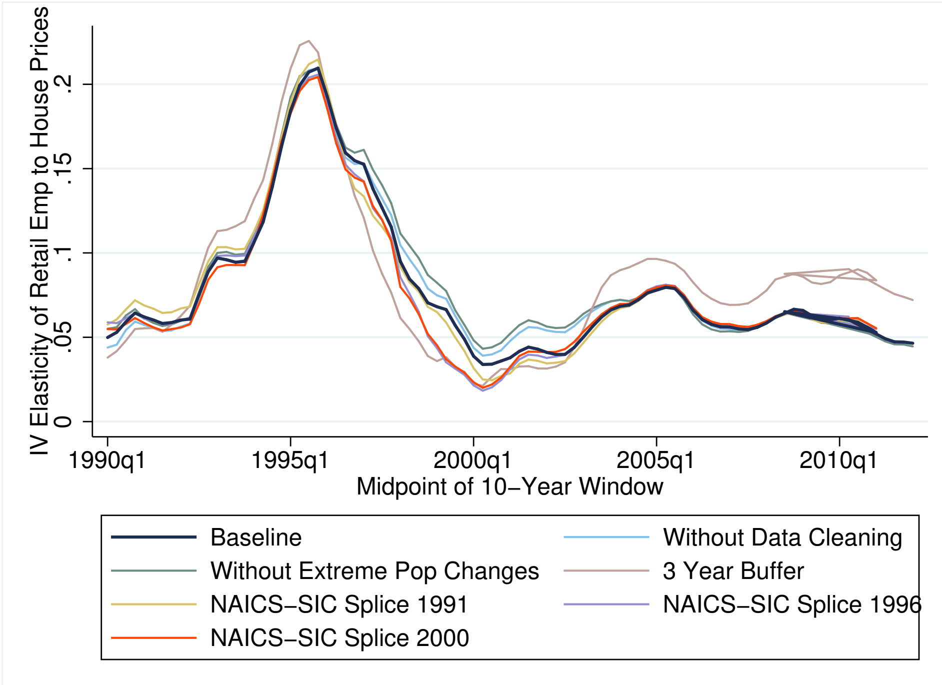

Figure A.12: Elasticity of Retail Employment Per Capita to House Prices: Misc. Robustness Tests

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a in dark blue and with seven other specifications that do not substantially affect the results. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the specifications is clearer. The "without data cleaning" specification does not drop counties that have bad observations in the QCEW and also does not remove periods in which a CBSA has an unusual jump in employment. The "without extreme pop changes" specification drops periods with extreme population changes. The "3 year buffer" specification drops a three year buffer around the 10-year window in question for regression (4) and for the regressions as in equation (2) used to create the controls for differential city-level exposure to regional retail employment, real 30-year mortgage rates, and Gilchirst and Zakrajek's measure of bond risk premia. The "NAICS-SIC Splice 1991," "NAICS-SIC Splice 1996," and "NAICS-SIC Splice 2000" uses these three alternate dates rather than Q1 1993 as the date we use to splice the NAICS and SIC retail employment series together.

Figure A.14 addresses concerns about the regional house price index and employment being driven by nearby cities that share common shocks. Rather than creating the regional index and employment for each city using a leave out mean, this figure drops all cities within 250 miles when creating the regional average. The results are essentially the same.

Figure A.15 addresses concerns that the more sensitive cities may be closer to the economic "core" of a region by including controls for the distance to the centroid of the census region interacted with time fixed effects. The centroid is computed from the 2000 Census population. The results do not change significantly. We have found essentially unchanged results if we define the population centroid based on measures of economic activity rather than population or distance to the largest city in the region.

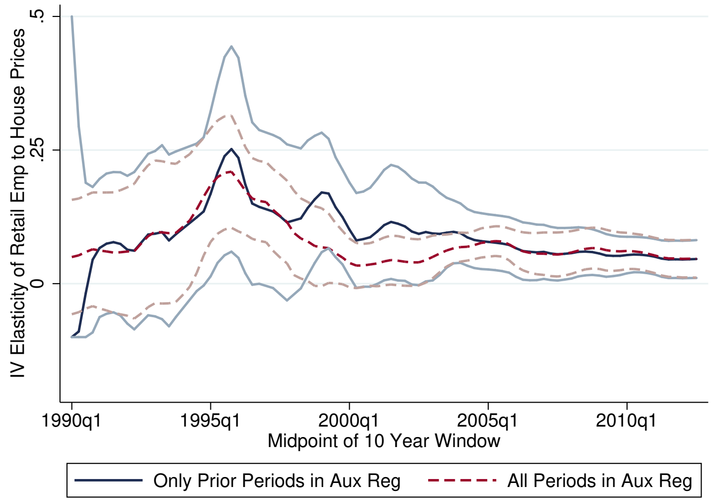

Figure A.13: Elasticity of Retail Employment Per Capita to House Prices: Only Prior Periods To Create Instrument

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a in red (dashed) and a version in which only periods prior to each 10-year window are used to estimate the instrument in blue. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. The standard errors are constructed using two-way clustering by CBSA and time.

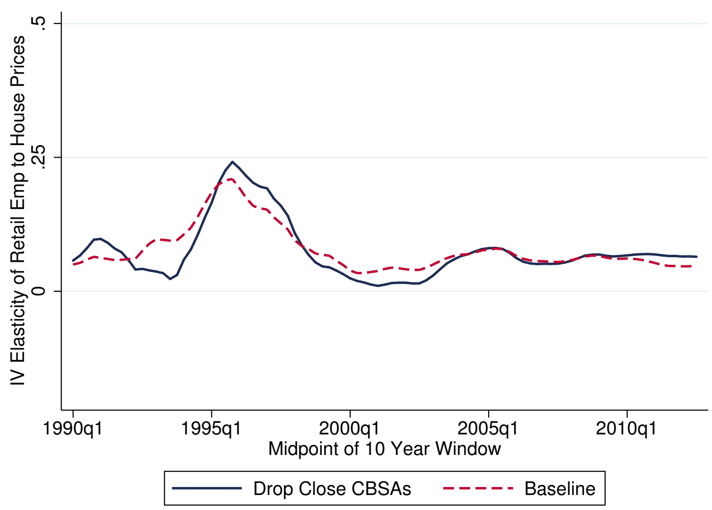

Figure A.14: Elasticity of Retail Employment Per Capita to House Prices: Dropping CBSAs Within 250 Miles For Regional

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a in red (dashed) and a version for which the regional indices used for each CBSA do not include any CBSAs within 250 miles rather than only dropping the CBSA in question in blue. We use an instrumental variables estimator that is described in Section 3. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer.

