NBER WORKING PAPER SERIES

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THE LONG VIEW

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Working Paper 24729
http://www.nber.org/papers/w24729

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2018, Revised November 2018

We would like to thank Massimiliano Cologgi, Hope Kerr, Jimmy Kuo, Jesse Silbert, Xuiyi Song, Yeji Sung, and Sergio Villar for excellent research assistance. We would like to thank Aditya Aladangady, Adrien Auclert, James Cloyne, Masao Fukui, Peter Ganong, Dan Greenwald, Jonathon Hazell, Erik Hurst, Virgiliu Midrigan, Raven Molloy, Pascal Noel, Chris Palmer, Jonathan Parker, Monika Piazzesi, Esteban Rossi-Hansberg, Martin Schneider, Johannes Stroebel, Stijn Van Nieuwerburgh, Joseph Vavra, Gianluca Violante, Ivan Werning, and seminar participants at various institutions and conferences for useful comments. Guren thanks the National Science Foundation (grant SES-1623801) and the Boston University Center for Finance, Law, and Policy. Nakamura thanks the National Science Foundation (grant SES-1056107). Nakamura and Steinsson thank the Alfred P. Sloan Foundation for financial support. The views expressed herein are those of the authors and not necessarily those of the Federal Reserve Bank of Minneapolis, the Federal Reserve System, or the National Bureau of Economic Research.

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NBER Working Paper No. 24729
June 2018, Revised November 2018
JEL No. E21,R21

ABSTRACT

We provide new time-varying estimates of the housing wealth effect back to the 1980s. These estimates are based on a new identification strategy that exploits systematic differences in city-level exposure to regional house price cycles as an instrument for house prices. Our estimates of housing wealth effects are substantially more precise and smaller than recent estimates, though they remain economically important. Our time-varying estimates indicate that housing wealth effects were not particularly large in the 2000s. This contradicts a popular narrative that lax lending standards in the boom and skyrocketing loan-to-value (LTV) ratios during the bust elevated the housing wealth effect in the 2000s. We show, furthermore, that this narrative is inconsistent with a standard life-cycle model with borrowing constraints, uninsurable income risk, illiquid housing, and long-term mortgages. The housing wealth effect in the model is relatively insensitive to changes in the distribution of LTV for two reasons: First, impatient low-LTV agents have a high elasticity; Second, a rightward shift in the LTV distribution increases not only the number of highly sensitive constrained agents but also the number of underwater agents whose consumption is insensitive to house prices.

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An Online Supplement is available at http://www.nber.org/data-appendix/w24729

1 Introduction

How important are housing wealth effects for business cycle dynamics? Recent work argues that housing wealth effects – the sensitivity of economic activity to house prices – played an important role in both the boom of the early 2000s and the recession that followed (Mian and Sufi, 2011; Mian, Rao, and Sufi, 2013; Mian and Sufi, 2014). In this paper, we develop a new approach to estimating housing wealth effects based on a "sensitivity instrument." This instrument exploits the fact that house prices in some cities are systematically more sensitive to regional house-price cycles than house prices in other cities (Sinai, 2013; Palmer, 2015). Our methodology yields both substantially more precise and smaller estimates of the housing wealth effect than recent estimates using the well-known Saiz (2010) housing supply elasticity instrument.

The additional precision of our approach allows us to go beyond the recent literature and provide time-varying estimates of the housing wealth effect back to the 1980s.[1] A popular narrative suggests that large housing wealth effects may have been a special artifact of the 2000s boom-bust housing cycle. In the boom, it is often hypothesized that automated underwriting, lax credit standards, and increased access to home equity lines of credit (HELOCs) allowed households to use their "houses as ATMs" to a greater extent than previously. In the bust, the sharp decline in house prices pushed an unusually large number of households to high loan-to-value (LTV) ratios, causing borrowing constraints to bind and potentially increasing the sensitivity of consumption to house prices. We empirically assess this narrative and find no evidence to support it. Indeed, our time-varying estimates indicate that the housing wealth effect was not historically large in the 2000s; if anything, it was larger prior to 2000.[2]

Perhaps surprisingly, we show that the narrative described above is not only inconsistent with the data but also inconsistent with a standard life-cycle model with borrowing constraints, unin-

[1] Prior to our work, there is essentially no work that estimates whether housing wealth effects have changed over time using a consistent empirical methodology. To our knowledge, two papers have looked at changes over time. First, Case, Shiller, and Quigley (2013) find that the wealth effect was larger after 1986 than before using an OLS approach. Second, Aladangady (2017) finds that housing wealth effects pre-2002 are not significantly different from post-2002, although his estimates are imprecise. Finally, by comparing Case, Shiller, and Quigley (2005), which uses data for 1982-1999, and Case, Shiller, and Quigley (2013), which covers 1978-2009 and has a higher estimate, one can attempt to back out the effect of adding the 2000s (along with 1978-82) to the sample. However, the two estimates are not in fact directly comparable, since both the econometrics and data are different between the two papers. Other empirical estimates for the recent period include Hurst and Stafford (2004); Campbell and Cocco (2007); Carroll, Otsuka, and Salacalek (2011), Attanasio et al. (2009, 2011), Calomiris, Longhofer, and Miles (2012), Cooper (2013); DeFusco (2016); Kaplan, Mitman, and Violante (2016), and Liebersohn (2017).

[2] While the sensitivity (i.e., elasticity) of economic activity to house prices was not historically large in the 2000s, the size of the boom-bust cycle in house prices *was* particularly large in the 2000s at the national level implying that housing likely played a larger overall role in economic developments (i.e., elasticity times price change was large).

surable income risk, illiquid housing, and long-term mortgages. In this model, the housing wealth effect turns out to be highly *insensitive* to the distribution of household leverage and credit constraints for reasons we explain in more detail below. This result depends crucially on the presence of long-term mortgage debt in the model.

The primary empirical challenge in estimating the housing wealth effect is that house prices and economic activity are jointly determined and causation can run in both directions, potentially leading to a substantial upward bias of ordinary least squares (OLS) estimates. Measurement error in local house prices are a second potentially important source of bias that may offset the first. We use an instrumental variables strategy to address these biases. Our instrument builds on earlier work of Palmer (2015) by exploiting the fact that house prices in some cities are systematically more sensitive to regional house-price cycles than house prices in other cities. For example, when a house price boom occurs in the Northeast region, Providence systematically experiences larger increases in house prices than Rochester.

We construct our instrument by first estimating the systematic historical sensitivity of local house prices to regional housing cycles and then interacting this historical sensitivity estimate with today's shock to regional house prices. The basic structure of our instrument is, therefore, similar to that of the well-known "Bartik instrument." This simple approach infers the housing wealth effect from the differential response of retail employment in cities like Providence relative to cities like Rochester when the Northeast experiences a housing boom or bust. We show how to refine this approach to account for the fact that Providence and Rochester may exhibit systematic differences in sensitivity to aggregate shocks for non-housing reasons by estimating the sensitivity parameter using only the *residual* variation in house prices after controlling for local economic conditions. Importantly, our approach does not rely on regional house price variation being exogenous. In fact, regional house price variation can be driven by the same shocks that drive regional economic activity.[3]

Our instrument is a powerful predictor of local house prices: regional housing cycles explain roughly 40 percent of the variation in local house prices even after controlling for local economic conditions. As a consequence, our instrument generates much more precise estimates of the housing wealth effect than instruments based on Saiz's (2010) estimates of housing supply elasticities. Saiz's

[3]The recent literature on general equilibrium models of house prices has emphasized shocks to current and expected future productivity, credit constraints, and risk premia as plausible sources of variation in house prices (see, e.g. Landvoigt, Piazzesi, and Schneider, 2015; Favilukis, Ludvigson, and Van Nieuwerburgh, 2017; Kaplan, Mitman, and Violante, 2017).

elasticity estimates are based mainly on land unavailability. But housing supply elasticities are likely affected by a host of other factors. For many cities, land-constraints are not currently binding but may bind in the future. Whether this happens depends on future growth, implying that beliefs about long-run growth play an important role in determining the size of housing cycles (Nathanson and Zwick, 2017). Our sensitivity estimates will capture variation due to these types of factors (e.g., optimism about the future long-run prospects of Las Vegas leading to large speculative housing cycles). Our analysis suggests that the OLS estimates of the housing wealth effect are upward biased relative to our IV estimates, consistent with an important role for endogenous variation in house prices. In contrast, IV estimates using the Saiz elasticity are typically larger than their OLS counterparts.

A major challenge associated with studying the housing wealth effect during the Great Recession is the nearly contemporaneous timing of the housing price bust and the overall recession. This comovement makes it difficult to distinguish between a city being generally more cyclical and the casual effects of the housing price cycle. For example, coastal cities, which tend to have lower housing supply elasticities, might happen to be more cyclically sensitive than inland regions. Without a panel data approach it is very difficult to distinguish between these hypotheses. Fortunately (for researchers), earlier house price cycles were less correlated with recessions than the 2000s boom and bust. Taking a longer view allows us to control for variation in cyclical sensitivity in constructing our estimates of the housing wealth effect. In fact, we are able to include many additional controls including city fixed effects, region-time fixed effects, industry shares with time-specific coefficients, and differential local sensitivities to risk premia and mortgage interest rates. The addition of these controls allows us to account for a variety of potential sources of endogeneity (Davidoff, 2016). Our main identifying assumption is that conditional on these controls, there is no unobserved factor that is both correlated with house prices in the time series and differentially affects the same cities that are more historically sensitive to regional housing cycles.

We use retail employment as our main dependent variable. We view retail employment as the best available proxy for consumer expenditures for our purposes. This is because our empirical approach requires many years of geographically-disaggregated panel data. Using retail employment as a proxy for consumer expenditures is a relatively standard choice in the measurement literature. For example, this approach is taken by the BEA in constructing regional income and product accounts and by private sector organizations such as Moody's and the Survey of Buying Power. One reason retail employment is a good proxy is that it is an intermediate input into consumer expenditures. Retail employment comoves strongly with the BEA's PCE measure of consumption at the aggregate level. Indeed, the comovement is considerably stronger than between PCE and an aggregate of the Consumer Expenditure Survey (which has actually been falling in recent periods). Changes in retail technology have had little impact on this relationship, as we show in Section 2. Retail employment is also of interest in its own right as a measure of local non-tradeable economic activity (e.g., Mian and Sufi, 2014).[4]

We highlight three main empirical findings. First, our estimate of the housing wealth effect is smaller than recent estimates, though it remains economically important. Our full-sample elasticity estimate is 0.053, which is roughly equivalent to a marginal propensity to consume out of housing wealth (MPCH) of 2.4 cents on the dollar. If we drop the noisiest period of our sample (pre-1990), the elasticity rises to 0.071, equivalent to an MPCH of 3.3 cents on the dollar. By contrast, Mian, Rao, and Sufi (2013) estimate a housing wealth effect of 7.2 cents on the dollar using the Saiz instrument. Similarly, the estimates of Mian and Sufi (2014), which use retail employment as a dependent variable, imply a housing wealth effect of between 4.1 and 7.3 cents on the dollar.

Second, using a 10-year rolling window specification, we find no evidence that the housing wealth effect was particularly large in the 2000s. If anything, it was larger before 2000. This time-series pattern is highly robust across methodologies. We find the same pattern using OLS and the Saiz instrument. Third, we find no evidence of a boom-bust asymmetry in the housing wealth effect that might arise from households hitting borrowing constraints during housing busts.

Theoretically-minded readers may find it hard to interpret a "housing wealth effect." House prices are equilibrium variables that are affected by many shocks which may affect consumption through other channels. So, what do our empirical estimates capture? In Section 5, we discuss how in a simple general equilibrium model in which all markets are regional except for housing markets, which are local, our empirical approach yields an estimate of the partial equilibrium effect of house prices on consumption. In this case, both the direct effects of the shocks that drive aggregate variation in house prices and all general equilibrium effects are soaked up by the region-time fixed effects in our regressions. We also discuss a more realistic general equilibrium model with segmented markets across cities (presented in more detail in Guren et al. (2018)) in which our empirical approach yields an estimate of the partial equilibrium effect of house prices

[4] The existing literature on housing wealth effects uses a variety of dependent variables, ranging from particular consumption categories such as consumer packaged goods or cars (e.g., Mian and Sufi, 2011; Kaplan, Mitman, and Violante, 2016), to credit card spending (e.g., Mian, Rao, and Sufi, 2013) to broader measures based on the Current Expenditure Survey (Aladangady, 2017).

on consumption multiplied by a local general equilibrium multiplier that can be obtained from the literature on fiscal stimulus (e.g., Nakamura and Steinsson, 2014).[5]

We next develop a simple partial equilibrium model of housing wealth effects that builds heavily on the recent literature that has incorporated illiquid housing and long-term mortgages into models with uninsurable income shocks and borrowing constraints.[6] In contrast to earlier models, this class of models can generate large housing wealth effects.[7] Our calibrated model generates an average housing wealth effect of 0.09, somewhat larger than what we estimate in the data.

We show that this model implies that the housing wealth effect is highly insensitive to large changes in household LTV ratios and to large variation in credit constraints. Two features of the model are important to understand these theoretical results. First, incomplete markets models of the type we analyze feature households that are impatient relative to the interest rate, and therefore respond strongly to changes in house prices even when they have low LTV ratios. This implies that a large fraction of the overall housing wealth effect in our model is coming from the many households that are far from any LTV constraints. Second, because mortgages are long-term, underwater households are not forced to de-lever to satisfy an ongoing LTV constraint in a housing bust. Since these underwater households cannot access changes in their housing equity due to house price fluctuations, they are largely unresponsive to these changes, as Ganong and Noel (2017) and Berger et al. (2017) have emphasized. We show that the large rightward shift in the LTV distribution that resulted from the fall in prices during the 2007-2010 housing bust had two offsetting effects on the housing wealth effect. On the one hand, more households were pushed closer to their LTV constraint and consequently became more sensitive to changes in house prices. On the other hand, more households became underwater on their mortgage to the point that they became insensitive to changes in house prices. Quantitatively, these two effects roughly offset to deliver a relatively stable elasticity in the housing bust. By contrast, in models with short-term debt, the housing wealth effect rises sharply in the bust as households are forced to de-lever, which is at odds with our empirical results.

Some may find it surprising to learn that households were spending out of their home equity a quarter century ago. However, the main tools used to extract housing equity — such as cash-out

[5] This formalizes intuitive arguments made by Mian and Sufi (2015).

[6] See, e.g., Agarwal et al., 2017; Berger et al., 2017; Chen, Michaux, and Roussanov, 2013; Davis and Van Nieuwerburgh, 2015; Gorea and Midrigan, 2017; Guren, Krishnamurthy, and McQuade, 2018; Kaplan, Mitman, and Violante, 2017; Li and Yao, 2007; Favilukis, Ludvigson, and Van Nieuwerburgh, 2017).

[7] Sinai and Souleles (2005) present a model in which the housing wealth effect is zero because increased wealth from higher house prices is offset by higher implicit costs of living. This stark conclusion results from several simplifying assumptions including complete markets and households that live in the same house forever.

refinancing and HELOCs — have been available for several decades, and the HELOC share of mortgage debt only rose from 7 percent to 9 percent in the 2000s boom according to the Flow of Funds. Mortgage securitization was invented in the late 1960s and has been done on a large scale since the late 1970s. Others have argued that the major changes in mortgage debt availability occurred in the 1970s (see, e.g., Foote, Gerardi, and Willen, 2012; Kuhn, Schularick, and Steins, 2017). While certain mortgage products may have become available in the 2000s to segments of the population that did not have access to them before, our model shows that this is not likely to have materially affected the overall housing wealth effect. The following quote from Townsend-Greenspan's August 1982 client report written by Alan Greenspan illustrates well how much access households had to housing equity even before the start of our sample period:

> The combination of very rapidly rising prices for existing homes and a sharp increase in sales ... of these homes has created a huge increase in capital gains and purchasing power during the past two years ... by far the greater part has been drawn out of home equities and spent on other goods and services or put into savings. In fact, of the more than $60 billion ... increase in the market value of existing homes ... virtually the entire amount was monetized as mortgage debt extensions, creating nearly a 5% increase in consumer purchasing power.

A modern reader might be excused for thinking that this paragraph was written by Greenspan circa 2005.[8]

The paper proceeds as follows. Section 2 describes our main data sources. Section 3 describes our empirical methodology. Section 4 describes our empirical results. Section 5 makes explicit the link between our empirical analysis and the theoretical analysis that follows. Section 6 presents our partial equilibrium model. Section 7 analyzes how changes in household balance sheets affect the housing wealth effect in the model. Section 8 concludes.

2 Data

Our main measure of local economic activity is retail employment per capita. Retail employment is an interesting outcome variable in its own right. In addition, retail employment has long been

[8]See Mallaby (2016) for further discussion of this point. We thank Sebastian Mallaby for helping us obtain the original copy of this report. Mallaby writes that Greenspan's calculations were based on direct estimates of home equity extraction from mortgage data and the assumption that households spent the entire amount of money extracted from housing wealth in this way.

viewed by measurement agencies as one of the best available proxies for consumer expenditures. For example, the BEA's Regional PCE measures and the private sector "Survey of Buying Power" both use retail employment data to impute consumer expenditures in between economic census years. Private sector measures of consumer expenditures also use retail employment as a proxy. For example, Case et al. (2005; 2013) use data from Regional Financial Associates (now Moody's Economy.com) that is imputed in part from retail employment data.[9]

Figure 1 shows the relationship between the annual change in aggregate retail employment and the annual change in personal consumption expenditures from the BEA's NIPA. The latter is typically viewed as the gold-standard measure of aggregate consumption at the national level. The two series track each other closely. Intuitively, retail services are an intermediate input into household consumption, since consumers must purchase things to be able to consume them. At an aggregate level, retail employment actually does a better job capturing time-series variation in non-durable PCE than the CEX, which has displayed implausible negative growth rates in recent years (see, e.g., Heathcote et al, 2010).

One might worry that the increasing prevalence of big box and online retailers might weaken the relationship between retail employment and the PCE. There is a very small downward trend in retail employment relative to real PCE in figure 1 (hardly visible to the naked eye) that may reflect these forces. However, slow-moving trends will not affect our estimates, since our specification is formulated in growth rates and includes time fixed effects. Consistent with the figure, unreported rolling-window regressions suggest the time series relationship between retail employment and PCE is relatively stable over the time period we study.

There are relatively few alternative measures of consumer expenditures available at a sufficiently high frequency and with a sufficiently long panel to study housing wealth effects. Retail sales data are available at a geographically disaggregated level only every 5 years. Some recent work has used expenditure series for particular categories, such as AC Nielsen data or data on car purchases. These series are not available over the long time horizon required for our study. Moreover, the aggregate time series suggests that retail employment provides at least as good a measure of consumer expenditures (based on the the production-based PCE measure) as these more specialized categories. Another possible source of data to consider might be retail sales tax data. However,

[9]Unfortunately, the specific details of how the "consumption" series published by these private sector sources are constructed is not disclosed. However, it is clear that both the Regional Financial Associates data and the Survey of Buying Power data used by Asdrubali et al (1996) rely substantially on retail employment in their data construction series. This is documented in Zhou (2010) and we have also verified this in private correspondence with the Survey of Buying Power.

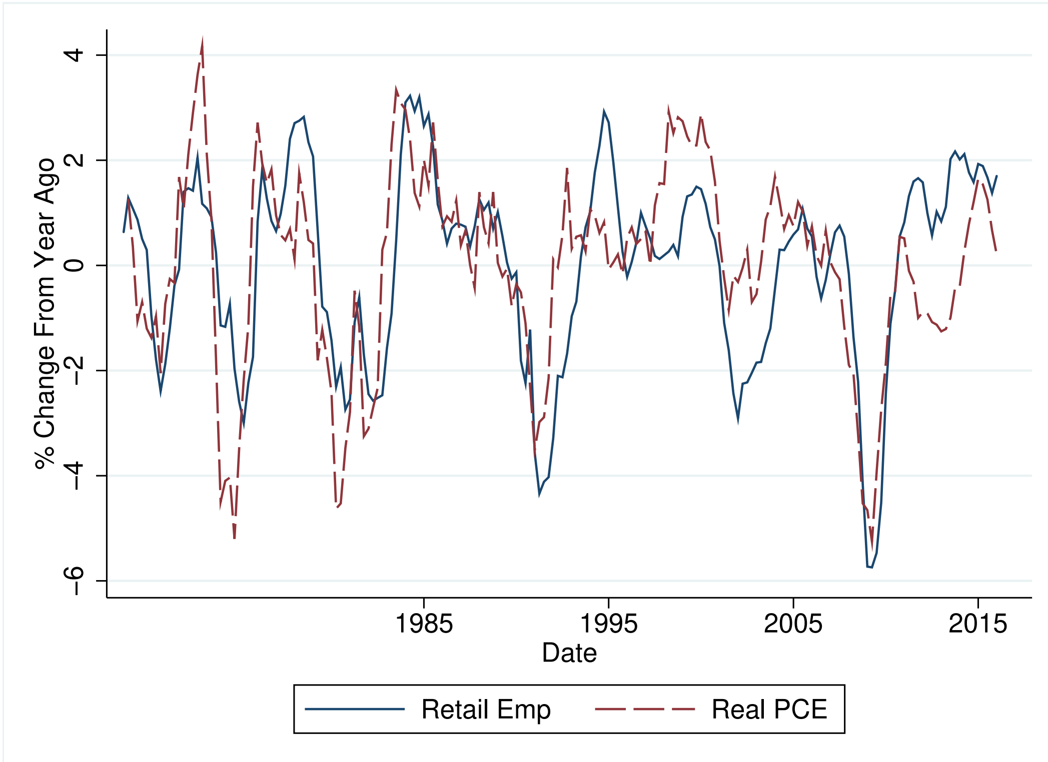


Figure 1: Growth of Retail Employment vs. Growth in Personal Consumption Expenditures
Note: The figure plots the 4-quarter change in aggregate retail employment (FRED series CEU4200000001) and the 4-quarter aggregate change in real personal consumption expenditures (FRED series PCECC96). We take out a linear time trend from both series to account for differential trend growth. The retail employment series has had a slightly larger secular decline than the real PCE series, falling .08% per year as opposed to 0.03% per year. Because our regressions include time fixed effects, we take out this differential trend growth from our analysis.

retail sales tax data are only available for a subset of states and are incredibly noisy in raw form (Garett et al, 2005).[10] Some researchers have used data compiled by private sector data sources, such as the Regional Financial Associates or Survey of Buying Power data, but these sources do not introduce any additional microdata and are imputed from a combination of sources, including retail employment, as we describe above.