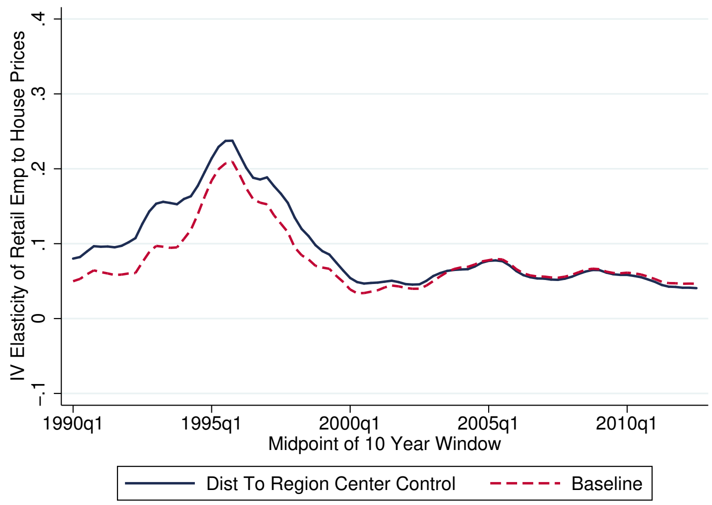

Figure A.15: Elasticity of Retail Employment Per Capita to House Prices: Controlling For Distance to Region's Population Centroid

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a in red (dashed) and a version that controls for the distance between the centroid of the CBSA's principal city and the region's population centroid computed from the 2000 Census interacted with time fixed effects. We use an instrumental variables estimator that is described in Section 3. Each point indicates the elasticity for a 10-year sample period with its midpoint in the quarter indicated on the horizontal axis. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer.

Figure A.16: Elasticity of Retail Employment Per Capita to House Prices: Rolling 5-Year Window

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level as in Figure 5a except for rolling 5-year windows instead of 10-year windows. Each point indicates the elasticity for a 5-year sample period with its midpoint in the quarter indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. The figure reports 95% confidence intervals in addition to point estimates for the elasticity. The standard errors are constructed using two-way clustering by CBSA and time.

Finally, Figure A.16 shows results for 5-year windows This finer resolution does not yield evidence that the elasticity rose significantly during the 2000s. This assuages concerns that by using a 10-year window we are obscuring significant higher-frequency variation. One notable feature of this plot is that the elasticity falls below zero for several years around 2013 before swinging back to positive at the end of the sample, although the standard errors increase significantly in this period.

D.1.4 Single Cross-Section Results

Figure A.17 shows the point estimates and standard errors for repeated 3-year cross-sections of the type that have been used in recent analyses of the Great Recession. Our specification here is the same as our baseline in equation (1) except that we use a single cross section and replace the region-time fixed effect with a region fixed effect. We also demean all variables including controls once for the whole period from 1978-2017.

The results are much more volatile, and the standard errors are sufficiently large in many periods to make the estimates essentially uninformative. These tend to be time periods where the 3-year difference in the regional house price index that is used to construct our instrument is near zero (e.g., a peak or a trough), so the instrument loses power. Aside from these periods, though, one can see a tendency for the housing wealth elasticity to be greater from 1990 to 2003 than it was in the 2000s.

D.1.5 Alternate Data: CoreLogic House Prices and County Business Patterns Employment

Our baseline analysis uses house price data from Freddie Mac. Figures A.18 and A.19 instead use the CoreLogic house price index. The CoreLogic index is an arithmetic repeat sale house price index that has two advantages. First, it includes a broader sample of homes bought with non-conforming loans. Second, it includes only transactions white the Freddie Mac index includes appraisals. Because appraisers tend to look backwards, this would create to a "smoothed" index that would may cause an upward bias in our estimates of the house price elasticity, because we would be observing the same retail employment change for a smaller smoothed change in house prices. The CoreLogic index does, however, have a downside: many CBSAs are imputed from a higher geography prior to 2000. This would create issues with our estimation strategy because it would create an artificial correlation between the house prices in a CBSA and nearby cities that are used to impute the CBSA's house prices. Consequently, we take two approaches to the CoreLogic

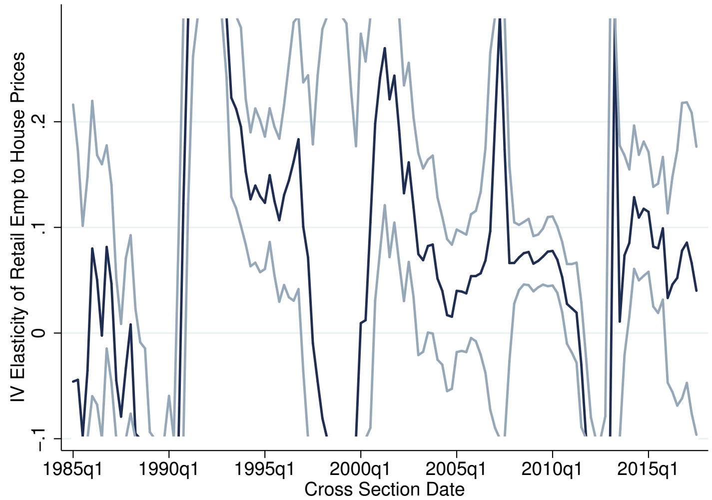

Figure A.17: Repeated Cross-Sections: 3 Year Differences

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for repeated 3-year difference cross sections. Each point indicates the elasticity for a single cross section between the indicated date and three years prior. We use an instrumental variables estimator that is described in Section 3 but with only region FE instead of region-time FE. We take out the CBSA fixed effect (or equivalently demean) once for the full 1978-2017 sample. The figure reports 95% confidence intervals in addition to point estimates for the elasticity. We report robust standard errors.

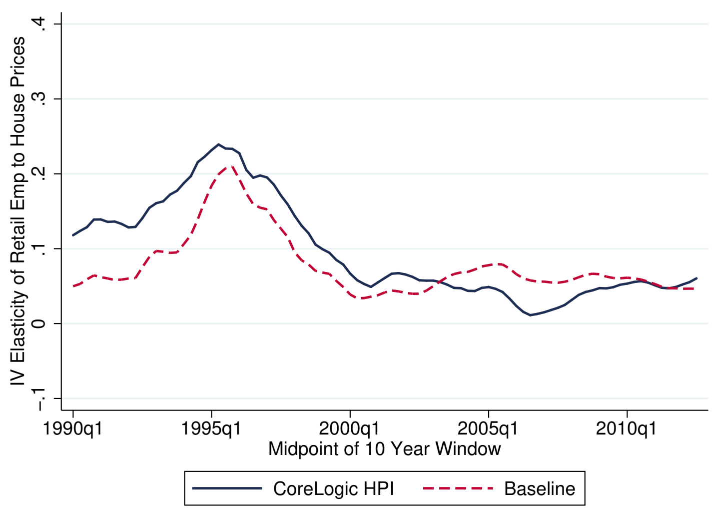

Figure A.18: Alternate Data: CoreLogic House Price Data (Unbalanced Panel)

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a except using the CoreLogic house price index instead of the Freddie Mac house price index. The CoreLogic index does not include appraisals and includes a broader sample of homes purchased with non-conforming mortgages. However, it is not available as far back for every geography. This figure shows results using an unbalanced panel that adds each CBSA as it becomes available. Each point indicates the elasticity for a 10-year sample period with its midpoint in the year indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer.

data. In Figure A.18, we use only the CoreLogic data dropping any imputed observations in an unbalanced panel. In Figure A.19, by contrast, we use the Freddie Mac data in prior to 2000 and the CoreLogic data after 2000, when many fewer house prices are imputed, in a balanced panel. The results of both specifications are similar to our baseline analysis.

Figure A.20 uses County Business Pattern (CBP) employment data rather than QCEW data for retail employment, which has somewhat different sampling frames and industry definitions than our baseline QCEW data. The CBP is only available annually, and so we report annual results rather than quarterly. As a consequence, the standard errors are larger. Nonetheless, the same general time pattern we observe with the QCEW is evident with the CBP.

D.2 Pooled Results: With vs. Without Controls, Weighted vs. Unweighted

Table A.6 presents the results of our pooled regression This section presents results on the robustness of the pooled specifications weighting by 2000 population. Table A.6 presents the same results without controls. The point estimates for the weighted regression are somewhat lower, and the point estimates for the version without controls are somewhat higher. The weighted results show stronger evidence of a boom-bust asymmetry, but the difference remains statistically insignificant.

E Model Extensions and Robustness

This section analyzes the robustness of the model results to various extensions and relaxations of assumptions. It is organized as follows:

- E.1 Linearity and Interaction Effects
- E.2 Changes to β
- E.3 Changes in Credit Constraints
- E.4 Interest Rate Changes
- E.5 Rental Cost of Housing
- E.6 Short-Term Debt
- E.7 Housing Transaction Costs
- E.8 No Short-Run Housing Adjustment

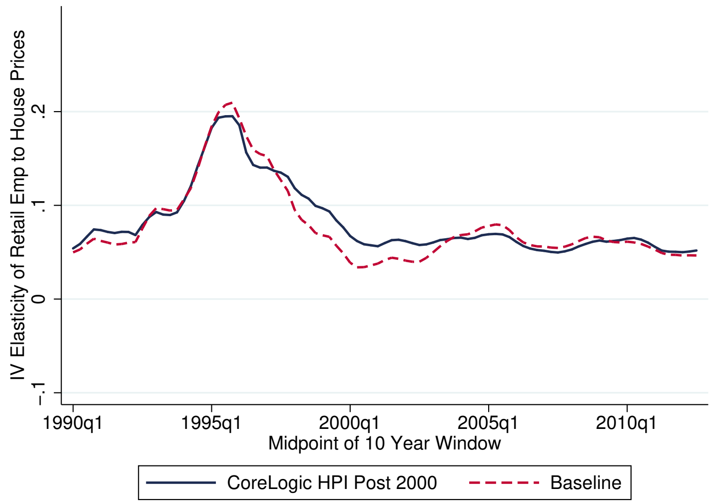

Figure A.19: Alternate Data: Freddie Mac Pre-2000, CoreLogic Post (Balanced Panel)

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a except using the CoreLogic house price index instead of the Freddie Mac house price index after 2000 but uses the Freddie Mac house price index before 2000. This allows us to create a balanced panel. This figure shows results using an unbalanced panel that adds each CBSA as it becomes available. Each point indicates the elasticity for a 10-year sample period with its midpoint in the year indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. Confidence intervals are similar to Figure 5a and are not shown so that the comparison between the two specifications is clearer.

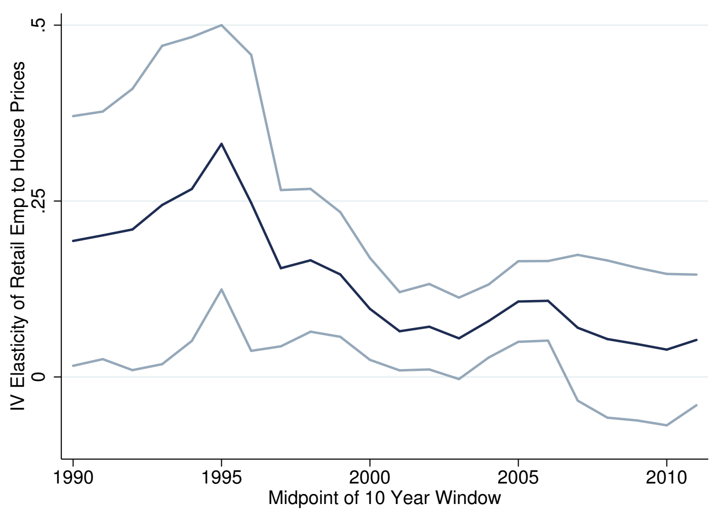

Figure A.20: Alternate Data: County Business Patterns Employment Data

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5a except using County Business Patterns data for retail employment rather than the QCEW. The CBP is available annually, and so the figure is annual. Each point indicates the elasticity for a 10-year sample period with its midpoint in the year indicated on the horizontal axis. We use an instrumental variables estimator that is described in Section 3. The figure reports 95% confidence intervals in addition to point estimates for the elasticity. The standard errors are constructed using two-way clustering by CBSA and time.

Table A.5: 1978-2017 Elasticity of Retail Employment Per Capita to House Prices: Weighted

	(1)	(2)	(3)
$\Delta \log(P)$	0.067**		
	(0.022)		
$\Delta \log(P) -$		0.089**	
		(0.031)	
$\Delta \log(P) +$		0.042	
		(0.025)	
P Test for Equality		0.188	
$\Delta \log(P)$			0.063**
			(0.023)
$\Delta \log(P)^2$			-0.040
			(0.053)

Note: For these estimates, we first construct our instrument for each quarter by estimating the γ_i's in equation (4) for each quarter, using a sample period that leaves out a three-year buffer around the quarter in question. We then estimate equation (1) pooling across all years. Specification 1 does so for all price changes, specification 2 does so by comparing positive and negative house price changes, and specification 3 uses a quadratic in the log change in house prices. For specification 2, we instrument with $Z \times 1[Z \geq 0]$ and $Z \times Z[< 0]$ and for specification 3 we instrument with Z and Z^2. The estimating equation is the same as equation (1) except for $\Delta \log(H)$ being interacted with indicators for $\Delta \log(H) \geq 0$ and $\Delta \log H < 0$ in specification 2 and the addition of the quadratic term in specification 3. Standard errors are two-way clustered at the time and CBSA level. All regressions are weighted by 2000 population.

Table A.6: 1978-2017 Elasticity of Retail Employment Per Capita to House Prices: No Controls

	(1)	(2)	(3)
$\Delta \log(P)$	0.068***		
	(0.015)		
$\Delta \log(P) -$		0.071***	
		(0.023)	
$\Delta \log(P) +$		0.065***	
		(0.020)	
P Test for Equality		0.858	
$\Delta \log(P)$			0.067***
			(0.016)
$\Delta \log(P)^2$			-0.008
			(0.042)

Note: For these estimates, we first construct our instrument for each quarter by estimating the γ_i's in equation (4) for each quarter, using a sample period that leaves out a three-year buffer around the quarter in question. We then estimate equation (1) pooling across all years. Specification 1 does so for all price changes, specification 2 does so by comparing positive and negative house price changes, and specification 3 uses a quadratic in the log change in house prices. For specification 2, we instrument with $Z \times 1[Z \geq 0]$ and $Z \times Z[< 0]$ and for specification 3 we instrument with Z and Z^2. The estimating equation is the same as equation (1) except for $\Delta \log(H)$ being interacted with indicators for $\Delta \log(H) \geq 0$ and $\Delta \log H < 0$ in specification 2 and the addition of the quadratic term in specification 3. Standard errors are two-way clustered at the time and CBSA level. All regressions do not include the standard controls in our baseline specification.