In Appendix A.3 we analyze the relationship between city-level consumption and retail employment using data for 17 cities for which the BLS publishes city-level consumption using data from the Consumer Expenditure Survey. Both the CEX and retail employment have substantial sampling error. We use an instrumental variables approach to account for measurement error in retail employment per capita. Our instrumental variables estimates imply that consumer expenditures respond nearly one-for-one with changes in retail employment per capita, consistent with the

[10]Rodgers and Temple (1996) estimate that at a national level, the correlation between the growth rates of national retail sales and personal consumption is only 0.35.

aggregate time series in Figure 1.[11]

Our data for retail employment come from the Quarterly Census of Employment and Wages (QCEW) which is available back to 1975 at the county level. The population data come from the Census Bureau's post-Censal population estimates for 1970 to 2010 and inter-Censal population estimates for 2010 to 2017. These estimates are available annually, and we interpolate to a quarterly frequency. We aggregate the combined data set to the CBSA level and create retail employment per capita for 380 CBSAs.[12] Two issues that arise are how to handle missing data at the CBSA level and how to handle the change in industrial classifications from SIC to NAICs. Appendix D.1.3 provide further detail on how we handle these issues, and show that alternative plausible approaches yield very similar results.

For house prices, our primary data source is the Freddie Mac House Price Indices, which are a balanced panel of indices based on repeat sales for 381 CBSAs from 1975 to 2017 (1976 is thus the first year for annual differences). We convert to a real house price index using the GDP deflator. The Freddie Mac House Price Indices have the advantage that they do not impute any data from neighboring cities. Imputation has the potential to bias our empirical estimates of the differential sensitivity of house price indexes to aggregate shocks across cities. A downside of the Freddie Mac data is that they are limited to conforming loans and makes use of a combination of transaction and appraisal prices. Appraisal prices tend to be smoother than transaction prices.

In Appendix D.1.5, we redo our analysis using the CoreLogic house price index. The results are very similar to our baseline results. Unlike the Freddie Mac Price indices, the CoreLogic indices are all-transaction price indices and include homes purchased with non-conforming loans. The disadvantage is that CoreLogic has far more limited time coverage after dropping city-level indices that are imputed form state and regional indices. The similarity of the results shows that our results are not driven by including appraisals or dropping non-conforming loans.

We also use a variety of other data for controls, which we describe in Appendix A.1.

3 Empirical Approach

The goal of our empirical analysis is to estimate the effect of a change in house prices in one city relative to another on relative per-capita retail employment in the two cities. We do this using the

[11] We have also verified, in unreported work, that changes in CBSA-level retail employment are highly correlated with changes in retail sales over the 5-year intervals at which retail sales are available in the Economic Census.

[12] We drop Dover, DE from our analysis because retail employment data is missing for the entire CBSA for a majority of years.

following empirical specification:

$$\Delta y_{i,r,t} = \psi_i + \xi_{r,t} + \beta \Delta p_{i,r,t} + \Gamma X_{i,r,t} + \varepsilon_{i,r,t}. \tag{1}$$

The subscript i denotes core-based statistical areas (CBSAs) — roughly speaking cities — r denotes Census regions, and t denotes time (measured in quarters). $\Delta y_{i,r,t}$ denotes the log annual change in retail employment per capita, while $\Delta p_{i,r,t}$ denotes the log annual change in house prices. We allow for CBSA fixed effects, ψ_i, which control for long-term trends in each CBSA, region-time fixed effects, $\xi_{r,t}$, which imply that our effects are identified only off of differential movements across CBSAs within a region and time period, a set of additional controls, $X_{i,r,t}$, and other unmodeled influences on retail employment, $\varepsilon_{i,r,t}$.

The coefficient of interest in equation (1) is β, which measures the housing wealth effect as an elasticity. Several challenges arise in estimating β. Causation runs both ways between local employment and house prices, implying that the error term in equation (1) will be correlated with the change in house prices. This is likely to bias OLS estimates of β upward since a strong economy will cause house prices to rise. On the other hand, house prices are measured with error, potentially biasing β towards zero. To address these two sources of bias, we propose a new instrumental variables strategy for estimating β.

The basic idea underlying our identification strategy is the same as in much of the the recent work on housing wealth effects: Heterogeneity in housing supply elasticities across cities will yield heterogeneity in the response of house prices to aggregate shocks. Much of the recent literature has used Saiz's (2010) estimates of housing supply elasticities to construct instrumental variables. These housing supply elasticity estimates are largely based on land-unavailability—the share of land within a 50km radius of the center of a city that is not suitable for construction due to steep slopes or water.[13] But housing supply elasticities are likely affected by a host of other factors, implying that Saiz's estimates are only a relatively crude proxy. To improve on this, one strategy would be to attempt to add more variables to an empirical model of housing supply elasticity such as the one that Saiz uses. We adopt a different strategy, which is to infer differences in housing supply elasticities across cities from systematic differences in the sensitivity of local house prices to regional house price variation.

[13] The elasticity is formally the predicted values from regression six in Table III 6 in Saiz (2010). The Wharton Land Use Regulation Index and land unavailability in levels and interacted with log population are the only factors that are used to predict the elasticity. In practice, land unavailability is the dominant force.

3.1 Simple Intuition for Identification

Before developing our identification strategy in detail, it is useful to consider an example. Figure 2 plots the time series of house prices in Providence and Rochester as well as the Northeast region as a whole. Two features of this example are important for our identification strategy. First, house prices in the Northeast have experienced large regional boom-bust cycles throughout our sample period. In particular, there was a large house-price cycle in the Northeast in the 1980s in addition to the house-price cycle of the 2000s. Regional house price cycles like the 1980s cycle in the Northeast occurred in several regions of the U.S. in the 1980s and 1990s. The timing of these regional cycles has varied, and they largely averaged out for the nation as a whole except for the nationwide boom-bust cycle of the 2000s. The existence of these regional cycles helps us estimate the housing wealth effect before 2000 when identification strategies using nation-wide variation in house prices lose power.

Second, the sensitivity of house prices in different CBSAs in the Northeast to the regional house price cycle varies systematically. When house prices boom in the Northeast, house prices in Providence respond much more than house prices in Rochester. This pattern of differential sensitivity is quite stable over the entire sample period, as noted by Sinai (2013). Furthermore, this pattern is a pervasive feature of house price data across different CBSAs and regions.

These two features of house price dynamics suggest the following simple identification strategy, which we will subsequently refine. First, estimate the sensitivity of house prices in different CBSAs to regional house price movements by running the regression:

$$\Delta p_{i,r,t} = \varphi_i + \zeta_{r,t} + \gamma_i \Delta P_{r,t} + \nu_{i,r,t}, \tag{2}$$

where $\Delta P_{r,t}$ denotes the log annual change in regional house prices and γ_i is a city-specific coefficient.[14] Then use $z_{i,r,t} = \hat{\gamma}_i \Delta P_{r,t}$ as an instrument for $\Delta p_{i,r,t}$ in equation (1), where $\hat{\gamma}_i$ denotes the estimate of γ_i from equation (2). In this identification strategy, $\hat{\gamma}_i$ is our proxy for (the inverse of) the housing supply elasticity in city i. Equation (2) is not the first-stage regression. Rather it is the empirical model we use to generate a proxy for the housing supply elasticity in each city $\hat{\gamma}_i$. Our $\hat{\gamma}_i$ estimates, therefore, play the same role in our empirical strategy as Saiz´s (2010) estimates of housing supply elasticities play in the empirical strategy of, e.g., Mian, Rao, and Sufi (2013) and

[14]To keep our notation simple, we denote $\Sigma_{i \in I} \gamma_i \Delta P_{r,t} I_i$ where I_i is an indicator for city i (that is separate city-specific coefficients for each city i) by $\gamma_i \Delta P_{r,t}$. We use this simplified notation throughout the paper.

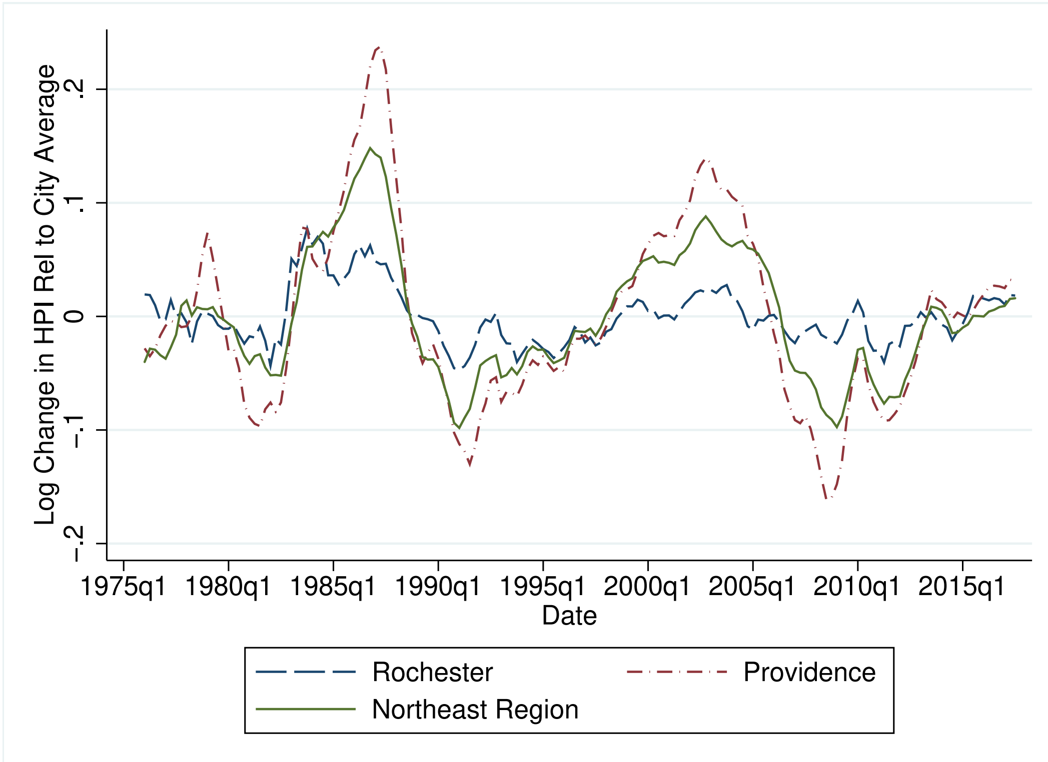


Figure 2: House Prices in Providence, Rochester, and the Northeast Region

Note: The figure shows house prices in the Providence CBSA, Rochester CBSA, and the Northeast Region. All data series are demeaned relative to the CBSA or region average from 1976 to 2015.

Mian and Sufi (2014).

Another way to describe our instrument is that it is similar to a difference-in-difference design: When there is a housing boom in the Northeast, house prices systematically increase more in Providence than in Rochester, i.e., Providence is differentially treated. Since we have panel data, we are able to estimate the systematic extent of differential treatment across CBSAs using equation (2). The question, then, is whether this differential treatment translates into differential growth in retail employment. This empirical strategy is similar to Palmer (2015), who instruments for house prices in the Great Recession using the historical variance of a city's house prices interacted with the national change in house prices.

3.2 Refined Identification Strategy

The simple procedure described above runs into problems if local house prices respond differentially to regional shocks through other channels than differences in housing supply elasticities. Suppose, for example, that there are differences in industrial structure across CBSAs that induce differences in the cyclical sensitivity of employment to the aggregate business cycle (for reasons other than housing). In this case, the heterogeneity in $\hat{\gamma}_i$ may arise from reverse causality. A hypothetical example is instructive: Suppose that Providence has an industrial structure tilted towards highly cyclical durable goods relative to Rochester. In this case, a positive aggregate demand shock would lead retail employment to increase more in Providence than Rochester. If local economic booms raise house prices, this would induce a larger change in house prices in Providence than Rochester and, thus, imply that we would estimate a higher γ_i for Providence using equation (2) purely due to reverse causality. In this case, variation in $\hat{\gamma}_i$ would reflect factors other than differences in housing supply elasticities across cities, potentially invalidating our instrument.

To address this problem, we refine the procedure described above for estimating γ_i by controlling for local and regional changes in retail employment with city-specific coefficients as well as a set of other controls $X_{i,r,t}$ described in more detail below:

$$\Delta p_{i,r,t} = \varphi_i + \delta_i \Delta y_{i,r,t} + \mu_i \Delta Y_{r,t} + \gamma_i \Delta P_{r,t} + \Psi X_{i,r,t} + \nu_{i,r,t}. \tag{3}$$

In this case, we are estimating the γ_i's using only the variation in local house prices that is orthogonal to $\Delta y_{i,r,t}$, $\Delta Y_{r,t}$ and $X_{i,r,t}$. This implies that our $\hat{\gamma}_i$ estimates are not driven by the type of reverse causation described above. Furthermore, $X_{i,r,t}$ includes (among other variables) two-digit industry shares multiplied by time dummies. We therefore non-parametrically control for all variation that is correlated with industry structure in the cross section.

For this approach to yield a powerful instrument, there must be substantial variation in house prices that is orthogonal to movements in local and regional retail employment. This is the case in our data: when we run regression (3) without the differential sensitivity term $\gamma_i \Delta P_{r,t}$, the R-squared is 0.18, but when $\gamma_i \Delta P_{r,t}$ is added, the R-squared rises to 0.62. In other words, our sensitivity instrument explains a large fraction of the total variation in local house prices, even conditioning on local and regional employment.[15]

The key identifying assumption in our analysis is that, conditional on controls, there are no other aggregate factors that are both correlated with regional house prices in the time series and

[15]One potential concern with this procedure is the role of measurement error in $\Delta y_{i,r,t}$ biasing the δ_i terms and thereby creating bias in the γ_is. To assess the severity of this concern, we have also considered a specification in which we instrument for $\Delta y_{i,r,t}$ using a 2-digit Bartik instrument for local economic conditions. For power reasons, we must assume that δ_i is the same across CSBAs, but the δ we obtain is a causal elasticity. We obtain an estimate for δ of 2.9. This estimate for δ can be used to subtract $\delta \Delta y_{i,r,t}$ from $\Delta p_{i,r,t}$, and then we can use this adjusted $\Delta p_{i,r,t}$ to estimate γ_i. This approach yields values for the γ_i that are highly correlated with our baseline approach, and using these alternate γ_is does not significantly alter our results.

that differentially impact retail employment per capita in the same CBSAs that are sensitive to house prices as captured by $\hat{\gamma}_i$. In other words, to bias our results there must exist a confounding factor with the structure $\alpha_i \mathcal{E}_{r,t}$ where $\mathcal{E}_{r,t}$ is correlated with regional house prices in the time series and α_i is correlated with $\hat{\gamma}_i$ in the cross section. Appendix C presents a more formal discussion of our identifying assumptions in the context of a two-equation simultaneous equations system from which we explicitly derive our estimating equations.

Our instrument is a close cousin of the Bartik instrument, which instruments for city labor demand with city industry shares interacted with national changes in employment in each industry. For example, consider a Bartik instrument in which the key source of variation is differential exposure to oil shocks in Texas versus Florida. The identifying assumption is that there is not some other factor that happens to differentially affect Texas at the same time as oil prices go up. Our identifying assumption that there is no aggregate factor that is correlated with regional house prices in the time series and that differentially impacts retail employment in a way correlated with $\hat{\gamma}_i$ has a similar flavor. It is important to understand that for these strategies to be valid, treatment intensity (in our case $\hat{\gamma}_i$ and in the case of the Bartik instrument the industry shares) need not be randomly assigned. This is in fact rarely the case. In the Bartik example, Texas and Florida obviously differ in other ways than just their exposure to oil shocks, but as long as we can attribute any differential effects that occur at the time of oil price shocks to differences in oil exposure, this does not invalidate the instrument.

An important advantage of estimating β using a panel specification is that we can control for differential sensitivity of local retail employment to observable aggregate variables. This allows us to rule out many potential confounding factors with a $\alpha_i \mathcal{E}_{r,t}$ structure. First, as discussed above, we control for local industry shares with separate coefficients for each time period. This accounts for all differential factors that are correlated in the cross-section with industry structure. For example, this control captures unobservable variables relating to some cities having more risky industries than others and therefore being differentially affected by shocks to labor demand or risk premia associated with industrial structure. Second, we include separate controls for the differential city-level exposure to regional retail employment, real 30-year mortgage rates, and Gilchirst and Zakrajek's (2012) measure of bond risk premia. For each of these, we construct the control in an analogous fashion to our instrument by estimating an OLS regression:

$$\Delta y_{i,r,t} = \psi_i + \xi_{r,t} + \alpha_i \Delta X_{r,t} + \varepsilon_{i,r,t}, \tag{4}$$

where ΔX is either the log change in regional retail employment, the change in the 30-year fixed mortgage rate, or the change of the Gilchrist-Zakrajek excess bond premium. We then include each $\hat{\alpha}_i \Delta X_{r,\tau}$ as a control.[16] Finally, in equation (3) only, we control for changes in average wages as reported in the QCEW with CBSA-specific coefficients.

Our panel data approach also allows us eliminate sources of mechanical correlation. In particular, we exclude the CBSA in question from the construction of the regional house price index when running regression (3), so as to avoid bias in γ_i due to the same price being on both the left and right hand side.[17] In our rolling-window analysis, we also estimate equation (3) using time periods other than the time period for which we are estimating equation (1), while in the full-sample analysis the γ_i's for a particular time period are estimated using data from all years except a seven year window around the point in question. We do this to avoid $\hat{\gamma}_i$ reflecting contemporaneous or nearly contemporaneous variation in local house prices to the variation used to estimate equation (1). In practice, these different leave-out procedures yield similar results.

3.3 Inspecting the Variation in $\hat{\gamma}_i$

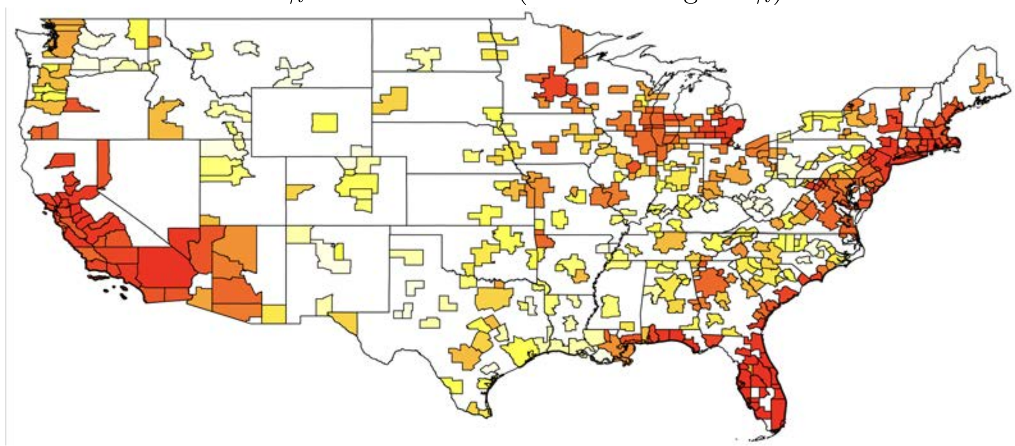
Our estimates of γ_i are a key element of our empirical strategy. The goal of this step is to generate a new proxy for housing supply elasticities of different cities that captures a more comprehensive set of the determinants of housing supply than the estimates of Saiz (2010) and can be used to construct a more powerful instrument for variation in house prices. It is, therefore, instructive to compare our $\hat{\gamma}_i$'s with Saiz's (2010) estimates of housing supply elasticities. Figure 3 does this using two heatmaps. Panel A shows our $\hat{\gamma}_i$'s, while panel B shows the inverse Saiz elasticity.

At a broad-brush level, Figure 3 shows significant similarity between our $\hat{\gamma}_i$'s and Saiz's elasticity estimates. Both measures indicate that many CBSAs on the California coastline, in Florida, and along the Northeast seaboard have inelastic housing supply, while many cities in the interior of the US, especially in Texas and on the Great Plains, have elastic housing supply. However, a closer look at Figure 3 reveals a substantial differences as well across the two measures. For example, Saiz's estimates suggest much lower housing supply elasticities in the Pacific Northwest, the Rocky

[16] We estimate the sensitivity of retail employment to the controls on the "leave-out sample" to avoid overfitting concerns. However, we have also tried the more direct approach of including $\alpha_i \Delta X_{r,t}$ as controls in equation (1). Doing so for the 30-year mortgage rate or the Gilchrist-Zakrajek excess bond premium yields essentially the same results with slightly larger standard errors. Doing so for retail employment yields similar results starting with 10-year windows centered in the mid-1990s and highly imprecise results with lower point estimates in the early 1990s.

[17] There is an arithmetic reason not to include region-time fixed effects in equation 3 that arises as a consequence of this leave-out procedure. Since a leave-out mean appears in this regression, arithmetically, it is possible to perfectly predict local house prices if region-time fixed effects are included.

A. γ_i at CBSA Level (Darker is Higher γ_i)



B. Saiz Estimated Housing Elasticity at City Level (Darker is More Inelastic)

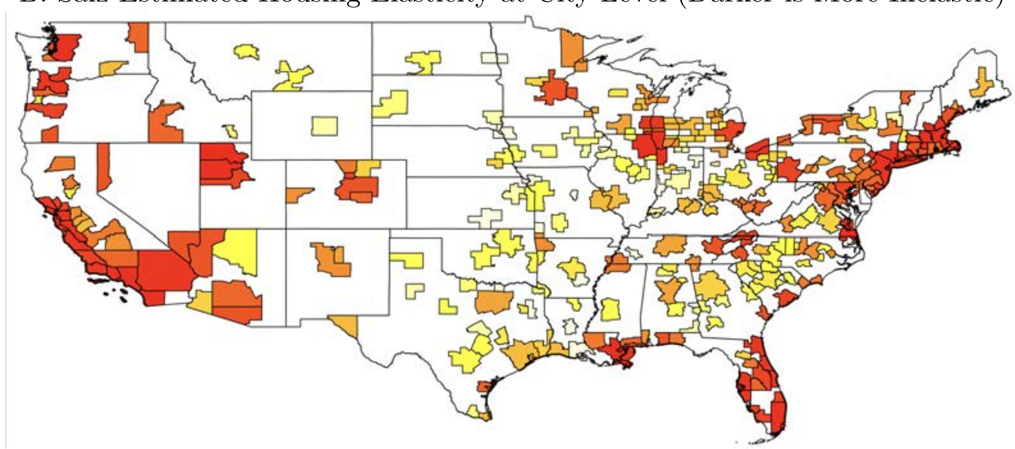


Figure 3: γ_i and Saiz Elasticity by CBSA for Continental U.S.