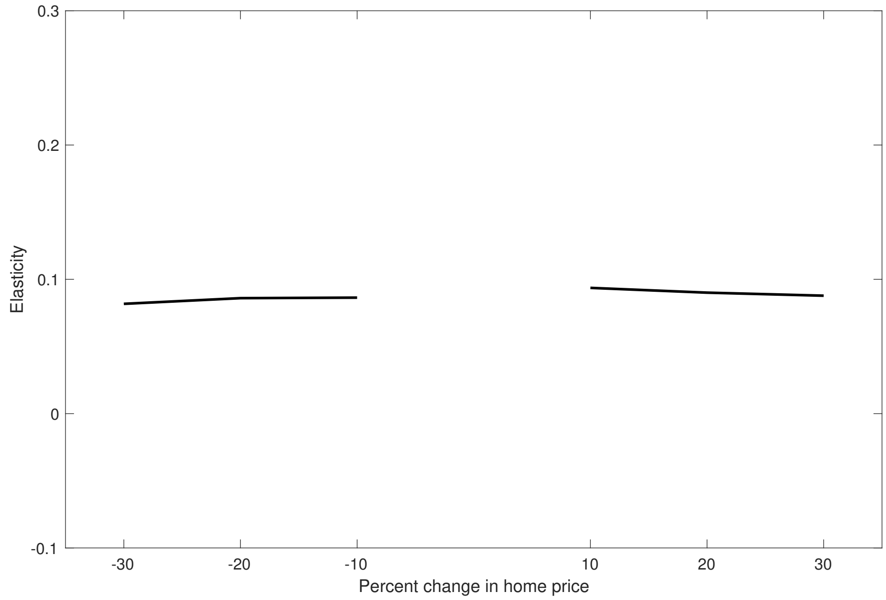

Figure A.21: Linearity of the Housing Wealth Elasticity

Note: The figure shows $(p/C)(dC/dp)$ where dC/dp is computed from a +10% change, a +20% change and so on.

E.9 Accounting for the Evolution of Household Balance Sheets

E.1 Linearity and Interaction Effects

Figure A.21 shows model estimates of the housing wealth elasticity for both positive and negative changes larger than the 10% changes we use in our main analysis. The figure shows that the housing wealth elasticity does not change meaningfully as we change the magnitude of the home price change nor does it show any meaningful asymmetry between positive and negative price changes.

In Section 5, we explain how aggregate shocks are absorbed by the time fixed effects in our empirical specification. In light of this, one way to interpret our theoretical experiments is in terms of two cities with different housing supply elasticities being subjected to an aggregate shock to housing demand. The demand shock itself is absorbed by the fixed effect so we do not model it explicitly and instead focus on the differential reaction of home prices in the two cities. This argument allows us to remain agnostic about the shocks driving home prices. However, the argument assumes that the second-order interaction of home prices and the demand shock does not have important consequences for consumption. Let us next evaluate the validity of this argument for a specific shock to housing demand: an increase in the preference for housing, ω. Figure A.22 shows that the housing wealth elasticity does not change meaningfully as we change ω. This implies that the cross derivative $d^2C/(dpd\omega)$ is small.

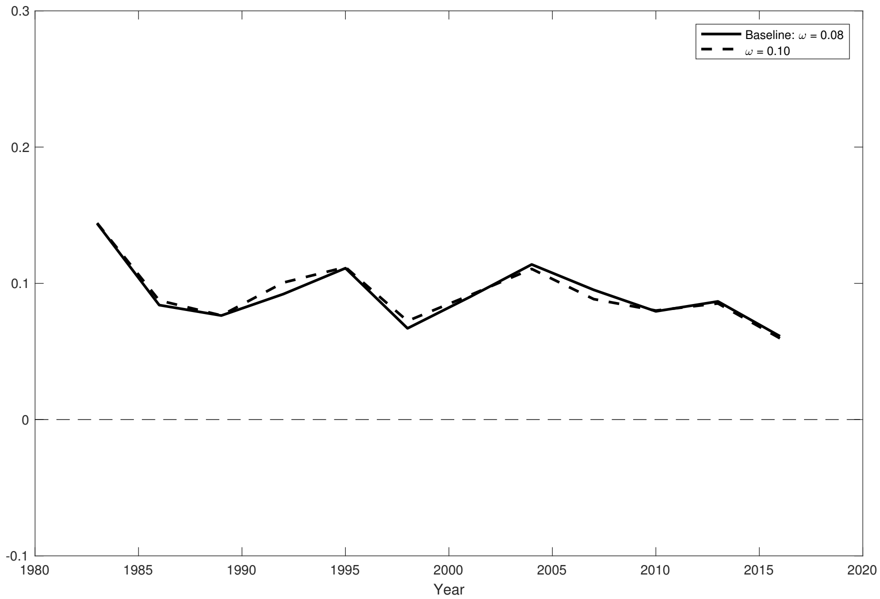

Figure A.22: The Housing Wealth Elasticity For Alternative ω

Note: The figure shows the housing wealth elasticity elasticity when households have a stronger preference for housing. Our calculation takes current states as given by the SCF data so the figure shows the effect of ω on the consumption decision rule.

E.2 Changes in the Discount Factor β

Figure A.23 shows that raising β by 0.01 reduces the level of the housing wealth elasticity by approximately the same amount without changing the time series pattern. Increasing β reduces the speed at which homeowners spend the resources freed up by changes in home prices.

E.3 Changes in Credit Constraints

Our baseline analysis assumes that credit conditions remain constant as households change their balance sheets, yet an important part of the narrative of the housing boom and bust was an expansion and contraction in household credit (e.g., Favilukis et al., 2017). To analyze how looser credit conditions in the housing boom and tighter credit conditions in the Great Recession affected the housing wealth elasticity, we consider two alternative parameterizations of the LTV constraint, one with a maximum LTV of $\theta = 0.90$ and one with a maximum LTV of $\theta = 0.70$ (both assumed to remain constant in the future).[2]

Figure A.24 shows that the housing wealth elasticity is barely changed by shifts in the LTV

[2]Some analyses of changing credit conditions (e.g., Guerrieri and Lorenzoni, 2015) take the initial distribution of individual states from a model simulation (e.g., a steady state). In that type of analysis, if there is a tightening of the credit constraint, households are forced to de-lever. Our analysis differs in that we are taking the distribution of idiosyncratic states from the data and conditional on these states the constraints that households faced in the past are irrelevant. To the extent that households were forced to delever, this should be reflected in the data we see.