Notes: These Figures provide heat maps for γ_i and the Saiz elasticity. γ_i is estimated in a single pooled regression that does not leave out any years from 1975 to 2017. The Saiz instrument is adjusted so that darker colors represent inelasticity rather than elasticity so that darker regions in both figures are where prices tend to move by more in response to a shock.

Mountains, and near Lake Erie and Lake Ontario than our $\hat{\gamma}_i$s. In fact, the R-squared of a regression of our $\hat{\gamma}_i$s on Saiz's elasticity is only 0.15.

Why might these differences arise? In thinking about this, it is important to recognize that the amplitude of house price cycles is determined not only by current housing supply elasticities but also by expectations about future housing supply elasticities. Many cities with an intermediate degree of land unavailability are not currently constrained but may become constrained in the future. Whether these cities become constrained in the future depends on their expected long-run growth rate. Indeed, Nathanson and Zwick (2017) emphasize that the amplitude of housing cycles in such cities can depend heavily on both expectations about future long-run growth and the degree of disagreement about future long-run growth prospects. The existence of a group of people that are very optimistic about the long-run prospects of a city with an intermediate degree of land constraints can create particularly large housing cycles in Nathanson and Zwick's model.

These ideas can potentially explain a good portion of the discrepancies between our $\hat{\gamma}_i$s and Saiz's elasticity estimates. Consider, for example, Las Vegas and Pittsburgh. Both have an intermediate degree of land unavailability, but our $\hat{\gamma}_i$ for Las Vegas is very large, while our $\hat{\gamma}_i$ for Pittsburgh is among the smallest among all large cities (see Table A.4 in the appendix for a list of cities with large and small $\hat{\gamma}_i$s in each region). One way to make sense of this large difference in $\hat{\gamma}_i$s is that Las Vegas is a high-growth city with an industrial structure that may be particularly conducive to high degrees of disagreement about future long-run growth (in particular wild optimism), while Pittsburgh's growth is much slower and few people are wildly optimistic about its long-run prospects. The same types of differences may explain discrepancies between our $\hat{\gamma}_i$s and Saiz's elasticity estimates for many other cities such as Orlando, Phoenix and the California Central Valley, on the one hand, and Cleveland, Rochester, Buffalo, New Orleans, and Salt Lake City, on the other hand.

Detroit is another interesting example. Both our $\hat{\gamma}_i$ and Saiz's elasticity estimate indicate that housing supply is relatively inelastic in Detroit. However, our $\hat{\gamma}_i$ for Detroit is large relative to Saiz's elasticity estimate for the city. A distinctive feature of Detroit is that it has been in steep decline throughout much of our sample period. Glaeser and Gyouko (2005) argue that cities in decline have particularly inelastic housing supply because houses are very durable. Essentially, the growth rate of the housing stock in Detroit is stuck at the rate of depreciation, making housing supply particularly unresponsive to economic conditions. The high value of $\hat{\gamma}_i$ we estimate for Detroit seems to capture this better than Saiz's estimate. Other factors that may play a role are that some regions are more "bubbly" due to social connections to inelastic cities (Bailey et al., 2017) or credit (Favara and Imbs, 2015).

4 Empirical Estimates of Housing Wealth Elasticity

We organize our empirical results into two groups. First, in section 4.1, we present full-sample estimates as well as estimates based on single cross-sections. Second, in section 4.2, we present time-varying estimates of the housing wealth effect based on 10-year rolling window regressions.

Table 1: Pooled Elasticity of Retail Employment Per Capita to House Prices

	(1)	(2)	(3)	(4)
Time Period	1978-2017	1978-2000	2000-2017	1990-2017
$\Delta \log(P)$	0.053***	0.057	0.054***	0.071***
	(0.015)	(0.064)	(0.013)	(0.012)

Note: For these estimates, we first construct our instrument for each quarter by estimating the γ_i's in equation (3) for each quarter, using a sample period that leaves out a three-year buffer around the quarter in question. Each column estimates equation (1) for the indicated time period. Standard errors are two-way clustered at the region-time and CBSA level.

4.1 Full-Sample and Single Cross-Section Estimates of Housing Wealth Effect

Table 1 presents estimates of the elasticity β in equation (1) for our full sample period as well as for several subperiods. We estimate CBSA fixed effects once for the entire sample period and apply them to all sample periods rather than estimating a different set of CBSA fixed effects for each sample period.[18] This avoids time variation in these fixed effects driving time variation in our coefficient of interest. We report standard errors that are constructed using two-way clustering by CBSA and region-time to allow for arbitrary time series correlations for a given CBSA and for correlations across CBSAs within a region at a particular time.[19]

The first column of Table 1 reports our estimate of the housing wealth effect pooled over the period 1978-2017. For this period, the pooled elasticity is 0.053, with a standard error of 0.015. This implies that a 10% decline in house prices in a CBSA relative to other CBSA's leads to roughly a 0.53% decline in retail employment. This pooled estimate is equivalent to a marginal propensity to consume out of housing wealth (MPCH) of 2.44 cents on the dollar assuming a one-to-one relationship between retail employment and consumption as suggested by regressions in Appendix A.3.[20]

The remaining columns of Table 1 report estimates for pre-2000 and post-2000 sample periods as well as 1990-2017. The elasticity is very similar for the pre-2000 period as it is for the post-2000 period: 0.057 and 0.054, respectively. However, the standard errors for the pre-2000 estimate are

[18]We regress all variables on CBSA fixed effects for the full sample and use the residuals from these regressions in our main analysis.

[19]The standard errors do not account for sampling error associated with the generated instrument, which would require a bootstrap procedure that only clusters on time. We take the conservative route of two-way clustering in the main text and consider alternative bootstrap standard errors that account for sampling variation in the instrument but cluster only on time in Appendix D.1.6.

[20]To convert our elasticity to a marginal propensity to consume out of housing wealth requires dividing the elasticity of consumption to house prices by the ratio of housing wealth to consumption. The average ratio of H/C over 1985 to 2016 where H is measured as the market value of owner-occupied real estate from the Flow of Funds and C is measured as total personal consumption expenditures less PCE on housing services and utilities, is 2.17. Hence, we obtain a marginal propensity to consume out of housing wealth of $0.053/2.17 = 2.44$ cents for each additional dollar of housing wealth.

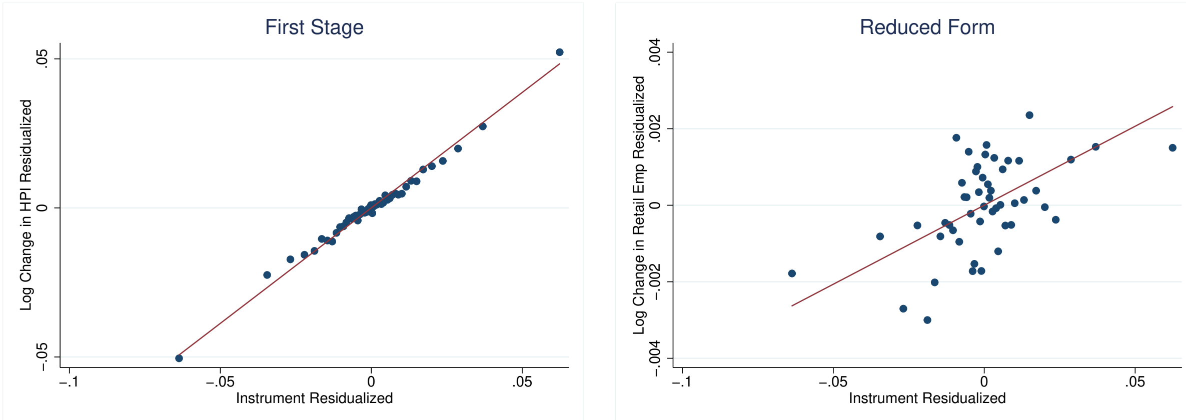


Figure 4: Pooled First Stage and Reduced Form Binned Scatter Plots

Note: The figure shows binned scatter plots of the first stage and reduced form of the IV elasticity of retail employment per capita to real house prices at the CBSA level for the pooled 1990-2017 sample. These correspond to specification (1) in Table 1. For these estimates, we first construct our instrument for each quarter by estimating the γ_i's in equation (3) for each quarter, leaving out a three-year buffer around the quarter in question. We then estimate equation (1) pooling over the sample period 1990-2017. Both the x and y variables are residualized against all fixed effects and controls to create a two-way relationship that can easily be plotted (the Frisch-Waugh theorem).

much larger due low precision prior to 1990. When we drop the data prior to 1990, we obtain a somewhat larger elasticity of 0.071, which implies to an MPCH of 3.27 cents on the dollar.

Figure 4 presents binned scatter plots for the first stage and reduced form of our instrumental variables specification for the 1978-2017 pooled sample. These plots show that neither the first-stage nor the reduced-form relationships are driven by outliers. The first stage is very strong, reflecting the statistical power of our approach.

Our estimates of the housing wealth elasticity in Table 1 are smaller than recent estimates based on the Saiz elasticity instrument. For example, Mian, Rao, and Sufi, (2013) estimate a non-durable consumption elasticity between 0.13 and 0.26, while Mian and Sufi's (2014) estimates an elasticity for retail employment between 0.09 and 0.16.[21] The most commonly cited MPCH in the literature is Mian, Rao, and Sufi's (2013) estimate of 7.2 cents on the dollar using the Saiz

[21] Mian, Rao, and Sufi report estimates of the elasticity of non-durable consumption to housing net worth in the range 0.5-0.8. To convert Mian and Sufi's elasticity with respect to total net worth to a housing wealth elasticity, one must multiply by the mean housing wealth to total wealth ratio in their data, which is between 0.25-0.33 (Berger et al., 2017). This yields a range for the elasticity of retail employment to house prices of between 0.13 and 0.26. Mian and Sufi (2014) estimate an elasticity of restaurant and retail employment to total net worth of between 0.37 and 0.49 for 2006-9, which must be adjusted using a similar procedure. This yields a range for the elasticity of retail employment to house prices of between 0.09 and 0.16. They do not adjust for population flows, which they find are unimportant in their sample. Similarly, Kaplan, Mitman, and Violante (2016) estimate the elasticity of non-durable consumption with respect to net worth using the Saiz instrument and find estimates between 0.34 and 0.38 which implies an elasticity with respect to house prices of between 0.085 and 0.13. Our pooled estimates are most comparable to Aladangady (2017) who estimates an MPCH of 4.7 cents for homeowners and zero for renters, which corresponds to an MPCH of roughly 3.1 cents overall given a homeownership rate of 65 percent. Other studies estimate a marginal propensity to borrow out of housing wealth. For instance, Cloyne et al. (2017) use quasi-experimental variation in refinancing timing due to expiring prepayment penalties in the UK to find an elasticity of 0.2 to 0.3.

Table 2: Comparison of Estimation Approaches for 2006-2009

Specification	2006-2009 Elasticity	
Baseline Instrument (Per Capita), CBSA FE	0.061**	(0.019)
Baseline Instrument (Per Capita)	0.096***	(0.018)
Baseline Instrument (Not Per Capita)	0.116***	(0.020)
Baseline Instrument, Saiz Sample (Not Per Capita)	0.126***	(0.025)
Saiz Elasticity Instrument (Not Per Capita)	0.165	(0.093)
OLS (Not Per Capita)	0.118***	(0.013)

Note: This table compares our regional sensitivity instrument to the Saiz Instrument and OLS for the 2006 to 2009 long difference. For the sensitivity instrument, we first construct our instrument for the three-year window estimating the γ_i's in equation (6), leaving out a three-year buffer around the quarter in question. We then estimate $\Delta y_{i,r,t} = \xi_r + \beta \Delta p_{i,r,t} + \Gamma X_{i,r,t} + \varepsilon_{i,r,t}$, where $X_{i,r,t}$ includes the control for city-level exposure to regional retail employment and 2-digit industry share controls, and region fixed effects. For the CBSA fixed effects specification, we first take out CBSA fixed effects (or equivalently demean) for the entire 1976-2017 period for all variables, but we do not do so for other specifications. The full sample includes 379 CBSAs (excluding Dover, DE and The Villages, FL, which has a suspicious jump in employment for the 2006-2009 window). The Saiz sample is limited to the 270 CBSAs for which we have land unavailability from Saiz (2010) instead of the full 379 CBSA sample. For the Saiz elasticity instrument, we run the same regression with the cyclical sensitivity control instrumenting with the elasticity rather than our sensitivity instrument. OLS runs the second-stage regression by OLS with the same controls but without taking out CBSA fixed effects or using per-capita variables. Robust standard errors are in parenthesis.

instrument, although sometimes their OLS estimate of 5.4 cents on the dollar is used. Motivated by these estimates, Berger et al. (2017) calibrate their state-of-the-art theoretical model so that it generates a housing wealth elasticity of 0.23.

Why is our estimate of the housing wealth effect smaller? To answer this question, it is useful to fix the time period and consider elasticity estimates based on a single cross section of 3-year growth rates from 2006 to 2009, which is the type of specification that Mian, Rao, and Sufi, (2013) and Mian and Sufi (2014) use. Table 2 presents results for several variants of this type of specification. All of these specifications include region fixed effects and the full set of controls that we include in our baseline specification.

The specification in the first row is analogous to our baseline panel specification and yields an estimate of 0.061, which is slightly larger than our full-sample estimate of 0.053, but smaller than our post-1990 pooled estimate. The second row presents a specification without CBSA fixed effects, i.e., without demeaning all variables using means over the entire 1976-2017 sample period. This raises the estimated elasticity to 0.096, which suggests that it is important to account for long-run differences in growth rates across CBSAs in calculating the housing wealth effect. Davidoff (2016) has pointed out that housing supply constraints are correlated with long-run demand growth and argued that this poses a problem for cross-sectional analysis of housing wealth effects based on the Saiz instrument. The fact that we can control for such long-run differences in growth rates using

CBSA fixed effects is an important virtue of our panel data approach relative to the single cross section specification prevalent in the recent literature.

The third row of Table 2 presents results for a specification in which we follow the common practice of not adjusting for population (e.g., Mian and Sufi, 2014). This raises the elasticity from 0.096 to 0.116, indicating that some of the non-per-capita response is due to population flowing towards regions with increasing house prices. The fourth row of Table 2 limits the sample to the cities for which the Saiz instrument is available. This raises the elasticity slightly to 0.126. The fifth row of Table 2 presents results based on the the Saiz instrument. This yields an elasticity of 0.165. Moving from our sensitivity instrument to the Saiz instrument also increases the size of the standard errors by more than a factor of three. The final row of Table 2 presents results based on OLS, which yields an elasticity of 0.118. Our sensitivity instrument gives an estimate of housing wealth effect that is essentially equal to OLS, while the Saiz instrument gives higher estimates than OLS.

4.2 Time-Varying Estimates of Housing Wealth Effect

Figure 5 presents 10-year rolling window estimates of the elasticity β in equation (1) using the empirical strategy described in section 3. Each point in the figure gives the elasticity for a 10-year sample period with its midpoint in the quarter stated on the horizontal axis (e.g., the point for quarter 2010q1 is the estimate for the sample period 2005q1-2015q1). We start the figure with the 10-year window from 1985q1 to 1995q1 because the standard errors for our estimates are very large prior to that point, but we use data back to 1976 in creating our instrument. As with Table 1, we take out a single CBSA fixed effect for the whole sample and two-way cluster by CBSA and region-time.

Figure 5 indicates that housing wealth effects were not particularly large in the 2000s relative to earlier years. If anything, housing wealth effects have weakened since the 1990s. Appendix D presents results based on a number of alternate specifications, data sets, and methodologies and shows that the time series pattern in Figure 5 is highly robust. We present results without controls, based on 5-year rolling windows, weighting by population, excluding the "sand states," using 3-year differences rather than annual differences, using housing data from CoreLogic, using a fixed set of $\hat{\gamma}_i$s, as well as several other specifications. Appendix D.1.2 presents 10-year rolling window estimates of the first stage and reduced form. The main time series patterns are clearly evident in the reduced form, and although the first stage is stronger after 2000, it still has a high F-statistic

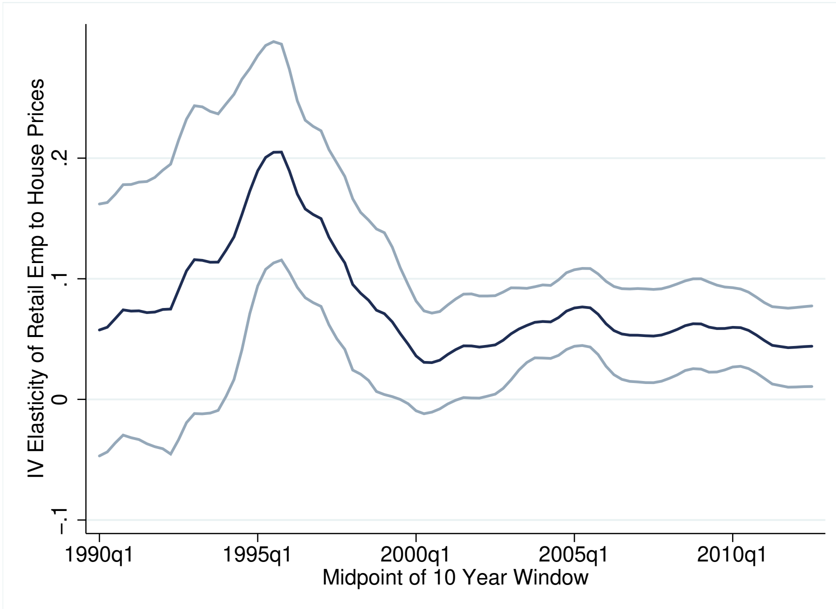


Figure 5: The Elasticity of Retail Employment Per Capita to House Prices Over 10 Year Windows
Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods. Each point indicates the elasticity for a 10-year sample period with it's midpoint in the quarter stated 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 region-time.

(above 100) prior to 2000.

The idea that housing wealth effects may have been particularly large in the Great Recession is related to the idea that housing wealth effects are particularly potent in housing busts — perhaps due to powerful debt-deleveraging. In Table 3, we consider non-linear regression specifications to assess this possibility. Column (2) in Table 3 includes separate coefficients for positive and negative house price changes, while column (3) includes a quadratic term in house price changes. We find no evidence of a boom-bust asymmetry in house price elasticities. In column (2), the coefficient on negative house price changes is slightly *smaller* not larger as powerful debt-deleveraging in busts would suggest, but the p-value of a test for equality is 0.77. In specification (3) the quadratic term is both statistically insignificant and quantitatively small.[22]

It is instructive to compare our results based on our sensitivity instrument to analogous results based on OLS and based on Saiz's (2010) estimates of housing supply elasticities. Figure 6 presents

[22]Previous evidence is mixed on whether there is an asymmetry in responses to house price increases versus decreases. Case et al. (2005) find an asymmetry, but Case et al. (2013) reject this initial finding with additional years of data. Cloyne et al. (2017) find a large elasticity if the collateral constraint is relaxed but nothing if it is tightened. Guerrieri and Iacoviello (2017) find an asymmetry in CBSA-level data for services employment using CBSA data. Finally, Liebersohn (2017) finds a large asymmetry for durables but not for consumption overall.

Table 3: Evaluation of Nonlinearity of Elasticity of Retail Employment Per Capita to House Prices

	(1)	(2)	(3)
$\Delta \log(P)$	0.053***		
	(0.015)		
$\Delta \log(P)-$		0.049*	
		(0.021)	
$\Delta \log(P)+$		0.057**	
		(0.021)	
P Test for Equality		0.769	
$\Delta \log(P)$			0.051**
			(0.016)
$\Delta \log(P)^2$			-0.030
			(0.043)

Note: For these estimates, we first construct our instrument for each quarter by estimating the γ_i's in equation (3) for each quarter, using a sample period that leaves out a three-year buffer around the quarter in question. Specification (1) estimates equation (1) pooling over the sample period 1978-2017. Specification (2) replaces Δp with $\Delta p \times 1[\Delta p \geq 0]$ and $\Delta p \times 1[\Delta p < 0]$ as regressors. Specification (3) adds a quadratic term in the log change in house prices to equation (1). For specification 2, we instrument with $Z \times 1[Z \geq 0]$ and $Z \times 1[Z < 0]$ and for specification 3 we instrument with Z and Z^2. Standard errors are two-way clustered at the region-time and CBSA level.

10-year rolling window estimates based on OLS alongside our baseline results. The OLS estimates have a similar declining time pattern as our baseline estimates but are slightly larger, reflecting the expected endogeneity bias. These results suggest that the time-series pattern for the housing wealth effect we estimate is not a quirky feature of our instrument.

Figure 7 compares our baseline estimates to estimates using Saiz's (2010) housing supply elasticity interacted with the log change in the national house price index as an instrument.[23] Again, the time series pattern of declining elasticities is evident in the point estimates based on Saiz's housing supply elasticities. However, the estimates of β based on Saiz's housing supply elasticities have much larger standard errors. Before 2005 the standard errors for this specification are sufficiently large that the estimates are essentially uninformative.

It is also instructive to consider whether changes in house prices affect manufacturing employment. Figure 8 plots results analogous to those presented in Figure 5 except that the dependent variable in the analysis is manufacturing employment. In contrast to our results for retail employment, the point estimates for manufacturing employment are close to zero for most of the sample period, although the estimates are fairly imprecise. The absence of an effect on manufacturing employment is consistent with our interpretation that the effects on retail employment we observe are driven by a housing wealth effect. One would expect a housing wealth effect to affect local

[23] In unreported results, we have found similar results for regional versions of this instrument and versions that use Saiz's land unavailability measure rather than his estimated elasticity.