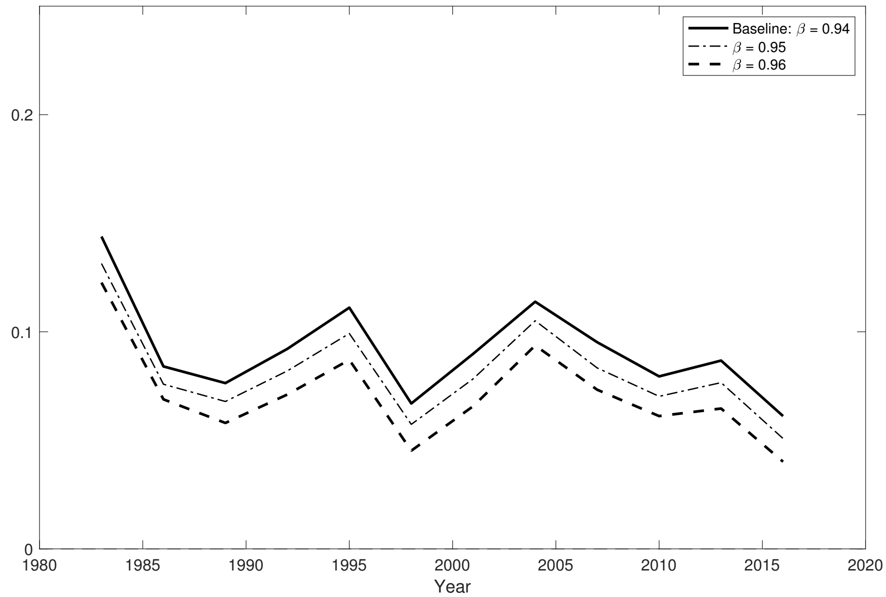

Figure A.23: The Housing Wealth Elasticity with Different Discount Rates

Note: The figure shows the same calculation of $(p/C)(dC/dp)$ as in Panel (a) of Figure 8 using an alternate β parameters for the household's decision problem.

constraint. The intuition in Table 6 is useful to understand these results: a large part of the housing wealth elasticity comes from households who are far from the LTV constraint and their behavior is little affected by the details of the constraint. We should also emphasize that our focus is on the impact of the credit constraint on the *elasticity* of consumption with respect to home prices as opposed to the *level* of consumption. This is an important difference from other analyses that focus on how the level of consumption reacts to changes in borrowing constraints. For example, Table 2 of Favilukis et al. (2017) shows that relaxing the collateral constraint from a 25 percent downpayment requirement to a 1 percent requirement raises the consumption share of GDP by two percentage points. This is entirely consistent with our finding that the housing wealth elasticity was relatively invariant to credit constraints.

E.4 Changes in Interest Rate

Figure A.25 shows results with alternate values for the real mortgage interest rate. We find that the housing wealth effect is increasing in the real mortgage interest rate especially from 2004 onwards, which is a reflection of the increase in mortgage balances relative to income and non-housing assets in those years. As we describe in the main text, the interest rate affects the level of the wealth elasticity because at higher interest rates, households are more likely to downsize their homes and

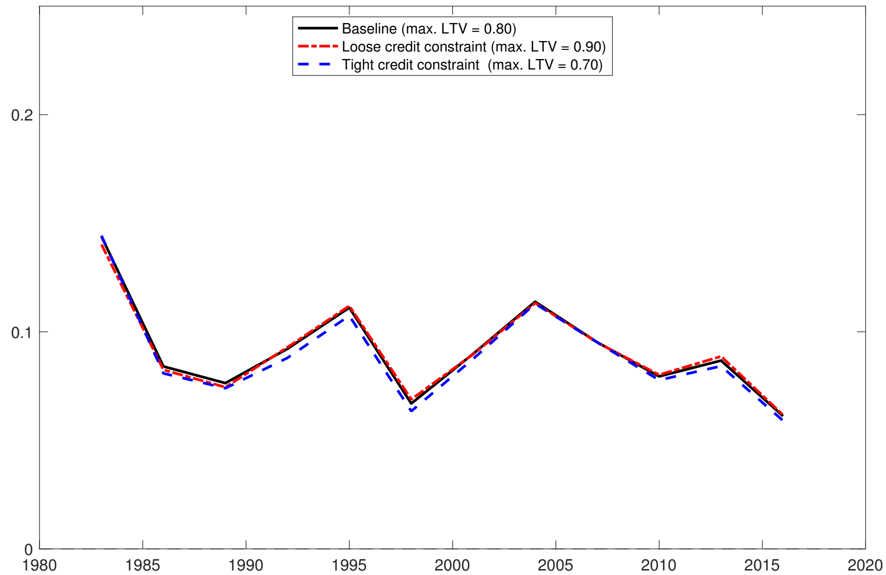

Figure A.24: Impact of Borrowing Constraints on the Housing Wealth Elasticity
Note: Housing wealth elasticity for alternative calibrations of the LTV constraint.

downsizers have large elasticities. The decline in real rates over time may have, to some degree, countered the upward movements in the housing wealth elasticity coming from higher home values and leverage.

E.5 Alternate Assumptions on the Cyclicality of the Rental Cost of Housing

We assume that rents are a constant fraction of home prices. The logic underlying that assumption is that in the absence of expected capital gains, the user cost of housing is roughly proportional to the home price as the main component of the user cost is the foregone interest. Nevertheless, during the housing boom of the 2000s, the rent-price ratio fell considerably. One interpretation is that in cities where home prices were rising sharply, rents remained low because the user cost was kept down by expected capital gains.

Figure A.26 shows that the level of the housing wealth elasticity rises but its time series remains unchanged if we make the polar opposite assumption that rents remain steady when home prices change. In this scenario, an increase in home prices makes renting relatively more attractive. Some renters delay purchasing a home and no longer need to accumulate savings for a downpayment, which allows them to increase their consumption. This force raises the housing wealth elasticity in the aggregate.

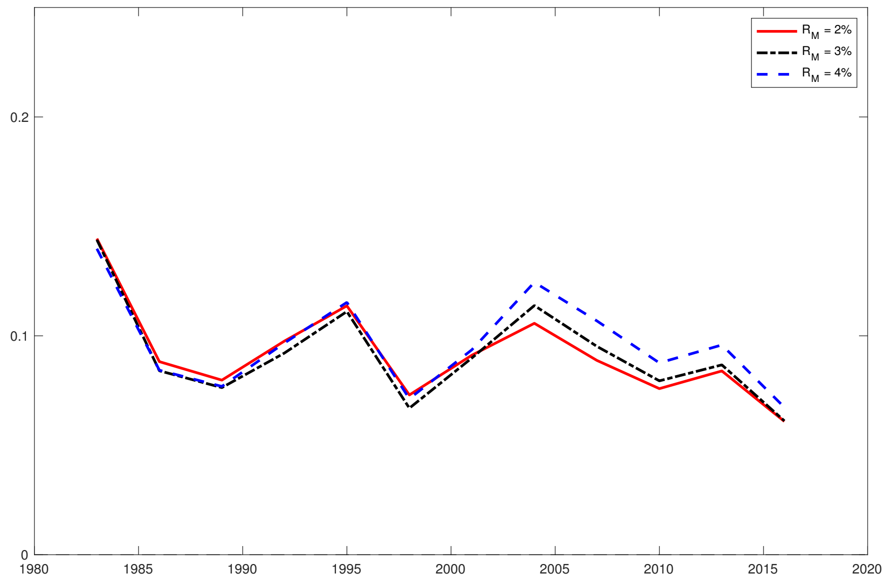

Figure A.25: Sensitivity to Interest Rates

Note: Housing wealth elasticity for alternative calibrations of the mortgage interest rate.