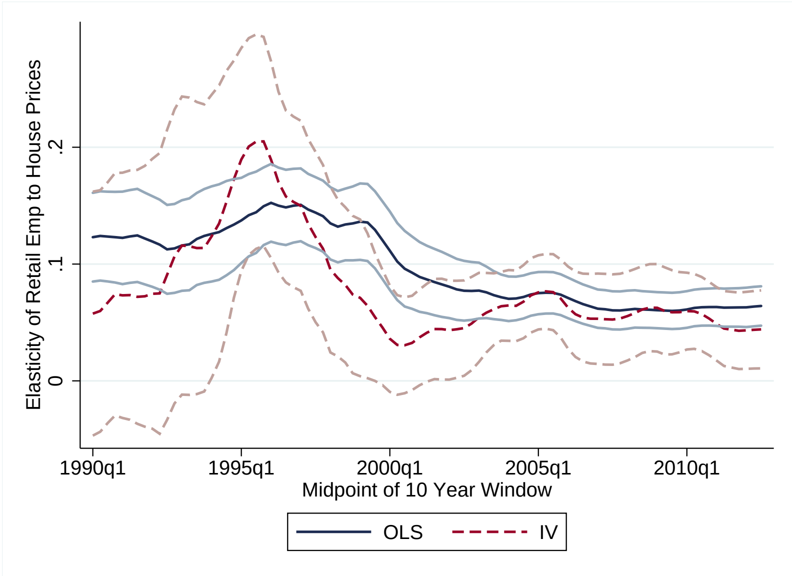


Figure 6: Housing Wealth Effect: Sensitivity Instrument vs. OLS

Note: The red dashed line plots point estimates of the housing wealth effect based on 10-year rolling windows with the date indicating the midpoint of the window using our instrument (same as in Figure 5). The light red dashed lines plot the upper and lower bounds of 95% confidence intervals for these estimates. The dark blue line plots point estimates of the housing wealth effect estimated using OLS with the same controls as our baseline IV specification. The lighter blue lines plot the upper and lower bounds of 95% confidence intervals. Standard errors are two-way clustered by region-time and CBSA.

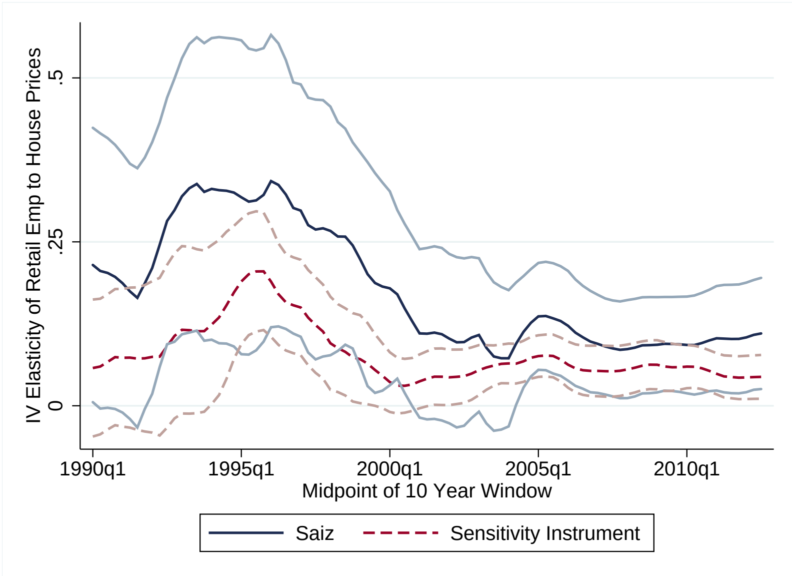


Figure 7: Housing Wealth Effect: Sensitivity Instrument vs. Saiz Instrument

Note: The red dashed line plots point estimates of the housing wealth effect based on 10-year rolling windows with the date indicating the midpoint of the window using our instrument (same as in Figure 5). The light red dashed lines plot the upper and lower bounds of 95% confidence intervals for these estimates. The dark blue line plots point estimates of the housing wealth effect based on 10-year rolling windows estimated using an instrument based on the estimated housing supply elasticity of Saiz (2010) interacted with the national annual log change in house prices. The lighter blue lines plot the upper and lower bounds of 95% confidence intervals for the estimates based on the Saiz instrument. Standard errors are two-way clustered by region-time and CBSA.

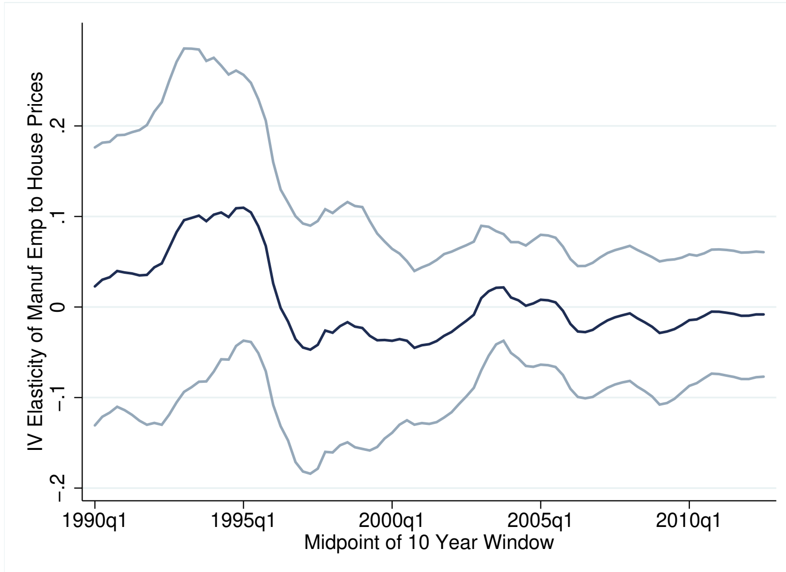


Figure 8: The Elasticity of Manufacturing Employment Per Capita to House Prices Over 10 Year Windows

Note: The figure plots the elasticity of manufacturing employment per capita to real house prices at the CBSA level for rolling 10-year sample periods. Each point indicates the elasticity for a 10-year sample period with it's midpoint in the quarter stated 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 region-time.

spending, but not demand for manufacturing goods which are presumably largely consumed in other cities. This result is similar to Mian and Sufi's (2014) finding that house prices mainly affect non-tradeable production through their effect on local demand.[24]

5 Data to Theory

In the decision problem of a household, house prices are exogenous. The "causal effect" of house prices on household consumption in such a partial equilibrium setting is therefore straightforward to interpret. By contrast, at the aggregate level or city level, house prices are an endogenous variable. House prices are affected by a myriad of shocks and these shocks may affect consumption not only through house prices but also directly or through other channels. So what does it mean to estimate the causal effect of house prices on consumption at the city level?

Consider a simple model of an economy consisting of several regions with many cities in each

[24]Mian and Sufi (2014) use "tradeable employment" which is dominated by manufacturing. We use manufacturing instead because we are faced with the SIC to NAICS transition in 2000, which makes it very difficult to create a consistent time series of tradeables using Mian and Sufi's approach for identifying such industries at the 4-digit level. By contrast, for manufacturing we can handle the transition by splicing together log changes for the manufacturing series under SIC and NAICS as we do for retail employment.

region. Suppose housing markets are local to each city and the cities differ in their housing supply elasticities. All other markets are fully integrated across cities within a region (and may in some cases be integrated across regions). The cities are initially in identical steady states before being hit by a one-time, unexpected, and permanent aggregate shock that alters the demand for housing. This shock leads house prices to respond differently across cities due to the difference in housing supply elasticities, but all other prices respond symmetrically within region because all other markets are integrated within region. It is not important for our argument exactly what the nature of the aggregate shock is. It could be an aggregate productivity shock, an aggregate demand shock (e.g., monetary, fiscal, or news shock), or an aggregate housing specific shock such as a shock to the preference for housing or to construction costs.

Consumption in city i, in region r, and at time t can be written as $c_{i,r,t} = c(p_{i,r,t}, \omega_{i,r,t}, \Omega_{r,t}, R_{r,t})$, where $\omega_{i,r,t}$ is a vector of idiosyncratic shocks, $\Omega_{r,t}$ is a vector of regional or national shocks, $R_{r,t}$ is a vector of prices such as interest rates and wages. One can interpret $R_{r,t}$ as including not only current prices, but also prices for future-dated goods. Since all markets other than the housing market are integrated across cities within region, $R_{r,t}$ does not have an i subscript. All cities have the same aggregate consumption function. Consumption only differs across cities to the extent that they experience different home prices and different shocks. In a companion paper (Guren et al., 2018), we provide an example of a fully-specified general-equilibrium model of the type described above that yields a consumption function of this form.

Take a log-linear approximation to the aggregate consumption function around the initial steady state and then take an annual difference. This yields:

$$\Delta c_{i,r,t} = \underbrace{\phi_p}_{\beta} \Delta p_{i,r,t} + \underbrace{\phi_\Omega \Delta\Omega_{r,t} + \phi_R \Delta R_{r,t}}_{\xi_{r,t}} + \underbrace{\phi_\omega \Delta\omega_{i,r,t}}_{\varepsilon_{i,r,t}}, \tag{5}$$

where $c_{i,r,t}$ denotes the logarithm of consumption and ϕ_x denotes the elasticity of $c(\cdots)$ with respect to the variable x evaluated at the steady state. These elasticities should be understood as vectors of elasticities where appropriate. Equation (5) is labeled to show how it relates to equation (1) in our empirical analysis.

Suppose we ran the empirical specification described in Section 3 on data from this model. Equation (5) shows that the general equilibrium impact of changes in prices other than house prices as well as the direct effect of aggregate and regional shocks will be absorbed by the region-time fixed effects $\xi_{r,t}$. Our coefficient of interest β captures the response of consumption to a house price change holding these other variables constant. This shows that if we are able to identify variation in local house prices that is orthogonal to the error term $\varepsilon_{i,r,t}$ and the assumptions stated above about market structure hold, the coefficient β will estimate the *partial equilibrium* effect of house prices on consumption.[25]

The simple general equilibrium model discussed above makes the strong assumption that all markets except the housing market are fully integrated across cities within a region. If we relax this assumption, the differential response of house prices across cities will result in differential responses in other markets as well. In other words, the differential house price movements will result in local general equilibrium effects. Since these local general equilibrium effects will differ across cities within a region, they will not be absorbed by the region-time fixed effects in our empirical specification and will affect our estimate of β.

Local general equilibrium effects result from changes in local demand affecting local wages, prices, and incomes. This suggests that evidence from other local demand shocks might be useful in pinning down the effect of local general equilibrium on our empirical estimates. In Guren et al. (2018), we present a general-equilibrium regional business cycle model with heterogeneous housing supply elasticities that allows for local general equilibrium effects. In this model, we show that the local government spending multiplier can be used to quantify local general equilibrium effects. More specifically, we show that the housing wealth effect estimate β that results from our empirical specification can be expressed as:

$$\beta \simeq \beta_{LFM}\beta_{PE},$$

where β_{LFM} denotes the local fiscal multiplier and β_{PE} denotes the partial equilibrium effect of house prices on consumption.[26] Intuitively, a dollar of spending triggers the same local general equilibrium response regardless of whether it arises from a housing wealth effect or government spending. Nakamura and Steinsson (2014) estimate that the local government spending multiplier is roughly 1.5 at the state level but 1.8 at the region level. Since our analysis is at the CBSA level,

[25]If non-linearities are important, the fixed effects in equation (5) will not fully absorb the general equilibrium price effects. For example, if consumption growth responds importantly to $\Delta p_{i,r,t} \times \Delta\Omega_{r,t}$ or to $\Delta p_{i,r,t} \times \Delta R_{r,t}$, then our estimated β will reflect these interactions in addition to the housing wealth effect. In the next section we present a fully non-linear model of the housing wealth effect and we show in Appendix E.1 that the model implies these interaction effects are small. In particular, the housing wealth effect is close to linear in the magnitude of the price change and symmetric with respect to positive and negative price changes.

[26]We make certain simplifying assumptions to derive this result. One of these is to assume GHH preferences to avoid wealth effects on labor supply. We abstract from the collateral channel emphasized by Chaney, Sraer, and Thesmar (2012) and Adelino, Schoar, and Severino (2015). We assume that the government and households both buy the same consumption good. Finally, we assume that construction employment does not respond to house prices. In Guren et al. (2018), we assess how relaxing this last assumption affects our results.

the relevant local government spending multiplier for our analysis is likely somewhat smaller than 1.5.

6 A Model of the Local Consumption Response to House Prices

We now present a partial equilibrium model of housing and consumption. The key features of the model are a life cycle, uninsured idiosyncratic income risk, borrowing constraints, illiquid housing, and long-term mortgage debt subject to an LTV constraint. We keep our model purposefully simple and evaluate its robustness to some of our starker assumptions in Appendix E.

6.1 Assumptions

Households live for T periods and have preferences over non-durable consumption and housing services given by,

$$\mathbb{E}_0 \left[\sum_{t=1}^{T} \beta^t u(c_t, h_{t+1}) + \beta^{T+1} B(w_{T+1}) \right],$$

where c is consumption, h is housing, $B(\cdot)$ is a bequest motive, and w_{T+1} is wealth left to offsprings. We parameterize household preferences as:

$$\begin{aligned} u(c,h) &= \frac{1}{1-\gamma} \left(c^{(\varepsilon-1)/\varepsilon} + \omega h^{(\varepsilon-1)/\varepsilon} \right)^{(1-\gamma)\varepsilon/(\varepsilon-1)} \\ B(w) &= \frac{B_0}{1-\gamma} \left(w + B_1 \right)^{(1-\gamma)}. \end{aligned}$$

Here γ captures the curvature of the utility function, ε is the elasticity of substitution between housing and non-durable consumption, B_0 captures the strength of the warm-glow bequest motive, and B_1 captures non-homotheticity in bequest motives.[27]

An individual can consume housing either by owning or renting. A unit of housing can be purchased at price p or rented for one period at cost δp. This implies that the rent-price ratio is fixed and given by the parameter δ. We make this simplifying assumption to avoid counterfactual flows between renting and owning in response to changes in house prices. We consider alternative assumptions about the behavior of rents in Appendix E. In our baseline model, people expect home prices will remain constant at their current level. In extensions, we consider cases with expected capital gains on housing. Throughout, we abstract from home price risk and the

[27] In the presence of illiquid durable goods such as housing, the parameter γ is related to, but not equivalent to, the coefficient of risk aversion (see Flavin and Nakagawa, 2008).

associated precautionary behavior associated with those risks. Renting h units of housing delivers the same utility as buying that amount of housing, but the rent is higher than the user cost of owner occupied housing, which makes owning attractive despite its associated transaction costs. To sell a house the individual must pay ψ^{Sell} of the value of the house in a transaction cost and to buy a house the individual must pay ψ^{Buy}.

Households can take out mortgages. We denote the mortgage principal that a household brings into the period by m. At origination, mortgage debt must satisfy,

$$m' \leq \theta p h', \tag{6}$$

where θ is the maximum LTV and primes denote next period values. The mortgage interest rate is R_m and a household must pay a transaction cost of $\psi^m m'$ to originate a mortgage. We model mortgages as long-term debt that households can refinance at any time. To refinance, a household must pay the same transaction cost as when a mortgage is initiated ($\psi^m m'$ where m' is the new mortgage balance). The repayment schedule requires a payment such that $m' = G(a) R_m m$, where a is the age of the household. Following Campbell and Cocco (2003), $G(a)$ is defined so that the loan amortizes over the rest of the homeowner's lifetime. The amortization schedule is given by:

$$G(a) \equiv 1 - \frac{1 - R_m^{-1}}{1 - R_m^{-(T-a+1)}}.$$

The household can save, but not borrow, in liquid assets with return $R_a < R_m$. Finally, 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.

6.2 Calibration

A household is born at age 25, works for 36 years, retiring at 61, and dies deterministically after age 80. We set most of the parameters through external calibration, which we describe first, and then we set a small set of parameters through internal calibration. We set the curvature of the utility function, γ, to 2. We set the elasticity of substitution between housing and non-durable consumption to 1.25 based on the estimates of Piazzesi, Schneider, and Tuzel (2007). We set the LTV limit, θ, to 0.80 based on GSE guidelines for conforming mortgages without private mortgage insurance. We set the after-tax, real interest rate on mortgage debt to 3 percent per year based on the long-run averages of nominal mortgage rates and inflation.[28] We set the real return on liquid assets to 1 percent based on the difference between the long-run averages of the 1-year Treasury rate and inflation. We set the cost of buying a house to 2 percent. This is meant to reflect closing costs associated with a home purchase.

During the household's working years, we model log annual income as the sum of a life-cycle component, a transitory component, and a persistent component. The life-cycle component is taken from Guvenen et al. (2016). We conceive of the transitory income shocks as non-employment shocks motivated by the income process in Guvenen et al. (2016). With some probability the household is employed for the full year and the (log) transitory income shock is zero. With the remaining probability, the household spends part of the year out of work. The fraction of the year the household spends non-employed is drawn from an exponential distribution truncated to the interval $(0, 1)$. The probability of a non-zero non-employment shock and the parameter of the exponential distribution are estimated by maximum likelihood using the distribution of weeks worked in the prior year reported in the 2002 March CPS. The persistent component of labor income is modeled as an AR(1) with an AR coefficient of 0.97 and innovations drawn from a mixture of two normals, which allows us to capture the leptokurtic nature of income growth rates (see Guvenen et al., 2016). The Appendix provides further explanation of the income process and the parameter values. At retirement, a household faces no further labor income risk and is paid a social security benefit based on their final working-life income, which is calculated in the manner proposed by Guvenen and Smith (2014).

We set the remaining parameters through internal calibration. These parameters are 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}. Our target moments are the life-cycle profiles of home value to income for working-age homeowners (we target the 25th, 50th, 75th, and 90th percentiles of the distribution at each age), mortgage LTV (P25, P50, P75, P90 by age), liquid assets (P25, P50, P75, P90 by age), and the homeownership rate by age. These empirical moments are calculated from the 2001 SCF. In addition, we target a 9.3 percent refinancing rate per year. Empirically, the refinancing rate is higher than this target (see, e.g., Wong, 2018; Bhutta and Keys, 2016) but some refinancing activity results from changes in interest rates, which are not part of our

[28] Between 1971 and 2017 the average CPI inflation rate was 4.1 percent, the average 30-year fixed rate mortgage rate was 8.2 percent, and the average 1-year treasury rate was 5.3 percent. Our choice of a 3 percent real interest rate on mortgage debt is meant to capture the tax-deductibility of mortgage interest.

Table 4: Internally Calibrated Parameter Values

β	ω	B_0	B_1	δ	ψ^m	ψ^{Sell}
0.939	0.0795	85.0	1.75	0.0435	0.0203	0.110

analysis. Our target is based on Deng et al. (2000) who estimate a model of refinancing probability as a function of mortgage age and the difference between the mortgage interest rate and the market rate. We simulate their model for an environment with constant interest rates and compute the fraction of mortgages that are refinanced each year. Finally, 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 ago. 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; for example, due to a change in marital status. We exclude these moves resulting in the 3.2 percent moving rate. In total, we have 650 moments for seven parameters, so our model is highly over-identified, and we seek to minimize a weighted sum of the squared difference between the model-implied and empirical moments. Appendix B explains our empirical moments and objective function in more detail. The resulting parameter values appear in Table 4.

7 Model Simulations

Let us begin by defining the object in the model that corresponds to the IV estimate of the housing wealth elasticity we present earlier in the paper.[29] In the model, we can write aggregate consumption in city i as:

$$C_i = \int c(x, p_i) d\Phi(x),$$

where $c(\cdots)$ is the consumption function of an individual, p_i is the price of a unit of housing in city i, x is a vector of idiosyncratic state variables, and Φ is the distribution of households over idiosyncratic states. The idiosyncratic states in the model are liquid assets, units of housing owned, mortgage debt, income, age, and relative price of housing. The object of interest is the elasticity of C_i with respect to p_i, which we refer to as the model-implied housing wealth elasticity. We compute this elasticity by averaging together the responses to a 10% positive and 10% negative change in

[29] Our empirical strategy uses retail employment as a proxy for consumption. In our analysis of the model we do not model the retail sector explicitly and assume that retail employment responds one for one with consumption. Appendix A.3 provides evidence using city-level CEX data in support of the one-to-one relationship between consumption and retail employment.

p_i.[30]

The model-implied housing wealth elasticity depends on the distribution Φ. Over our sample period, household balance sheets have changed substantially as households have become much more leveraged. The 75th percentile of the LTV distribution rose from about 0.4 in 1983 to 0.6 in the early 1990s and then spiked to 0.9 during the Great Recession as house prices fell. There has also been a great deal of variation in median home values relative to median income over our sample period. This ratio was about two in 1983, but rose to nearly four in 2007. We are interested in assessing the degree to which these large changes in household balance sheets affect the model-implied housing wealth elasticity.

To this end, panel A of Figure 9 reports the model-implied housing wealth elasticity using the distribution Φ that we observe for each wave of the Survey of Consumer Finances (SCF) from 1983 to 2016 (with a post-2007 adjustment based on LTV data from CoreLogic).[31] The variation observed across years in the figure therefore represents the extent to which our model implies that the observed changes in household balance sheets and demographics over the period 1983 to 2016 have lead to variation in the size of the housing wealth elasticity. The most striking feature of the figure, in our minds, is how small the variation in the model-implied housing elasticity is across years. The model generates a relatively smooth housing wealth elasticity across years despite very substantial changes in household balance sheets. For example, the large increases in leverage that occurred between 2007 and 2010 and between 1983 and 1992 do not result in significant changes in the housing wealth elasticity implied by our model.

To unpack this result, panel B of Figure 9 presents results where we vary the distribution of one state variable at a time holding the distribution of the other state variables constant. We begin by varying the LTV distribution to reflect the marginal distribution in year t while keeping all of the other state variables at their 2001 level. We start with the 2001 SCF data and for each year

[30]We consider larger shocks in Appendix Figure A.21 and find that the elasticity is stable with respect to the size of the price change and similar for negative and positive price changes, which is consistent with our empirical finding that there is no boom-bust asymmetry.