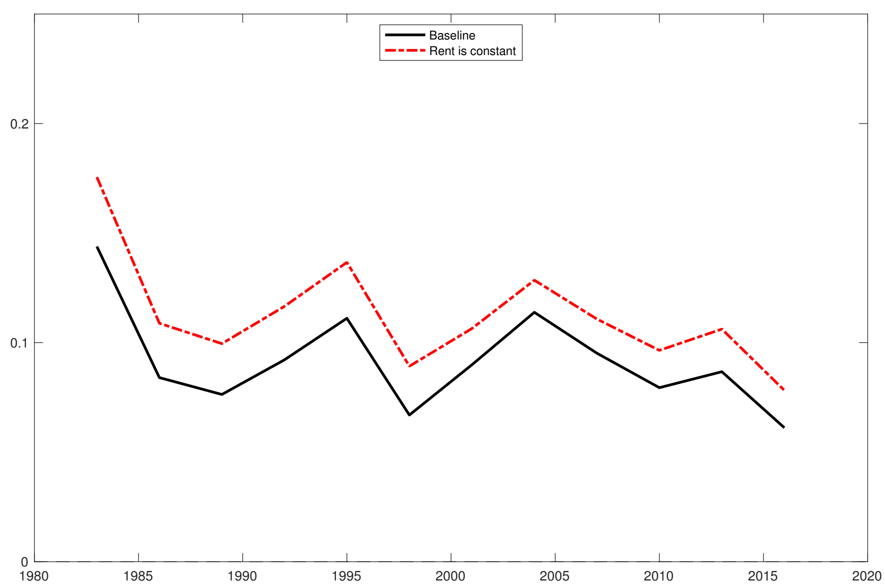

Figure A.26: Sensitivity to Rent-Price Ratio

Note: Housing wealth elasticity assuming that rents are constant as opposed to our baseline assumption that the rent-price ratio is constant.

E.6 Short-Term Debt

Figure A.27 shows the model implied elasticity of consumption to house prices by LTV as in Figure 9 for a short-term debt model in which households must satisfy the LTV constraint each period in order to roll over their debt. The elasticity is much higher for high-LTV homeowners and remains elevated even for underwater homeowners (note the difference in y-axis scales relative to Figure 9). This is the case because households are "margin called" when house prices fall in order to meet the LTV constraint. The model consequently generates an increase in the wealth elasticity in the Great Recession as well as a substantial boom-bust asymmetry.

E.7 Housing Transaction Costs

Figure A.28 shows the housing wealth elasticity for an alternative calibration in which we double the cost of selling a house, ψ^{Sell}. In this alternative calibration, the housing wealth elasticity is lower but the time series pattern is unchanged. With larger transaction costs, homeowners are less likely to realize capital gains on their homes and consumption is more insulated from home price changes. The alternative calibration under-predicts the fraction of households buying a home each year at 2.2% as compared to our empirical target of 3.2%.

E.8 No Short Run Housing Adjustment

To incorporate the inelastic nature of short-run housing supply, we consider an alternative experiment in which there is no change in the demand for housing in the short run. Specifically, we consider two cities with different long-run housing supply elasticities, but both cities have a zero short-run housing supply elasticity. In both cities, a development occurs that shifts housing demand out and in the long-run this will have a larger effect on prices in the less-elastic city. At higher prices, this city will demand less housing than the more-elastic city. We assume that the price of housing rises by 10 percent more in the less elastic city in the long run. In the short run, which we take to be the first year after the news arrives, the price must adjust so that neither city changes its housing demand. In practice, this means that the price differential is initially less than 10 percent because the less elastic city requires an expected capital gain in order to induce people to hold more housing in the short-run. To put it formally, we can write the demand for housing in city i as:

$$H_{i,t} = \int h(x, p_{i,t}, p_{i,t+1}) d\Phi_t(x),$$

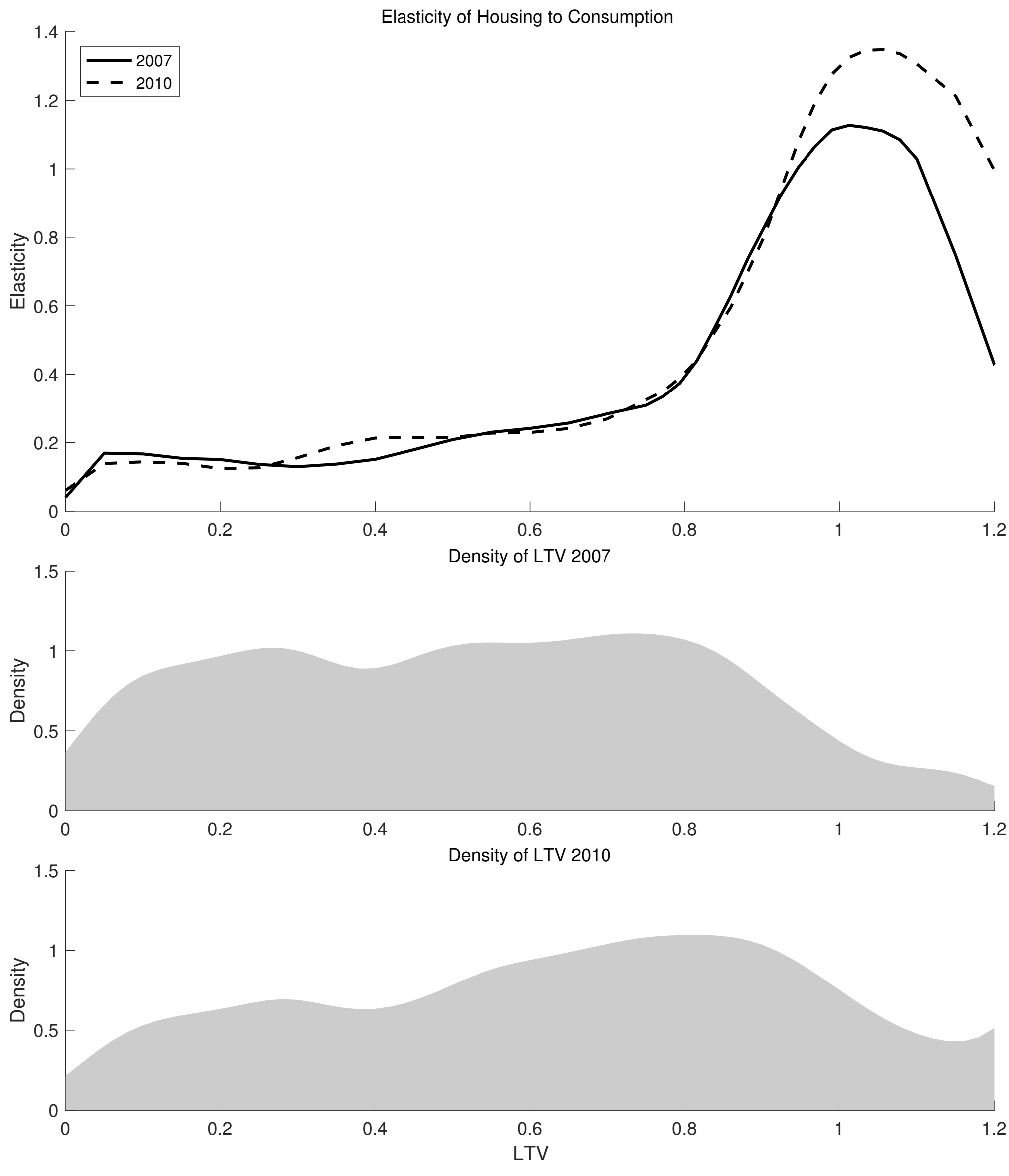

Figure A.27: Elasticity by LTV for Short-Term Debt Model

Note: Housing wealth elasticity across LTVs under the short-term debt model.