[31]Appendix B.3 describes how we estimate Φ from the SCF and CoreLogic data. We adjust home values from 2007 onwards to match the distribution of LTVs for homeowners with a mortgage in CoreLogic's Homeowner Equity Reports. We do so by assuming that households correctly report their mortgage balance in the SCF but misreport their house value so that the error is rank-preserving in LTV. We make this adjustment because we are concerned that the SCF data understates the fall in home values during the Great Recession and therefore understates the fraction of households with high LTVs. CoreLogic did not produce equity estimates for earlier years. However, most of our results are little affected by this adjustment, as we show below. The methodology that CoreLogic used to estimate LTVs changed between 2007 and 2010 to better account for loan amortization and HELOC draw-down, causing a leftward shift in the LTV distribution. Our main estimates switch methods between 2007 and 2010. To show this change does not affect the results substantially, Panel A of Figure 9 presents results for 2010 using both methods.

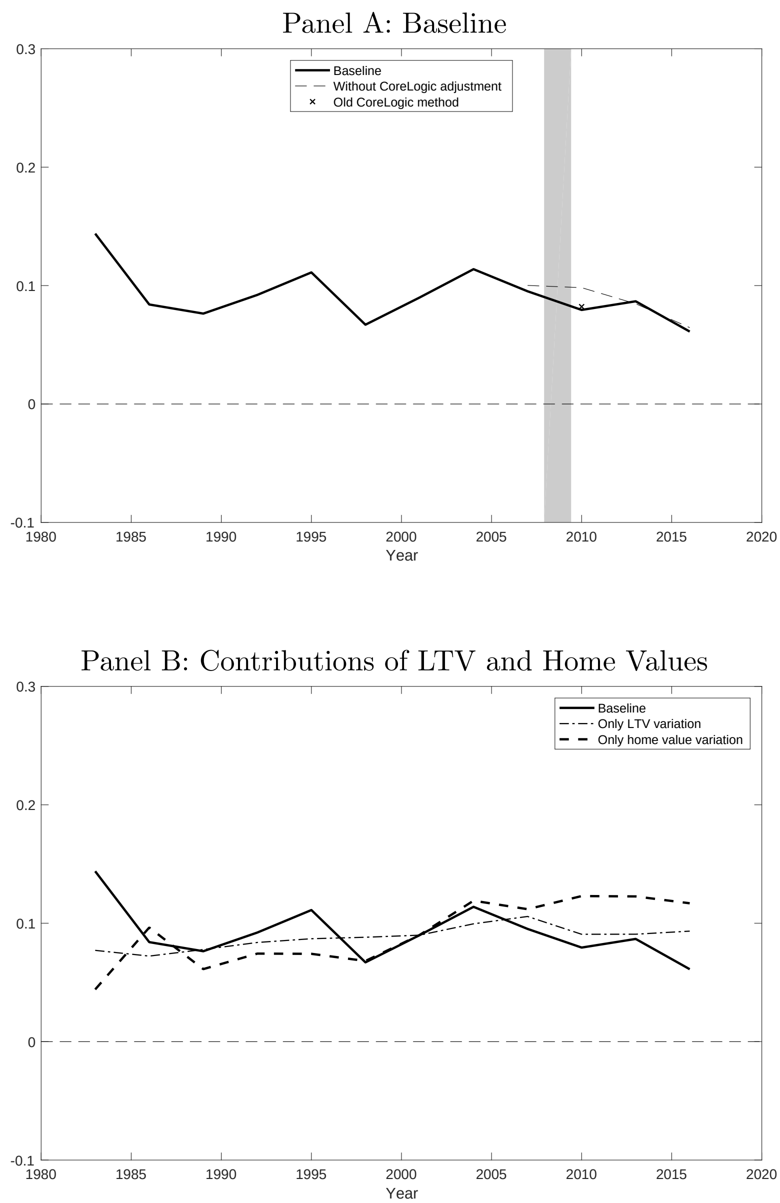


Figure 9: Model Housing Wealth Effect

Note: Panel (a) shows $(p/C)(dC/dp)$, where C is aggregate consumption in the population. C is calculated from $\int c(x,p)d\Phi_t(x)$, where the consumption function is the solution to the household's decision problem for a given relative price of housing and Φ_t is constructed from the SCF data for year t adjusted to match the CoreLogic Homeowner Equity Reports as explained in Appendix B.3. We use a finite difference derivative that averages the values of plus and minus 10% price changes. The dashed line shows the same elasticity without adjusting the LTV distribution to match the CoreLogic data. The cross at 2010 shows the same elasticity adjusting the LTV distribution based on the pre-2010 CoreLogic methodology. Panel (b) repeats the same calculation with counterfactual Φ_t's constructed as described in the text.

t we replace the LTV values with $F_t^{-1}(F_{2001}(LTV_{2001}))$ where $F_t(\cdot)$ is the CDF of the marginal distribution of LTV for year t. Intuitively, we preserve each household's rank in the 2001 LTV distribution, but alter the LTV distribution according to the marginal distribution of LTV in year t, holding all other state variables fixed. Panel B of Figure 9 also presents analogous results where we vary the marginal distribution of house values holding the distribution of all the other state variables fixed.

The dash-dot line in Panel B of Figure 9 shows the effect of changing only the distribution of LTVs on the housing wealth elasticity implied by the model. This line is quite flat. Evidently, even very large changes in the LTV distribution have limited effects on the housing wealth elasticity implied by the model. The housing wealth effect in this LTV counterfactual increases modestly between 1983 and 2010 and, in particular, it does not spike during the Great Recession despite a sharp increase in LTVs. These results differ dramatically from the common narrative that housing wealth effects may have been particularly high during the Great Recession due to sharp deleveraging by households in the face of large increases in LTVs. It is crucial to emphasize that these results are for the *elasticity* of consumption with respect to housing wealth; not the level of consumption.

The dashed line in Panel B of Figure 9 shows the effect of changing only the distribution of house values on the model-implied housing wealth elasticity. In contrast to changes in the LTV distribution, changes in house values do generate noticeable changes in the housing wealth elasticity. As Berger et al. (2017) have emphasized, when houses become a bigger part of the household balance sheet, a given percentage change in the value of housing becomes more important to consumption decisions, and the model implied housing wealth elasticity rises.

7.1 Why So Stable?

Why do large changes in the LTV distribution not lead to larger variation in the housing wealth elasticity in our model? To understand this, Table 5 decomposes the model-implied housing wealth effect for selected years. Specifically, the table shows the average elasticity within different groups of the population together with the relative size of each group.[32] Notice first that a substantial fraction of the overall housing wealth effect is driven by homeowners with relatively low-LTVs (below 60%) who remain in their homes despite the change in home prices. The average housing wealth elasticity for this group is between 0.1 and 0.2. This group has outsized importance because it accounts for

[32]We report $(p/C)(dc/dp)$, where C is average consumption in the population and c is group consumption. This means that the total elasticity can be calculated as the sum of the group elasticities multiplied by the group size.

Table 5: Decomposition of Housing Wealth Effect

	1986		2007		2010	
	Elas.	Group size	Elas.	Group size	Elas.	Group size
Renters (not moving)	0.01	0.30	0.00	0.30	0.00	0.31
Upsizers	-0.64	0.06	-0.89	0.04	-0.78	0.05
Downsizers	0.29	0.04	0.46	0.06	0.44	0.04
Stayers						
LTV ≤ 0.6	0.17	0.54	0.15	0.38	0.17	0.31
LTV $\in (0.6, 0.8]$	0.26	0.05	0.18	0.10	0.22	0.08
LTV $\in (0.8, 1.0]$	0.23	0.01	0.28	0.09	0.29	0.10
LTV ≥ 1.0	0.01	0.00	0.01	0.04	0.03	0.11
Total	0.084	1.00	0.095	1.00	0.079	1.00

Note: We classify people according to their housing tenure, moving decisions, and LTV and then compute aggregate consumption within each cell before and after a price change to compute the dc for the group. We report $(p/C)(dc/dp)$ where C is average consumption in the population. The aggregate elasticity is the dot-product of the group elasticities and group sizes. Group sizes refer to shares of the population in the SCF adjusted to match equity estimates for homeowners with a mortgage in the CoreLogic Homeowner Equity Reports.

for 38% of the population or equivalently 62% of homeowners in 2007. Since individuals in this group are not near the LTV constraint, their housing wealth elasticity is relatively insensitive to large changes in LTV.

Despite being far from their constraint, these low-LTV households have a fairly high elasticity. This arises partly because households react to an increase in house prices by substituting away from housing and toward other consumption goods. Moreover, households perceive themselves as being long housing and therefore feel richer when house prices rise and decide to spend some of that increased wealth. As is typical in incomplete markets models, households in our model are impatient in the sense that β is substantially below R^{-1}. This leads them to heavily discount the increase in implicit rents that result from increased house prices. It also implies that, unlike in a complete markets model in which the permanent income hypothesis holds, households have a relatively high marginal propensity to consume out of extra wealth even when they are not constrained. A key parameter in determining the housing wealth effect is consequently the discount rate β.[33] The size of housing transaction costs are also important as they temper the substitution effect.

The other factor that plays an important role in the stability of the elasticity of consumption with respect to house prices implied by our model – particularly in the Great Recession – is the fact that it does not rise monotonically with LTV but rather features a "hump" around the LTV constraint. The top panel of Figure 10 illustrates this by reporting the model-implied elasticity of

[33] Figure A.23 in the Appendix shows that raising β by 0.01 reduces the housing wealth elasticity by approximately 0.01.

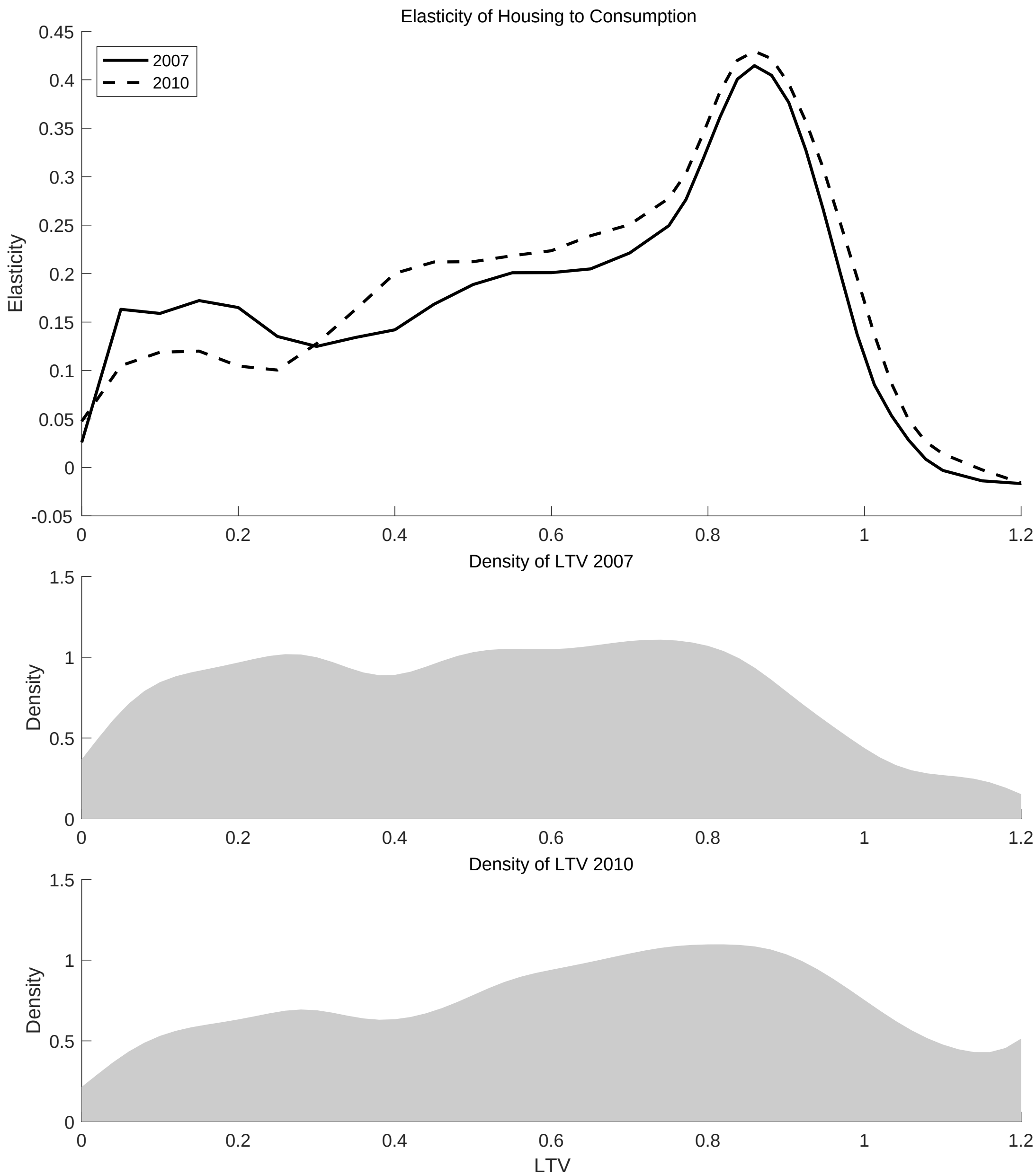


Figure 10: Housing Wealth Effect by LTV and Marginal Distributions of LTV in 2007 and 2010.

Note: The top panel shows the elasticity implied by the model for a particular LTV. For a given LTV, we weight households by their distance from that LTV using Gaussian kernel with bandwidth 0.05 and report the weighted average elasticity. The results of this calculation depending on the (conditional) distribution of other state variables for a given LTV and this accounts for the difference between 2007 and 2010. The lower panels show kernel density estimates of the LTV distribution using the same kernel and bandwidth.

consumption to house prices as a function of LTV. Households that have LTV ratios close to the LTV constraint tend to have low liquid assets and have a high marginal propensity to consume for precautionary reasons. At an LTV of 0.8, the LTV constraint binds, and the model implied housing wealth elasticity jumps and remains high until households reach an LTV of about 0.95. Intuitively, the households in this region tend to be highly financially constrained, and changes in the house price tighten or loosen these constraints. When house prices rise, these households respond by refinancing their mortgage, downsizing their house, or selling to rent, all of which allow them to increase consumption. Once the LTV ratio rises above roughly 0.95, however, the model-implied housing wealth elasticity drops rapidly. As Ganong and Noel (2017) have emphasized, households that are underwater on their mortgage are not able to access changes in housing wealth induced by changing house prices. Their LTV is too high for them to be able to refinance or sell their house unless they have other liquid wealth to help pay off their current mortgage. At the same time, these households are not forced to de-lever when their mortgage debt is long-term. They can simply pay their mortgage down over time. Their consumption is consequently highly insensitive to house price changes.

The hump in the model-implied housing wealth effect as a function of LTV means that as house prices fell during the Great Recession some households were pushed into the hump, but at the same time other households were pushed out the other side of the hump. The lower panels of Figure 10 illustrate this by plotting kernel density estimates of the LTV distribution of homeowners in 2007 and 2010. One can obtain the aggregate wealth effect for homeowners with a mortgage by integrating the function in the top panel with respect to these distributions. On net, the two effects roughly cancel, causing the overall housing wealth effect in the model to be insensitive to changes in the LTV distribution. While the extent to which these two effects cancel depends on the exact distributions, these two countervailing forces will generally help stabilize the housing wealth effect in a bust that pushes some homeowners underwater.

To further elucidate the changes in the Great Leveraging and Great Recession, Table 5 shows the fraction of households in low (≤ 0.6), medium $(0.6, 0.8]$, high $(0.8, 1.0]$, and underwater (≥ 1.0) LTV bins for 1986, 2007 and 2010.[34] Comparing 1986 and 2010, we see that 23 percent of the

[34]At first blush, the figures in Table 5 look different from many estimates of the prevalence of negative equity in the bust. The reason for this is that Table 5 reports group sizes as a fraction of the population, while most equity figures are for homeowners with a mortgage. For instance, CoreLogic estimates that in 2010Q4 23.2% of homeowners with a mortgage had negative equity. We use the share of households with a mortgage from the SCF to obtain that 11.7% of the population was a homeowner with negative equity. Table 5 shows 11% because some of the underwater households are allocated to the "downsizers" bin if they downsize, switch to renting, or default.

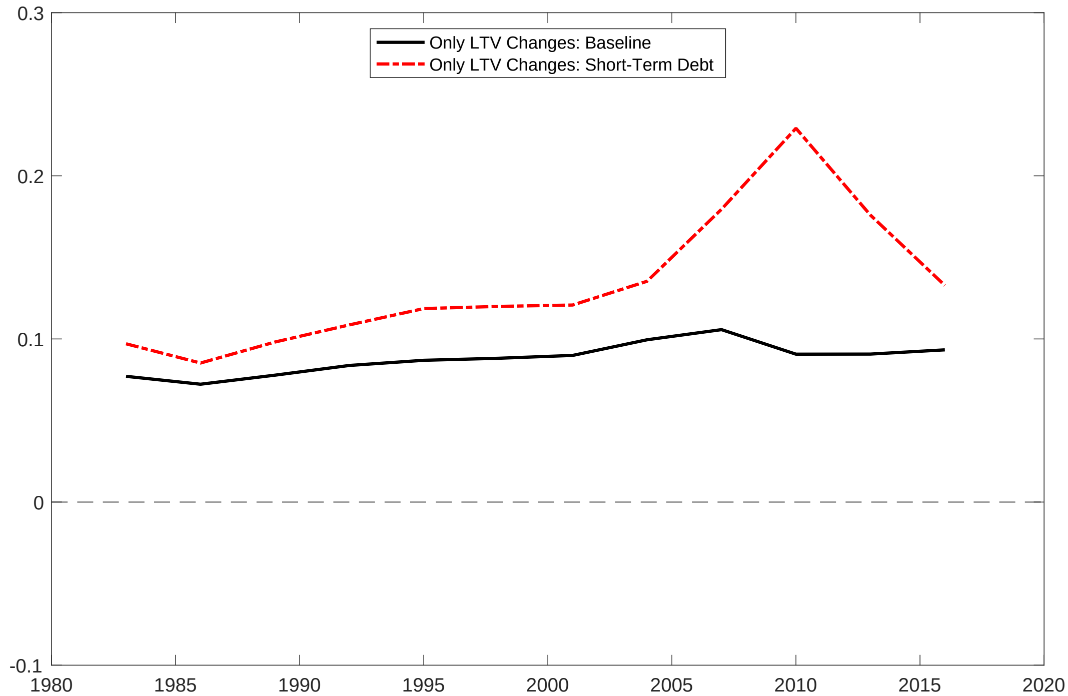


Figure 11: Short-Term Debt Counterfactual

Note: The figure shows the changes in the housing wealth effect that result from changes in the LTV distribution as in Panel (b) of Figure 9. The figure repeats the results from our baseline model and adds results from an alternative model in which the LTV constraint must hold every period not just at the origination of a mortgage.

population moved out of the low-LTV stayer category and were mostly pushed into the high-LTV and underwater categories. The high-LTV group grew by 9 percent of the population, while the underwater group grew by 11 percent of the population. The comparison between 2007 and 2010 is even more stark. The number of low-LTV households decreased by 7 percent and the number of underwater households increased by 7 percent, resulting in a net decline in the housing wealth effect.

The presence of long-term debt in our model is important for these results. In a model with short-term debt, all households must roll over their mortgage and satisfy the LTV constraint each period. This means that all households with high LTVs are forced to de-lever. This deleveraging when house prices fall creates a powerful housing wealth effect in the bust. Indeed, Appendix E.6 shows a version of the top panel of Figure 10 for a model with short-term debt. In this case, the model-implied housing wealth elasticity remains elevated for underwater homeowners. To illustrate these effects quantitatively, Figure 11 compares our baseline model to one with only short-term debt for the case where we allow only the marginal distribution of LTV to change over time. With short-term debt, the increase in LTVs in the Great Recession leads to a sharp increase in the model-implied housing wealth elasticity and a boom-bust asymmetry, in contrast to both our empirical analysis and our model with long-term debt.

7.2 Credit Conditions and the Housing Wealth Effect

Our analysis above abstracts from variation in credit constraints over time. Part of the reason why household leverage rose as much as it did in the early 2000s may have been because of increased credit availability. To assess how such changes might affect our results, consider a scenario in which the LTV limit rises from 80% to 95% and the mortgage origination cost is reduced to zero. We refer to this parameterization as the "boom" parameterization. Figure 12 compares the housing wealth effect for our baseline parametrization and the boom parametrization. In both cases, we are using the distribution of state variables (LTV, etc) observed in the SCF for each year. This implies that the difference between the housing wealth elasticity in the baseline and the boom case measures the effect of relaxing credit constraints substantially for a given set of households.[35] The striking result in Figure 12 is that relaxing credit constraints has minimal effect on the housing wealth effect in our model. The key intuition is very similar to the intuition for the stability of the wealth effect when the LTV distribution changes: A substantial part of the wealth effect is driven by unconstrained households. Furthermore, shifting the location of the credit constraint shifts the location of the "hump," leading to offsetting effects as households move in and out of the hump.

This finding may seem surprising in light of analyses such as Guerrieri and Lorenzoni (2017), who show that tighter credit conditions can lead to an economic contraction, or Landvoigt, Piazzesi, and Schneider (2015), who show that cheap credit for poor households was a driving force in the house price boom in San Diego. Note, however, that our object of interest is not the *level* of consumption– which is sensitive to credit conditions in our model– but the *sensitivity* of consumption to home price changes, which is not. Also, our analysis takes the distribution of individual states as given from the data, so we are not changing these state variables as we change credit conditions.

7.3 No Short-run Housing Adjustment

A potential concern with our partial equilibrium analysis above is that changes in house prices lead to non-trivial changes in the demand for housing. Intuitively, households would like to substitute away from houses and towards other consumption goods when the relative price of houses

[35] Conditional on a household's current state variables, the constraints faced in the past are irrelevant to current decisions. Therefore the two lines in Figure 12 show the effect of a permanent change in credit conditions for the immediate consumption response.