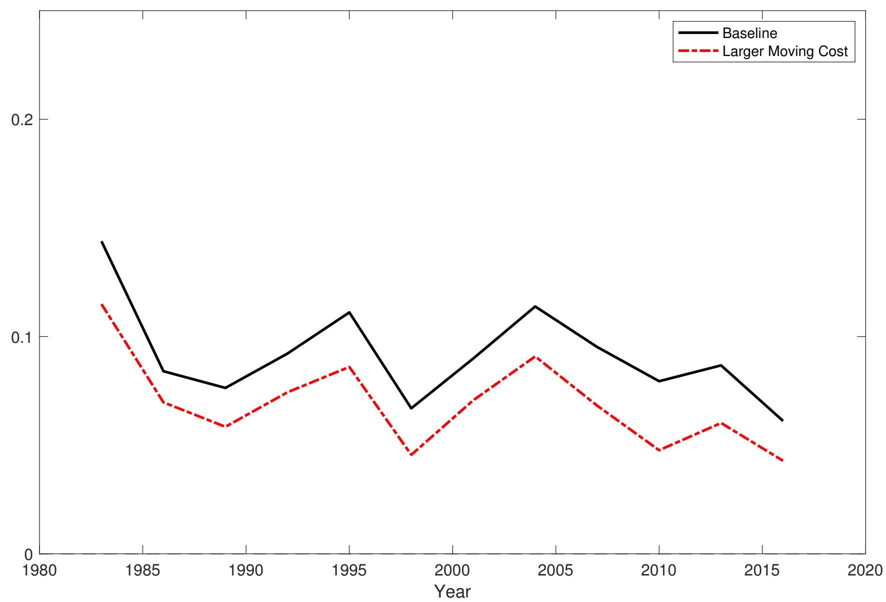

Figure A.28: Sensitivity to Housing Transaction Cost
Note: Housing wealth elasticity with $\psi^{Sell} = 0.22$, double its value in the baseline.

where we assume that the price is constant from $t+1$ onwards. In the more elastic city, we assume the price remains constant at $\bar{p}_t$. This should be interpreted as a normalization, since we focus on the differential behavior of the two cities. In the more-elastic city, housing demand is given by:

$$\bar{H}_t = \int h(x, \bar{p}_t, \bar{p}_t) d\Phi_t(x).$$

In the less-elastic city, we assume the price will rise by 10 percent relative to the more-elastic city in the long-run and in the short-run the price evolves so that the demand for housing in the two cities is equal. That is we solve for the $p_{i,t}$ that satisfies:

$$\bar{H}_t = \int h(x, p_{i,t}, 1.1 \times \bar{p}_t) d\Phi_t(x).$$

Finally, we compare consumption across cities, which we calculate from

$$C_{i,t} = c(x, p_{i,t}, p_{i,t+1}) d\Phi_t(x).$$

Figure A.29 shows the housing wealth elasticity in the short-run (i.e., it compares consumption in the two cities on the date the news arrives expressed as an elasticity with respect to the short-run difference in prices across the cities). The housing wealth elasticity is very similar to in our

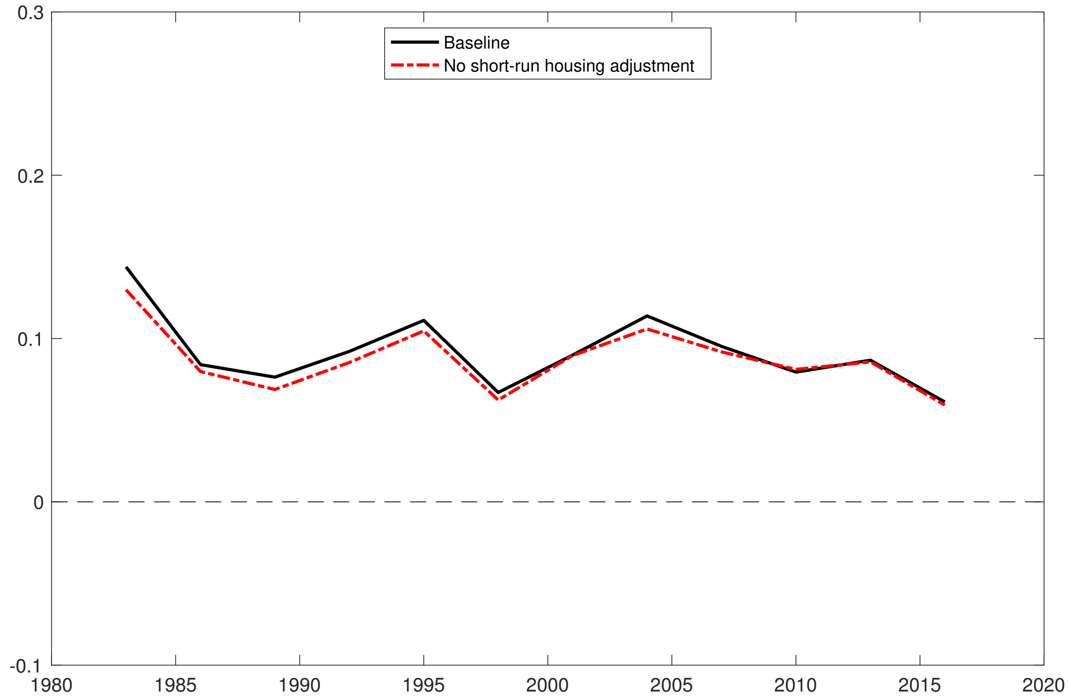

Figure A.29: No Short-Run Housing Adjustment

Note: The figure shows the housing wealth elasticity when expected capital gains on housing adjust so as to stabilize housing demand in the first period after the shock.

baseline case, though slightly lower, because the less-elastic city is no longer substituting out of housing towards consumption in the short-run. The difference from the baseline case is only minor because the demand for housing is sensitive to expected capital gains. Small expected capital gains are sufficient to obtain no short-run housing adjustment (and presumably could also explain increases, as opposed to decreases, in housing supply in response to a house price increase).

E.9 Accounting for the Evolution of Household Balance Sheets

Our baseline analysis takes the distribution of individual states from the SCF. In this Appendix, we show that the model does a fairly good job explaining the year-to-year changes in the distribution of LTVs except for the housing boom years of the early 2000s. We then show how the model can be extended to allow for a relaxation of credit constraints and news about future capital gains in the boom to better explain the evolution of the LTV distribution during those years without substantially affecting the housing wealth elasticity.

Given the observed distribution of individual states at the start of year t, and a sequence of home prices, $\{P_{t+k}\}_{k=0}^{K-1}$, the model implies an evolution of distributions of states for years $t+1$,..., $t+K$. We begin a simulation with each wave of the SCF (i.e. 1983, 1986,...) and simulate four years

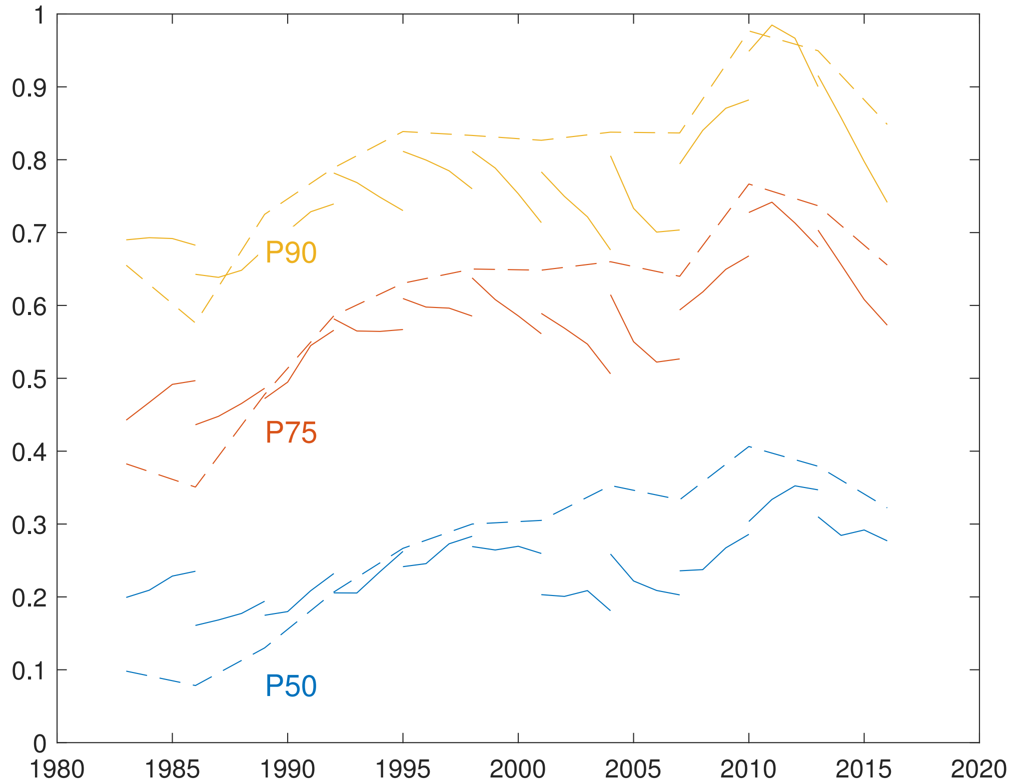

Figure A.30: Simulated LTV Distribution

Note: The dashed lines show the median, 75th percentile and 90th percentile of the LTV distribution among home-owners using data from the SCF. The short solid lines show simulated data, initializing the distribution of individual states from SCF and then feeding into the model the observed evolution of home prices.

of data using the observed evolution of home prices. In this analysis we do not make the CoreLogic adjustment to the SCF data because doing so creates a sharp break in the LTV distribution in 2007 that comes from a methodological change, and we should not expect the model to reproduce that pattern.

Figure A.30 plots quantiles of the LTV distribution both in the data and implied by the model. The model succeeds on three dimensions. First, it captures the increase in leverage in the late 1980s and early 1990s. Second, it is consistent with the increase in leverage in the Great Recession. Finally, it is consistent with the deleveraging observed at the end of the sample. Where the model fails is during the housing boom. During those years, the increases in home values would push LTV down if mortgage debt remained constant, but in the data there is no evident fall in LTV as mortgage debt rose in line with home values leaving LTVs roughly flat over this period. The model does not predict this increase in mortgage debt, so LTVs fall during these years according to the model.

Next we introduce the “boom” parameterization described in the main text that differs from the

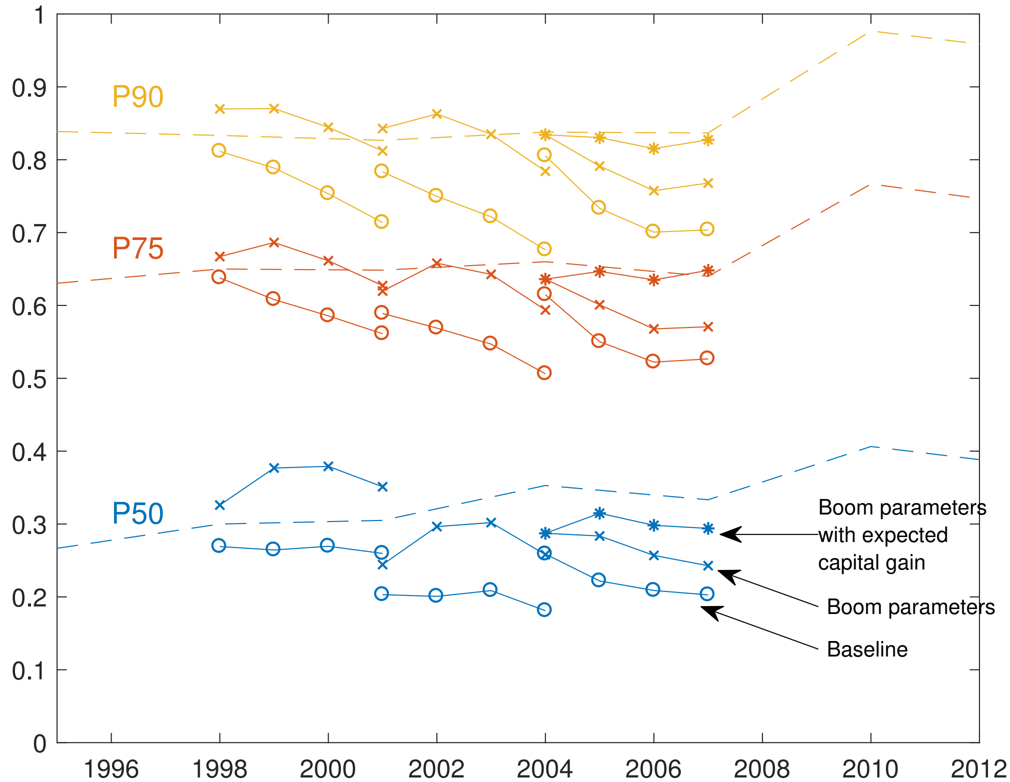

Figure A.31: Simulated LTV Distribution

Note: The figure repeats the simulation from Figure A.30 (baseline) and also shows results for the boom parameters and the boom parameters with expected capital gains for the period 2004-2007.

baseline parameters in that the LTV limit increases from 80 percent to 95 percent and refinancing is free. We use this calibration to simulate the years 1998 to 2007. One interpretation of free refinancing is that the decline in mortgage interest rates following the 2001 recession created strong incentives for refinancing that offset the transaction costs of doing so. Second, we allow for an increasing sequence of capital gains, with the one-year-ahead expected capital gain rising from 0 in 2004 to 2% in 2007 (i.e., 67, 133, and 200 basis points in 2005, 2006, and 2007). This is motivated by Kaplan, Mitman, and Violante (2019) who argue that expected capital gains are central to fitting the evolution of house prices and leverage during the 2000s boom-bust episode. Expected capital gains increase leverage for two reasons: first, homeowners feel richer and increase consumption due to a wealth elasticity and, second, the expected return on housing lowers the user cost of housing and prompts an increase in the demand for housing financed with mortgage debt.

Figure A.31 shows that these changes to the model give a fairly good account of the LTV distribution during the housing boom. As described in the main text, these changes do not significantly alter the housing wealth elasticity. Indeed, in 2007, the baseline parameters lead to a housing wealth elasticity of 0.095, the boom parameters lead to 0.098, and the boom parameters with the expected capital gain leads to 0.114.