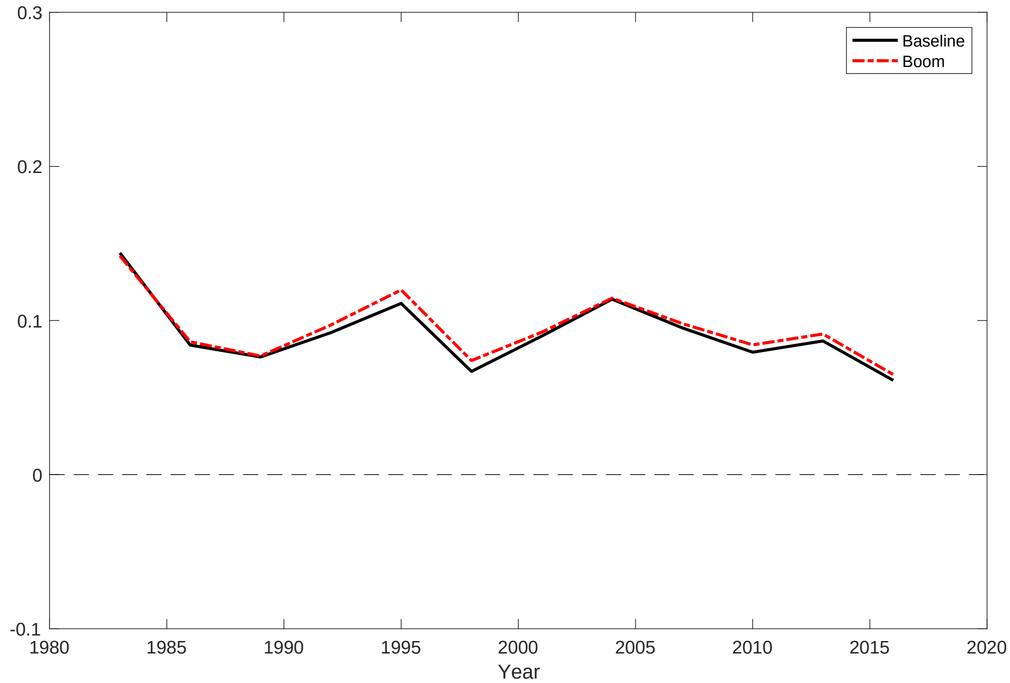


Figure 12: Housing Wealth Effect Under Boom Parameters

Note: The figure shows the housing wealth effect under the boom parameters: no mortgage origination cost, and an LTV limit of 95%.

increases.[36] In the short run, however, housing supply is quite inelastic both because construction of new houses takes time and also because each year's construction of new houses represents a small addition of the overall housing stock. This implies that the change in housing demand implied by our partial equilibrium model is unlikely to be consistent with market clearing in the housing market.

To address this concern, we consider an alternate experiment in which there is no change in housing demand in the short run. We compare two cities that both have completely inelastic housing supply in the short run, but have different long-run housing supply elasticities. These cities are hit by an aggregate shock that leads house prices to rise by 10% more in the less-elastic city in the long run. In the short run, house prices are endogenously determined so that housing demand is unchanged, i.e., the housing market clears with no change in the housing stock. The short-run equilibrium increase in house prices in the less-elastic city is less than 10%, leaving an expected capital gain that dissuades households in that city from reducing their housing demand despite its higher price.[37]

[36]Our baseline model implies a price elasticity of housing demand of 0.15.

[37]This experiment was motivated in part by the belief shocks of Kaplan, Mitman, and Violante (2017), which raise expectations of future housing demand without affecting current preferences. In their general equilibrium model, the

Figure A.29 in the Appendix compares the model-implied housing wealth effect for this alternate experiment to our baseline model-implied housing wealth elasticity. The demand for housing is highly sensitive to expected capital gains. Even a small expected capital gain is sufficient to equilibrate the housing market without any change in quantity of housing. The housing wealth elasticity in this alternate experiment is virtually identical to our baseline. Hence, while expected capital gains are a powerful force in affecting the quantity of housing demanded, they have little effect on the housing wealth elasticity.

7.4 Is the Model Consistent with the Evolution of Household Balance Sheets?

Our theoretical analysis uses the observed distribution of idiosyncratic states instead of the distribution that is generated by the model. Two considerations motivate this approach. First, it allows us to analyze the consequences of changes in household balance sheets while remaining agnostic as to the driving forces behind those changes. Second, this approach allows us to match the distributions of individual states observed in the data as closely as possible. However, a reasonable question to ask is whether our model is in fact consistent with the observed evolution of household balance sheets.

To address this question, we simulate the model's predictions for the evolution of the LTV distribution in response to the changes in aggregate home prices we observe in the data. We begin the simulation at each wave of the SCF and ask how the model's predictions compare to the next wave of the SCF. For example, we start with the 1983 data and simulate the model using changes in aggregate home prices for 1983-1986. We then compare the model's predictions to the SCF data in 1986. We present the results of this analysis in Figure A.30 in the Appendix. Our baseline model largely succeeds in fitting the evolution of leverage from the late 1980s until the mid-1990s and during the Great Recession. The baseline model is less successful during the 1998-2007 period when the LTV distribution was stable despite a large run-up in house prices.

We can explain the evolution of leverage during the 1998-2007 period if we allow for two additional plausible features of the housing boom. First, we allow for a relaxation of credit standards by assuming that the LTV limit rises to 95% and that mortgage origination costs fall to zero for the years 1998-2007, as in the "boom parameterization" discussed above. Second, we allow for increased optimism of home owners about house prices by assuming one-year expected capital gains that rise from 0% to 2% from 2004 to 2007. This is motivated by Kaplan, Mitman, and Violante

belief shock raises current prices through expectations of future capital gains.

(2017) who argue that expected capital gains are central to fitting the evolution of house prices and leverage during the 2000s boom-bust episode.

Figure A.31 shows that if we incorporate these two features, we can fit the stability of the LTV distribution during the boom despite the large increase in house prices. Intuitively, both features make households more aggressive in borrowing against their houses, and this explains why house price appreciation did not mechanically lower LTV's. Nevertheless, these modifications have a very limited effect on the housing wealth elasticity. Credit constraints have little effect on the elasticity, as we discuss in Section 7.2, while allowing for a modest increase in expected capital gains also has a small effect, increasing the housing wealth effect in 2007 by 16 percent. Appendix E.9 provides additional details of these simulations. We conclude that modifying our model to fit the evolution of the states in the boom has little effect on our main conclusions about the housing wealth elasticity.

7.5 Extensions and Robustness

We explore how our results are affected by several other modeling choices in the Appendix. We show that the housing wealth effect is modestly increasing in mortgage interest rates, which we set at 3% in our baseline specification (see Figure A.25). At higher interest rates, households are more likely to downsize their homes and downsizers have large elasticities. This is particularly true of high-LTV households, who were more common in recent years.

Our baseline analysis assumes that rents are proportional to home prices. During the housing boom of the 2000s, the rent-price ratio fell considerably. Making the polar opposite (and also unrealistic) assumption that rents remain constant when home prices change yields somewhat higher housing wealth effects because it leads renters to defer buying a house and spend more on non-housing consumption. However, this alternate rent assumption does not change the time series pattern of the wealth effect (see Figure A.26).

A larger cost of selling a home reduces the housing wealth effect in our model. As the transaction cost increases, homeowners become less willing to realize the capital gain or loss on their houses and consumption becomes more insulated from price fluctuations. While the moving cost affects the level of the housing wealth effect, it does not change the time series pattern (see Figure A.28).

Households can default subject to a utility cost in our model, and we set the utility cost at a level such that the default option is rarely exercised. However, our results are essentially unchanged if we make default more attractive. This finding is related to the insensitivity of our results to credit constraints: the housing wealth effect is heavily influenced by low-LTV households.

8 Conclusion

We present new evidence on the housing wealth effect going back to the 1980s. To do so, we develop a new identification approach that exploits the differential sensitivity of house prices in cities to regional house price cycles. This allows us to obtain estimates of the housing wealth effect using a consistent methodology over a long time period. Our estimate of the housing wealth effect is substantially more precise and smaller than recent estimates, though they remain economically important. We find no evidence that the elasticity to changes in house prices was particularly large in the 2000s; if anything, the elasticity was larger prior to 2000. We also find no evidence of a boom-bust asymmetry.

We develop a theoretical framework to interpret our empirical estimates. We show that our empirical approach yields an estimate of the partial equilibrium effect of house prices on consumption multiplied by a local general equilibrium multiplier that can be approximated by the local fiscal multiplier. All other general equilibrium effects are soaked up by fixed effects in our regressions. Our empirical approach thus allows us to draw inferences about the effects of house price fluctuations while remaining agnostic about fundamental shocks that drive house prices. We then analyze the partial equilibrium multiplier in the context of a partial equilibrium life-cycle model with borrowing constraints, uninsurable income risk, illiquid housing, and long-term mortgages.

In the model as in the data, there is little response of the housing wealth effect to shifts in the LTV distribution — even the large run-up in household leverage as house prices fell during the Great Recession. This arises for two reasons. First, much of the housing wealth elasticity arises from a substantial housing wealth effect of impatient low-LTV households. Second, while an increase in the number of highly constrained households during the Great Recession tended to raise the elasticity, this was offset by an increase in the number of underwater households whose consumption is very unresponsive to house prices. Both our empirical analysis and model thus imply that the housing wealth elasticity is a fundamental feature of the economy that was not driven by special features of the 2000s boom-bust cycle.

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Appendices

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. The log changes thus begin in 1976 with the difference between 1976 and 1975 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

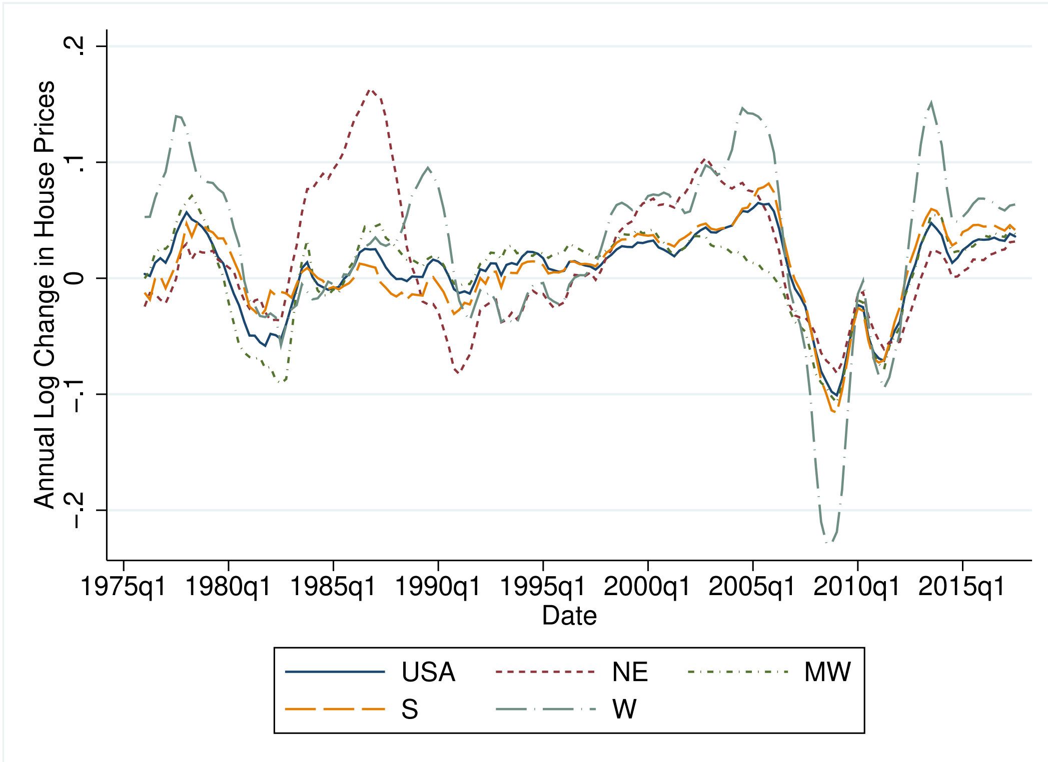

Figure A.1: Regional House Price Indexes

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

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.

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:

$$\Delta \log \bar{C}_{i,t} = \xi_i + \zeta_t + \beta \Delta \log \bar{Y}_{i,t} + \varepsilon_{i,t}, \tag{7}$$

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 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

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

	1	2	3	4
	OLS	IV	OLS	IV
	Total Cons	Total Cons	Ex Imputed Rent	Ex Imputed Rent
Retail Emp.	0.460**	0.940**	0.521*	0.969**
Per Capita Growth	(0.179)	(0.314)	(0.230)	(0.400)
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 (7). 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.

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). The IV coefficient is a ratio of the covariances of retail employment with house prices, and consumer expenditures with house prices.

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.

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 $\log y = \ell + z + \xi$,

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

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 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).

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, housing values and homeownership across ages. The calibrated model predicts a refinancing rate

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

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 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

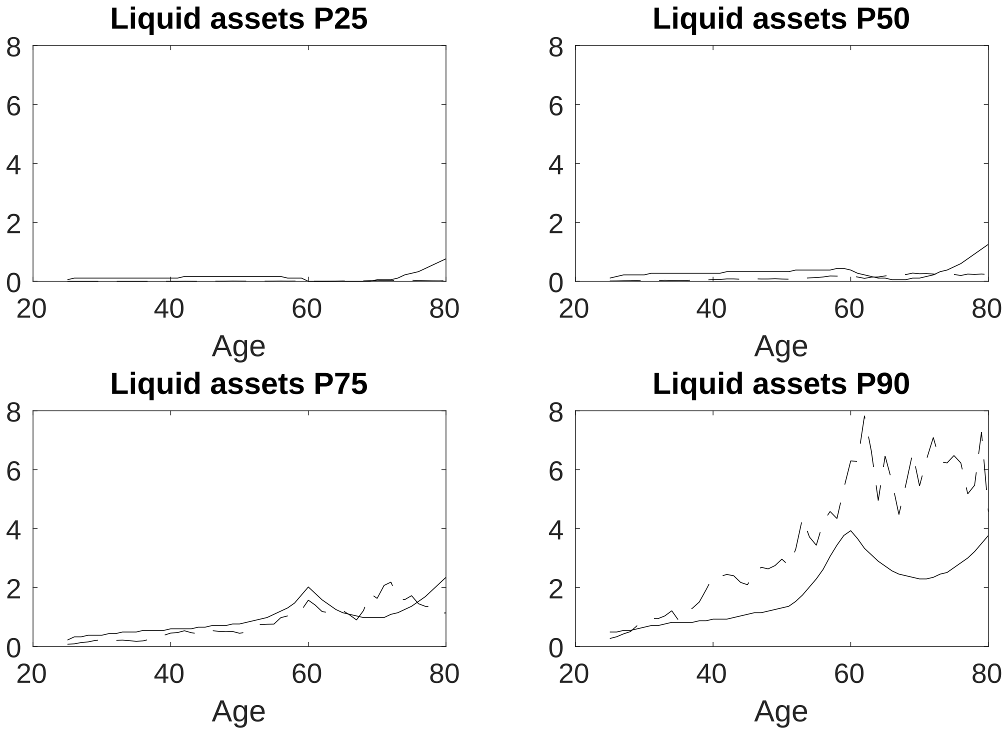

Figure A.2: Liquid Asset Holdings By Age for Model (Solid) and Data (Dashed)

Note: Data refer to SCF 2001.

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 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%,

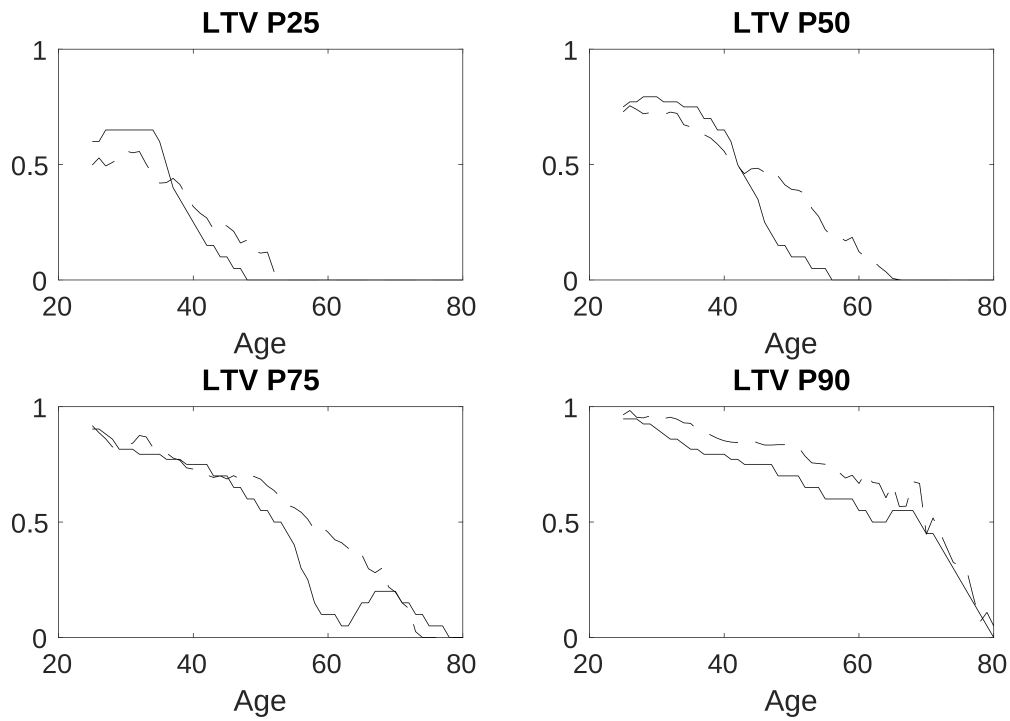

Figure A.3: LTV By Age for Model (Solid) and Data (Dashed)

Note: Data refer to SCF 2001.

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 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

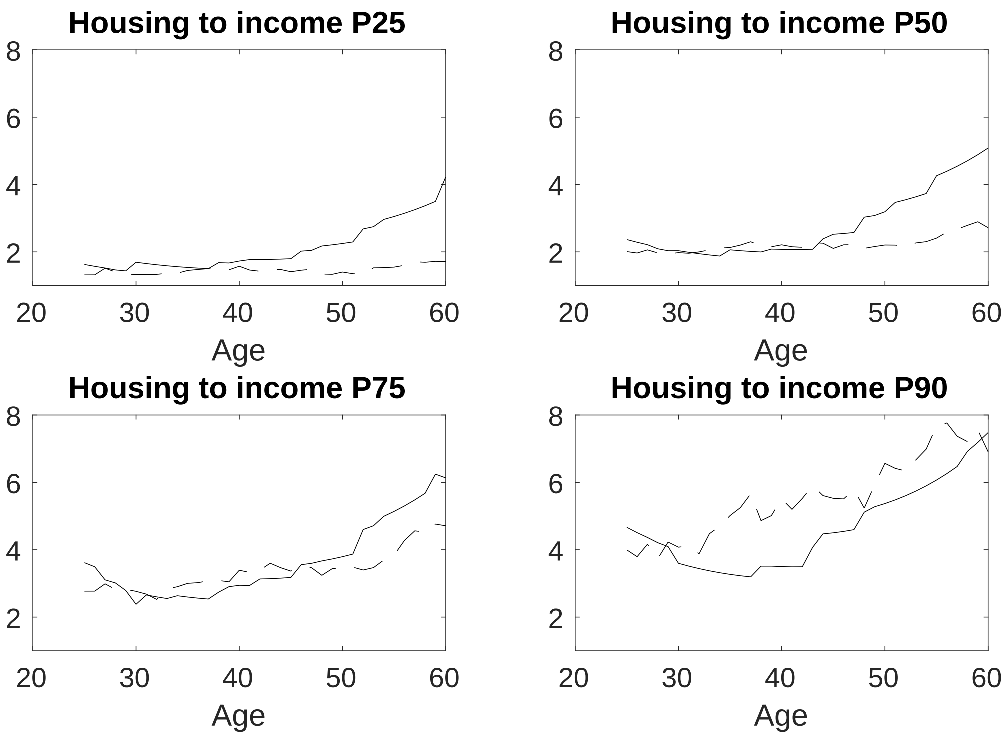

Figure A.4: Housing-to-Income By Age for Model (Solid) and Data (Dashed)

Note: Data refer to SCF 2001. Sample restricted to working-age homeowners.

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:

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

where w is defined as:

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

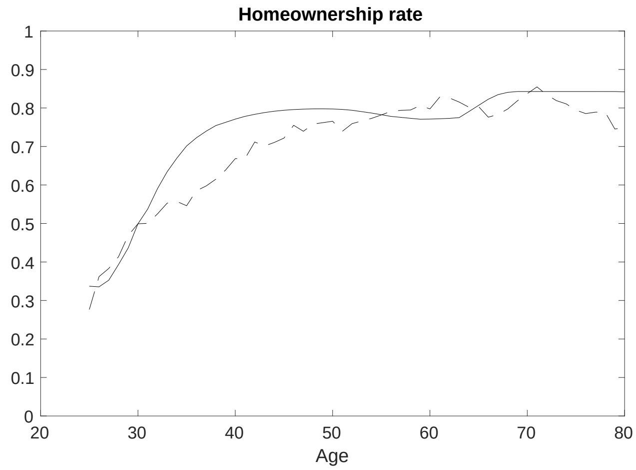

Figure A.5: Homeownership By Age for Model (Solid) and Data (Dashed)

Note: Data refer to SCF 2001.

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:

$$\Delta y_{i,r,t} = \psi_i + \xi_{r,t} + \beta \Delta p_{i,r,t} + \alpha_i \mathcal{E}_{r,t} + \varepsilon_{i,r,t}, \tag{8}$$

$$\Delta p_{i,r,t} = \varphi_i + \zeta_{r,t} + \delta \Delta y_{i,r,t} + \gamma_i \mathcal{V}_{r,t} + \nu_{i,r,t}. \tag{9}$$

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 (8) 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.[38]

Equations (8) and (9) 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 (8). However, changes in local employment also affect local house prices through the $\delta \Delta y_{i,r,t}$ term in equation (9). Since causation between local employment and house prices runs both ways, estimating equation (8) by OLS will

[38] 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 (8) and (9) is not as explicit about this heterogeneity in sensitivity.

yield a biased estimate of β. The classic approach to solving this problem is to look for a variable that shows up in equation (9) but not in equation (8) and to use this variable as an instrument for $\Delta p_{i,r,t}$ when estimating equation (8). 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 (2) and use $z_{i,r,t} = \hat{\gamma}_i \Delta P_{r,t}$ as an instrument for $\Delta p_{i,r,t}$ in equation (8). 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 (9), but that local retail employment does respond differentially to regional shocks through heterogeneity in α_is in equation (8). 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 (9). Were we to estimate equation (2) 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 (8). 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.

To address this problem, consider the following more sophisticated identification strategy. First aggregate equation (9) 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 (9) as:

$$\Delta p_{i,r,t} = \varphi_i + \tilde{\zeta}_{r,t} + \delta \Delta y_{i,r,t} + \frac{\gamma_i}{\gamma_r} \Delta P_{r,t} - \frac{\gamma_i}{\gamma_r} \delta \Delta Y_{r,t} + \nu_{i,r,t}. \tag{10}$$

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 (8). 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 (10). A slightly more general version of equations (8) and (9) allows for heterogeneity in the response of house prices to local employment, which replaces δ in equation (9) with δ_i. In this case, the coefficient on $\Delta y_{i,r,t}$ in

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	Raleigh, NC	Albuquerque, NM
	Harrisburg, PA	Indianapolis, IN	Baton Rouge, LA	Salt Lake City, UT
	Rochester, NY	Omaha, NE	Little Rock, AR	Colorado Springs, CO
	Scranton, PA	Youngstown, OH	Jackson, MS	Denver, CO
	Buffalo, NY	Columbus, OH	Greensboro, NC	Portland, OR
⋮				
	Bridgeport, CT	Toledo, OH	Jacksonville, FL	Fresno, CA
	Worcester, MA	Chicago, IL	Sarasota, FL	Las Vegas, CA
	New Haven, CT	Milwaukee, WI	Tampa, FL	Sacramento, CA
	New York-Newark, NY	Minneapolis, MN	Orlando, 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.

equation (10) 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 (8). Because we control for $\Delta y_{i,r,t}$ and $\Delta Y_{i,r,t}$ in defining the instrument in equation (10), 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.

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 rolling windows analysis in Figure 5. It 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, 5-Year Windows, Alternative Controls and Variable Construction

D.1.4 Single Cross Section Results

D.1.5 Alternate House Price and Employment Data

D.1.6 Sampling Error in Gammas

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 (3). 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 (3), 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 effect 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. Without controls, the pooled estimate of the elasticity post-1990 is 0.088 rather than 0.071.

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.

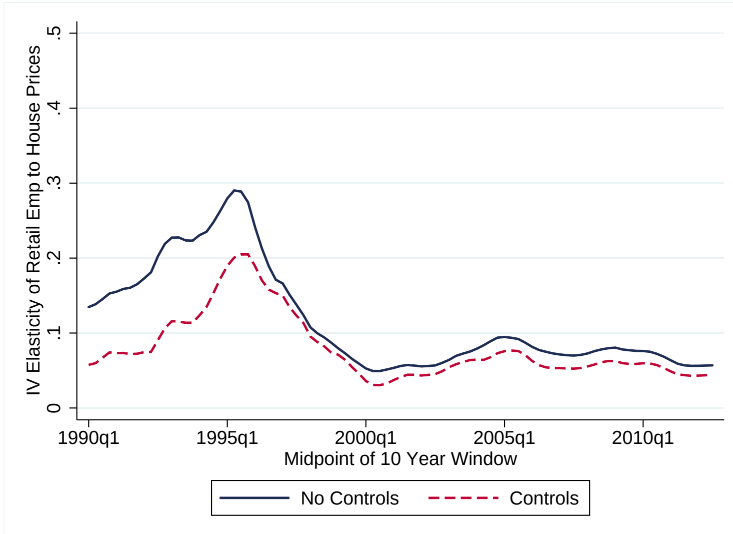

Figure A.6: Elasticity of Retail Employment Per Capita to House Prices: No Controls vs. Controls
Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods as in Figure 5 in red (dashed) and a version without the controls 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 5 and are not shown so that the comparison between the two specifications is clearer.

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 5. 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.7: First Stage and Reduced Form

Note: Panel A plots the first stage and panel B plots the reduce form of Figure 5. 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 5 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 region-time.

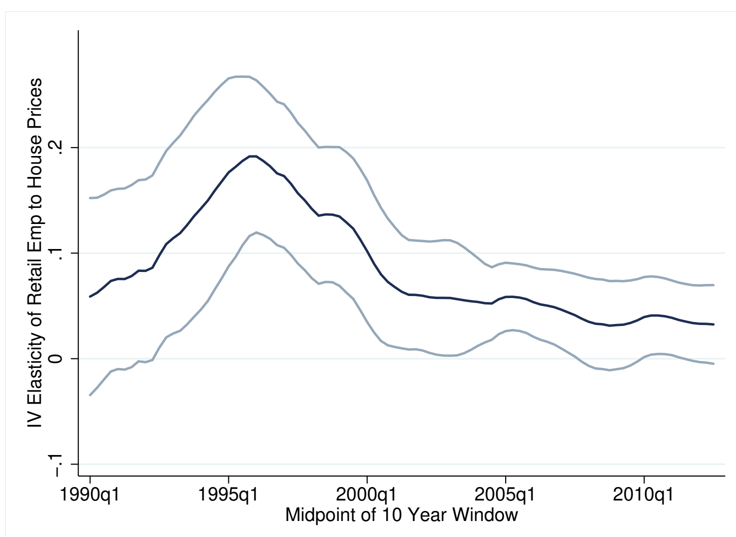

Figure A.8: Elasticity of Retail Employment Per Capita to House Prices: 3-Year Differences

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 5 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 region-time.

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 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 (3) and in constructing the controls for differential city-level exposure to regional retail employment, real 30-year mortgage rates, and the Gilchrist-Zakrajek excess bond premium using equation (4). 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

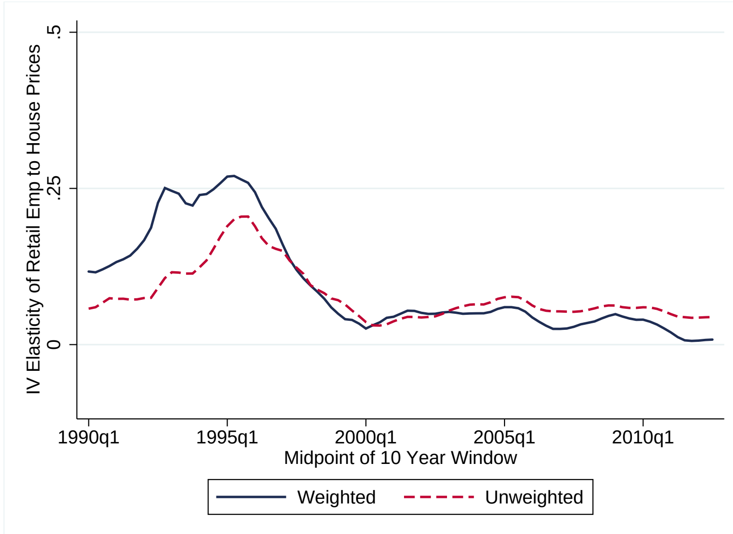

Figure A.9: Elasticity of Retail Employment Per Capita to House Prices: Population Weighting

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 5 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 5 and are not shown so that the comparison between the two specifications is clearer.

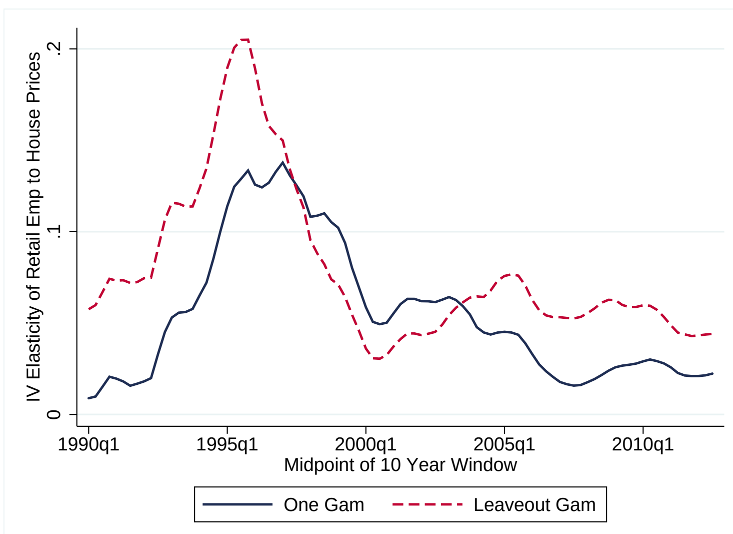

Figure A.10: Elasticity of Retail Employment Per Capita to House Prices: Single Gamma

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 5 in red (dashed). The specification in blue is the same except that γ_i is estimated for each CBSA by equation (3) 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 5 and are not shown so that the comparison between the two specifications is clearer.

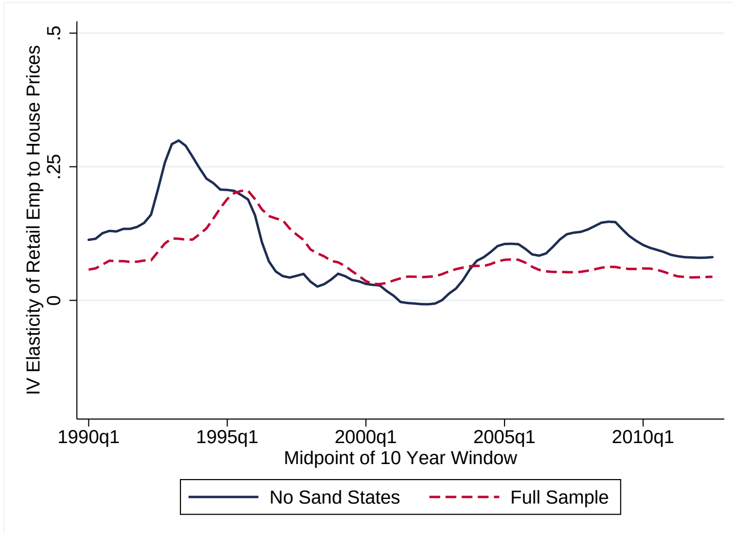

Figure A.11: Elasticity of Retail Employment Per Capita to House Prices: No Sand States

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 5 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 5 and are not shown so that the comparison between the two specifications is clearer.

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.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.

Finally, Figure A.15 shows results for 5-year windows This finer resolution does not yield evidence that the elasticity rose significantly during the2000s. 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.16 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

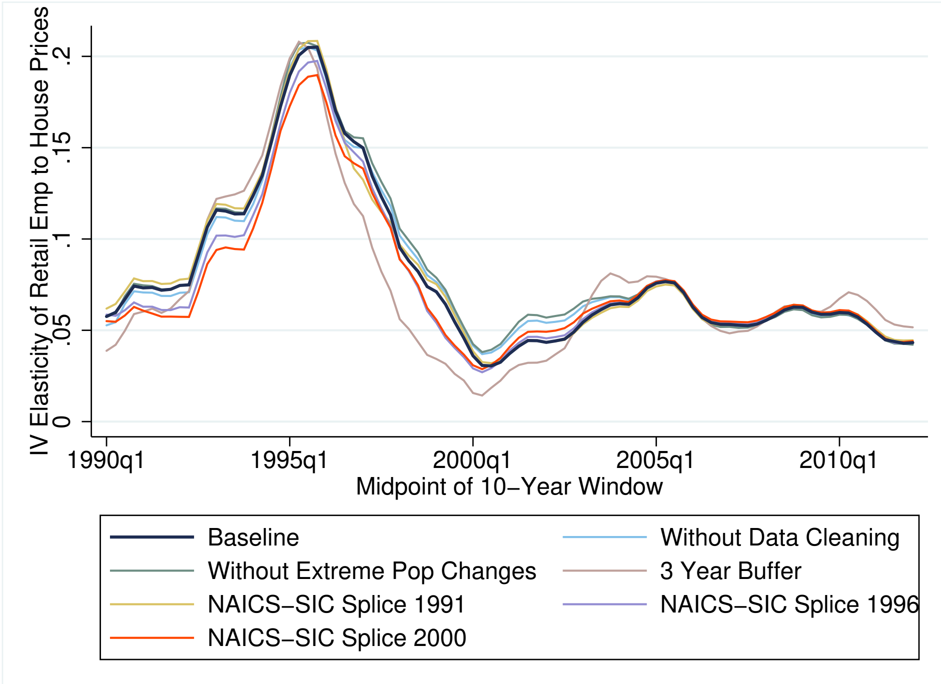

Figure A.12: Elasticity of Retail Employment Per Capita to House Prices: Misc. Robustness Tests

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 5 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 5 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 (3) and for the regressions as in equation (4) 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.

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 1976-2017.

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 effect to be greater from 1990 to 2003 than it was in the 2000s.

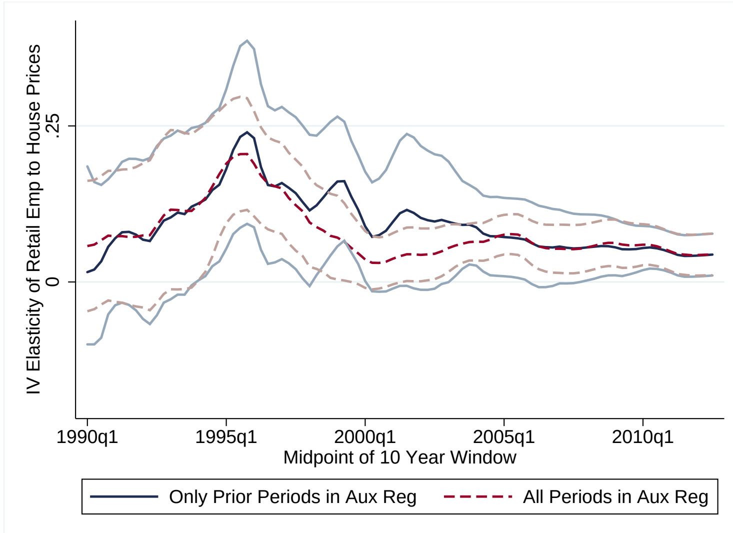

Figure A.13: Elasticity of Retail Employment Per Capita to House Prices: Only Prior Periods To Create Instrument

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 5 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.

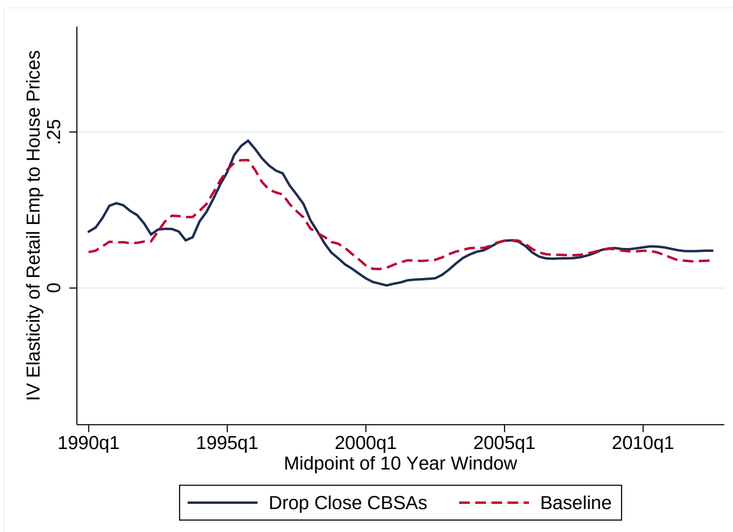

Figure A.14: Elasticity of Retail Employment Per Capita to House Prices: Dropping CBSAs Within 250 Miles For Regional

Note: The figure plots the elasticity of retail employment per capita to real house prices at the CBSA level as in Figure 5 except that the regional indices used for each CBSA do not include any CBSAs within 250 miles rather than only dropping the CBSA in question. The figure starts with the 2000-2005 window due to wide standard errors in the 5-year specification prior to 2000. 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 region-time.

Figure A.15: 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 5 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 region-time.

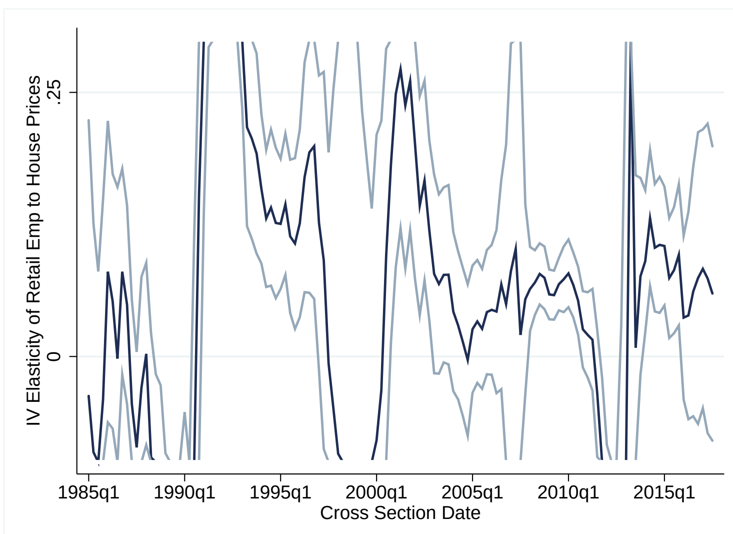

Figure A.16: Repeated Cross-Sections: 3 Year Differences

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 1976-2017 sample. The figure reports 95% confidence intervals in addition to point estimates for the elasticity. We report robust standard errors.

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.17 and D.1.5 instead uses 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 data. In Figure A.17, we use only the CoreLogic data dropping any imputed observations in an unbalanced panel. In Figure D.1.5, 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.19 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.1.6 Accounting for Sampling Error in the γ_is

We use two-way clustered standard errors by CBSA and region-time in most of our figures and our pooled analysis in Table 3. These standard errors do not account for sampling error in the estimation of the γ_is. To evaluate whether accounting for sampling error in the γ_is would significantly change our standard errors, Figure A.20 compares a specification that uses parametric standard errors clustering by time only with bootstrapped standard errors that block-bootstrap by time and account for the sampling error in γ_i.[39] To minimize computational burden, we use a specification without

[39] One can only block-bootstrap on one dimension. We choose time because block-bootstrapping by CBSA would involve drawing CBSAs randomly and each CBSA would have the same γ_i that it would in the baseline procedure

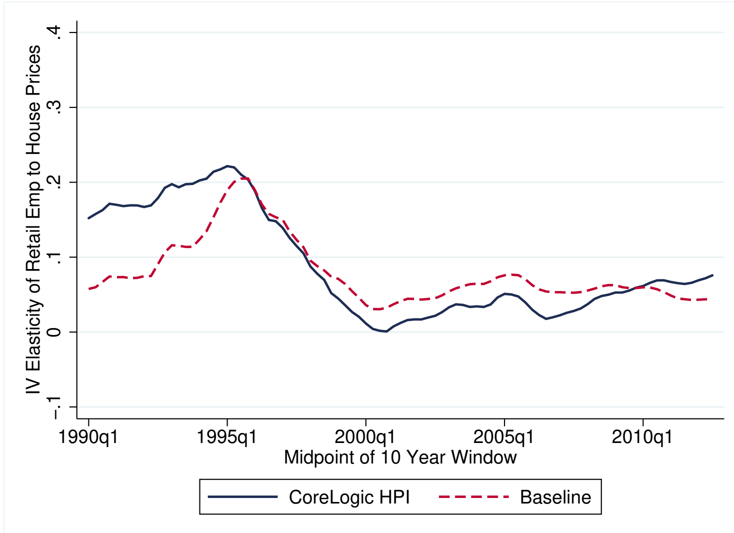

Figure A.17: Alternate Data: CoreLogic House Price Data (Unbalanced Panel)

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 5 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. 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 region-time.

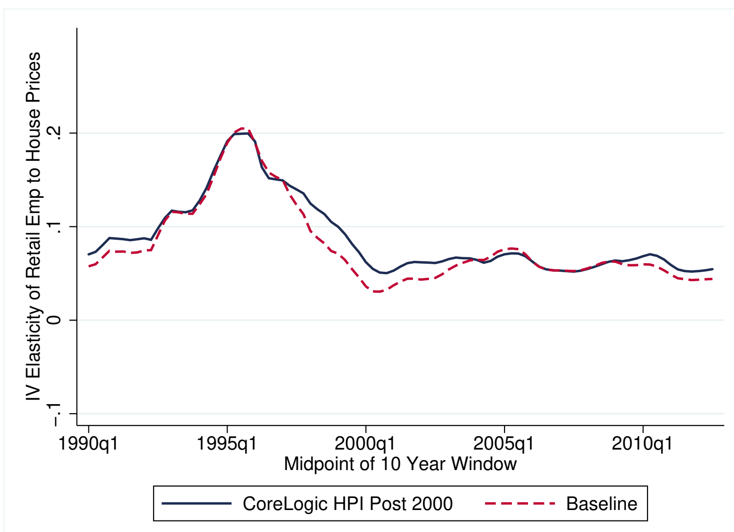

Figure A.18: Alternate Data: Freddie Mac Pre-2000, CoreLogic Post (Balanced Panel)

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 5 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. 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 region-time.

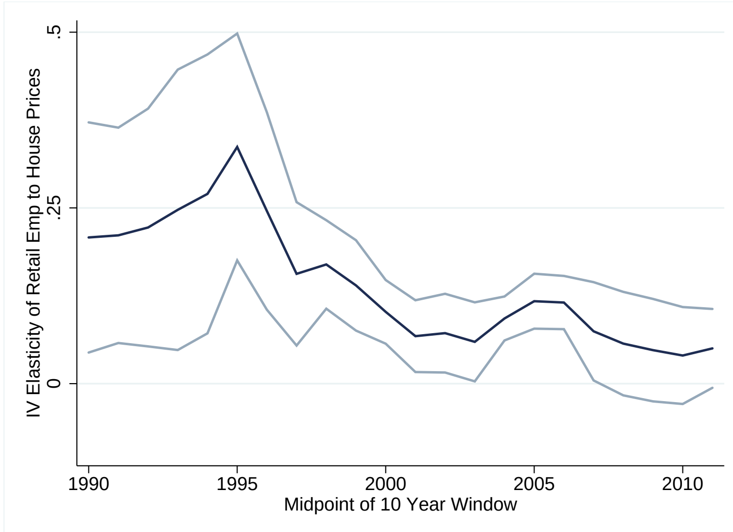

Figure A.19: Alternate Data: County Business Patterns Employment Data

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 5 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 region-time.

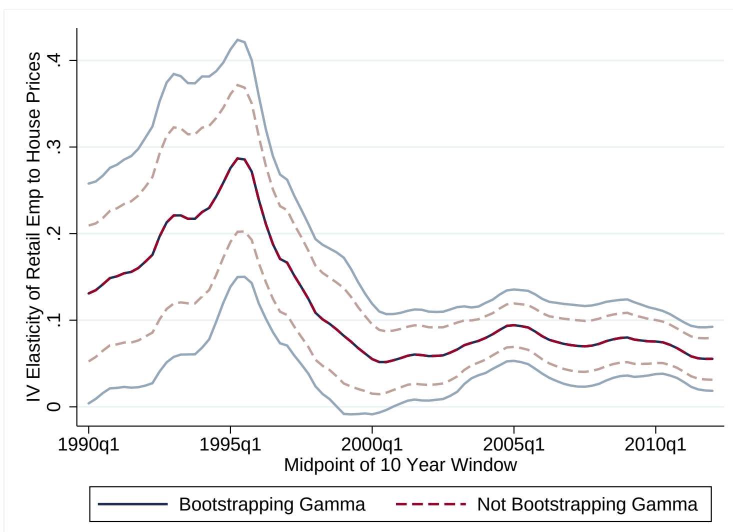

Figure A.20: Accounting for Sampling Error in the γ_is

Note: The dark blue line plots the elasticity of retail employment per capita to real house prices at the CBSA level for rolling 10-year sample periods. The dashed light red lines indicate 95% confidence intervals clustered by time. The light blue lines indicate 95% confidence intervals for an identical specification that block bootstraps by time to account for sampling error in the estimation of the γ_is. 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. This specification includes no controls to reduce the computational burden.

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

	(1)	(2)	(3)
$\Delta \log(P)$	0.059**		
	(0.018)		
$\Delta \log(P) -$		0.048	
		(0.025)	
$\Delta \log(P) +$		0.068**	
		(0.026)	
P Test for Equality		0.577	
$\Delta \log(P)$			0.057**
			(0.019)
$\Delta \log(P)^2$			0.014
			(0.055)

Note: For these estimates, we first construct our instrument for each quarter by estimating the γ_i's in equation (3) 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 region-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)
$\Delta \log(P)$	0.081***		
	(0.014)		
$\Delta \log(P) -$		0.070***	
		(0.020)	
$\Delta \log(P) +$		0.090***	
		(0.020)	
P Test for Equality		0.477	
$\Delta \log(P)$			0.079***
			(0.016)
$\Delta \log(P)^2$			-0.029
			(0.042)

Note: For these estimates, we first construct our instrument for each quarter by estimating the γ_i's in equation (3) 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 region-time and CBSA level. All regressions do not include the standard controls in our baseline specification.

controls. The Figure shows that accounting for sampling error in γ_i increases the standard errors 20 to 30 percent. However, the results remain statistically significant at standard confidence levels, and the time series patterns of the standard error bands are unchanged.

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. In no specification do we find any strong evidence of a boom-bust asymmetry.

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

E.9 Accounting for the Evolution of Household Balance Sheets

under such a sampling scheme. Any sampling error in γ_i would thus come from the time dimension.

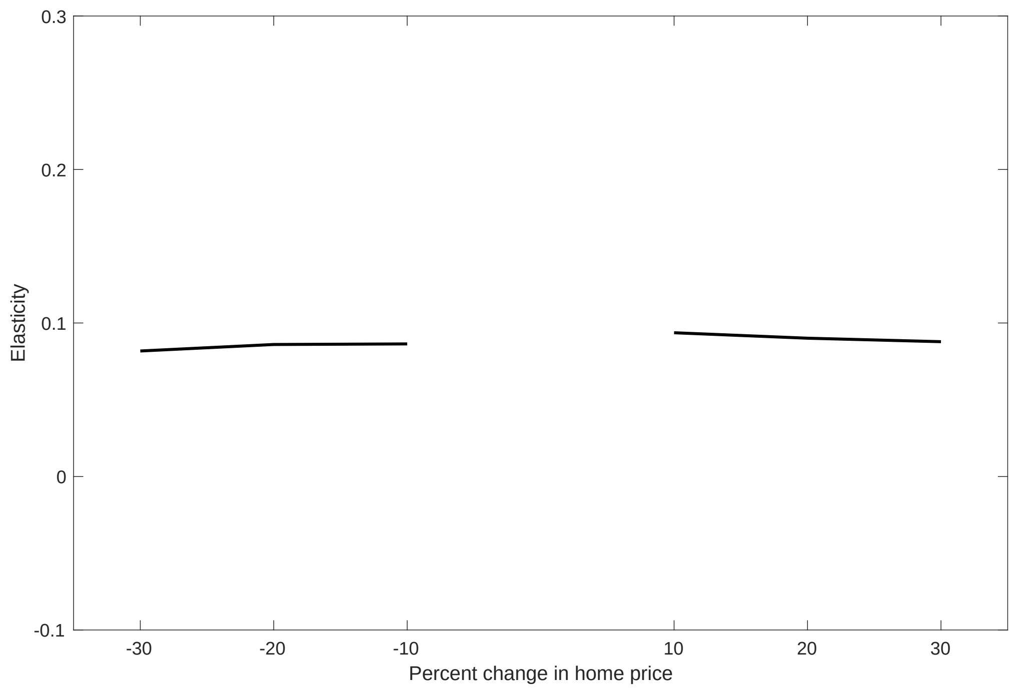

Figure A.21: Linearity of Housing Wealth Effect

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

E.1 Linearity and Interaction Effects

Figure A.21 shows model estimates of the housing wealth effect for both positive and negative changes larger than the 10% changes we use in our main analysis. The figure shows that the housing wealth effect 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 effect does not change meaningfully as we change ω. This implies that the cross derivative $d^2C/(dpd\omega)$ is small.

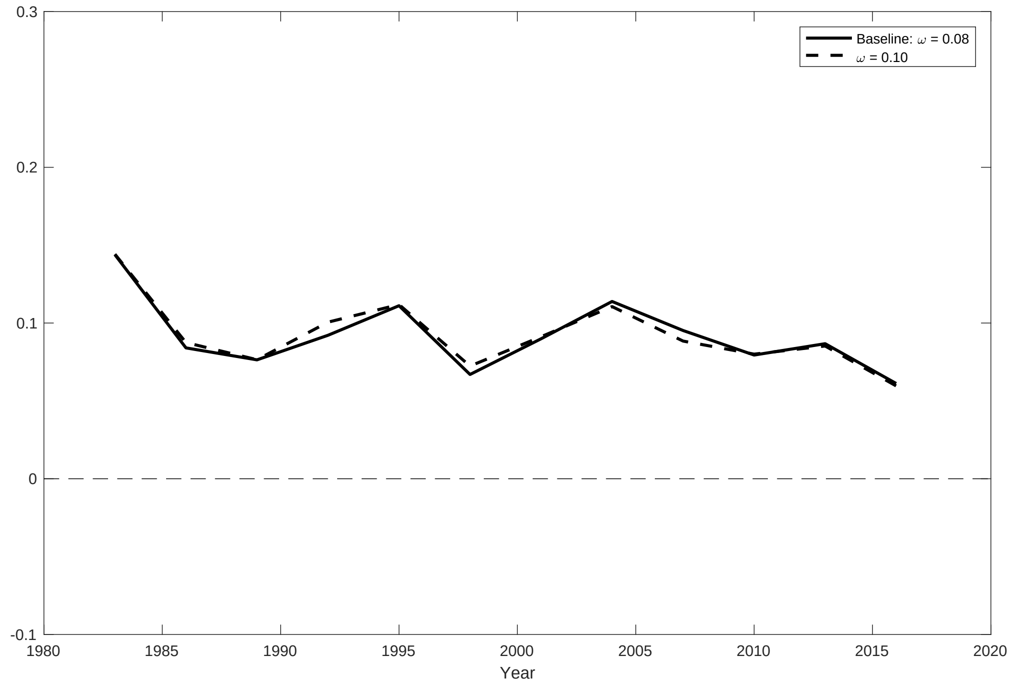

Figure A.22: Housing Wealth Effect For Alternative ω

Note: The figure shows the housing wealth effect 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 effect 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 effect, 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).[40]

[40]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

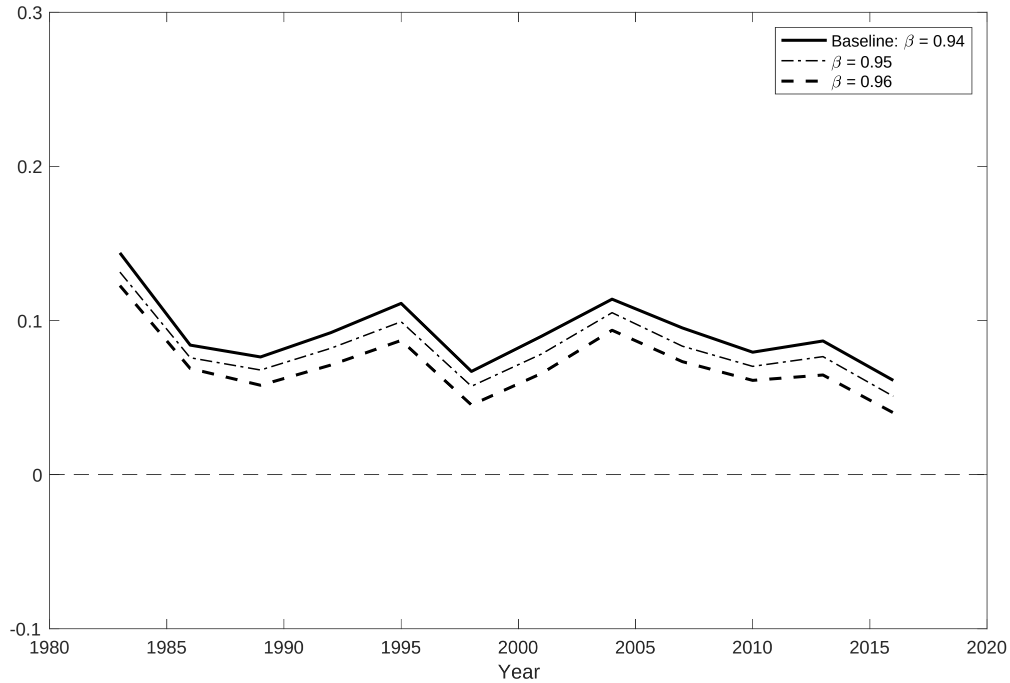

Figure A.23: Housing Wealth Effect with Different Discount Rates

Note: The figure shows the same calculation of $(p/C)(dC/dp)$ as in Panel (a) of Figure 9 using an alternate β parameters for the household's decision problem.

Figure A.24 shows that the housing wealth effect is barely changed by shifts in the LTV constraint. The intuition in Table 5 is useful to understand these results: a large part of the housing wealth effect 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.

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.

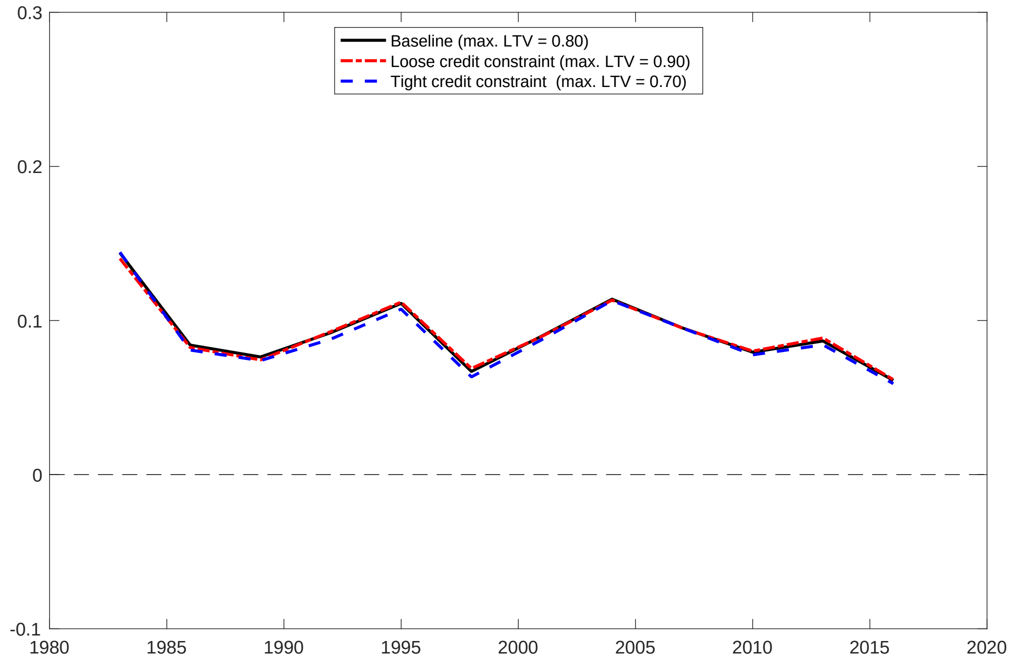

Figure A.24: Impact of Borrowing Constraints on the Housing Wealth Effect

Note: Housing wealth effect for alternative calibrations of the LTV constraint.

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 effect because at higher interest rates, households are more likely to downsize their homes and downsizers have large elasticities. The decline in real rates over time may have, to some degree, countered the upward movements in the housing wealth effect 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 effect rises but its time series remains

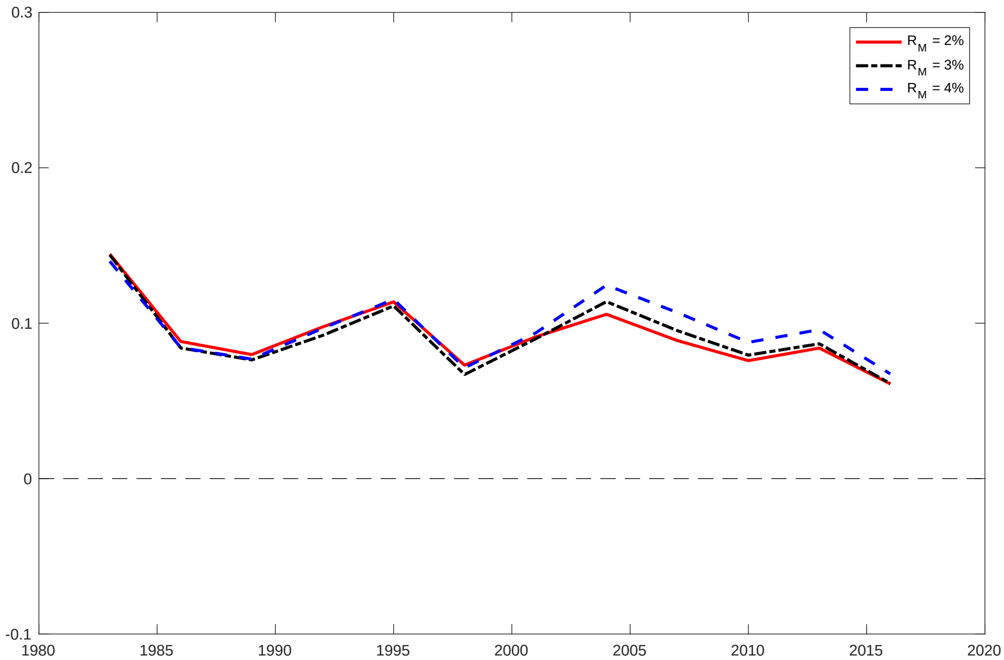

Figure A.25: Sensitivity to Interest Rates

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

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 effect in the aggregate.

E.6 Short-Term Debt

Figure A.27 shows the model implied elasticity of consumption to house prices by LTV as in Figure 10 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 10). 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 effect in the Great Recession as well as a substantial boom-bust asymmetry.

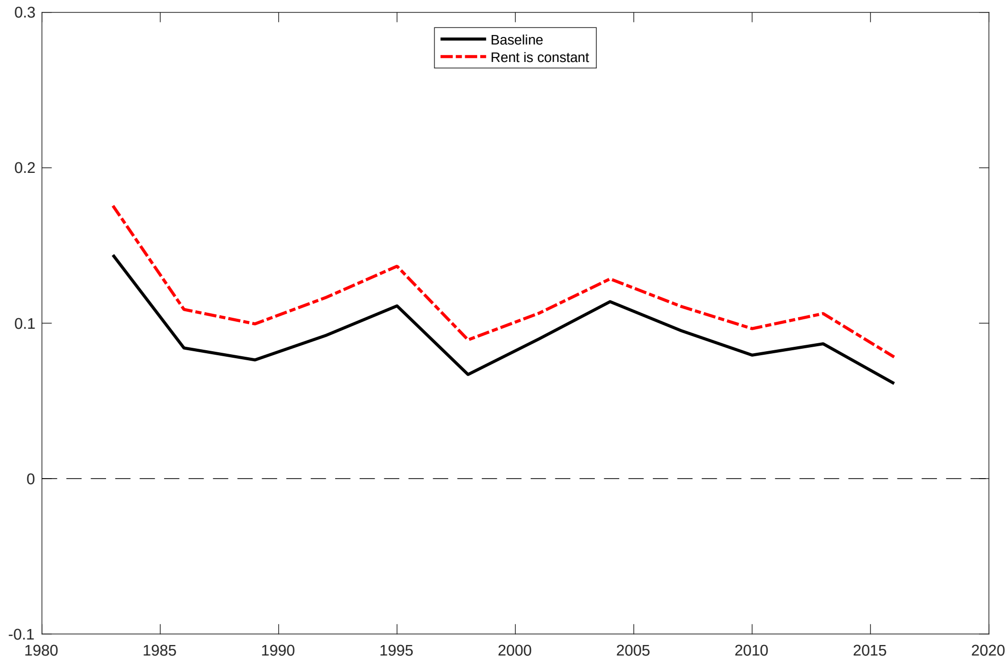

Figure A.26: Sensitivity to Rent-Price Ratio

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

E.7 Housing Transaction Costs

Figure A.28 shows the housing wealth effect for an alternative calibration in which we double the cost of selling a house, ψ^{Sell}. In this alternative calibration, the housing wealth effect 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,

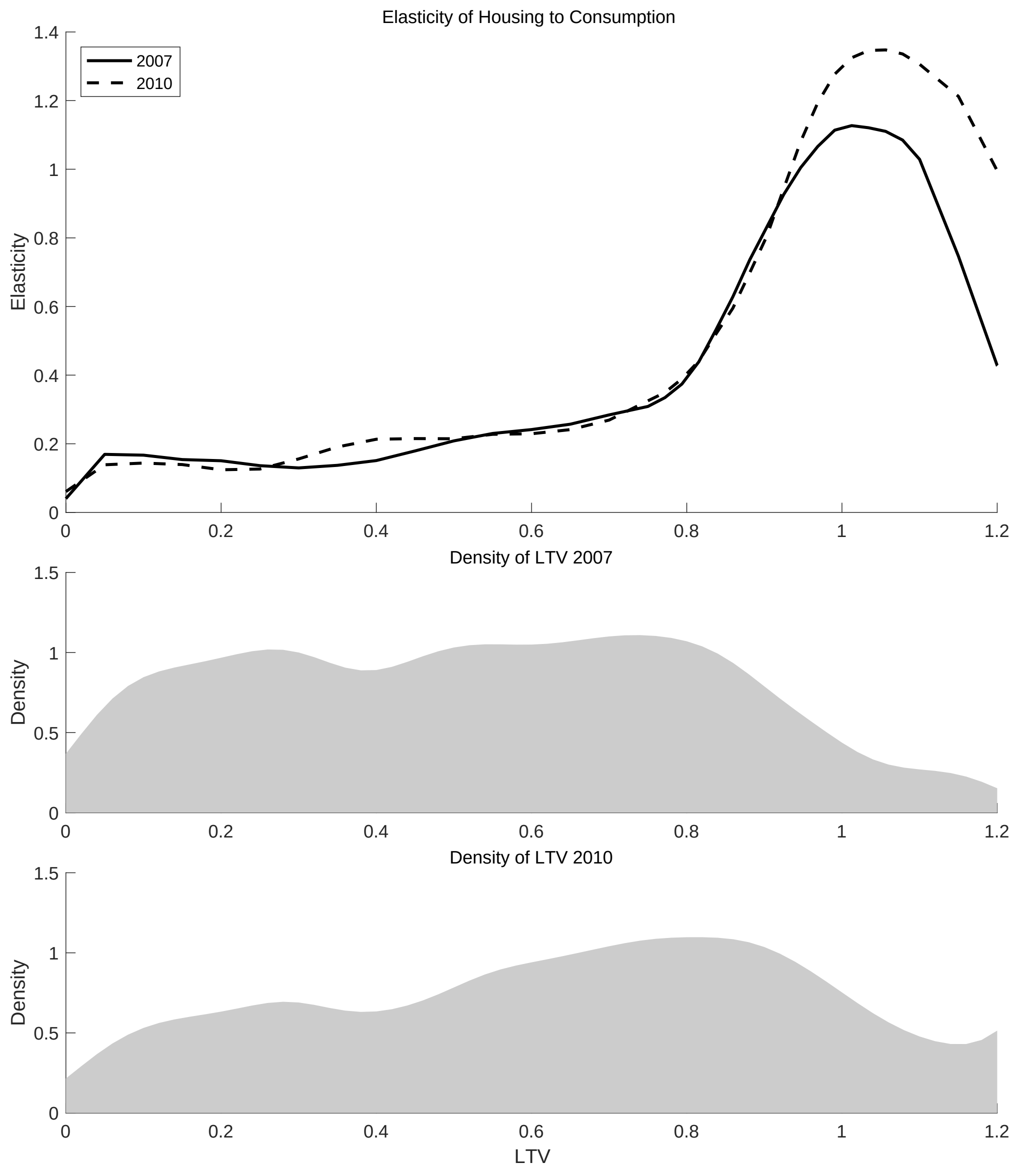

Figure A.27: Elasticity by LTV for Short-Term Debt Model

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

Figure A.28: Sensitivity to Housing Transaction Cost

Note: Housing wealth effect with $\psi^{Sell} = 0.22$, double its value in the baseline.

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:

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

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

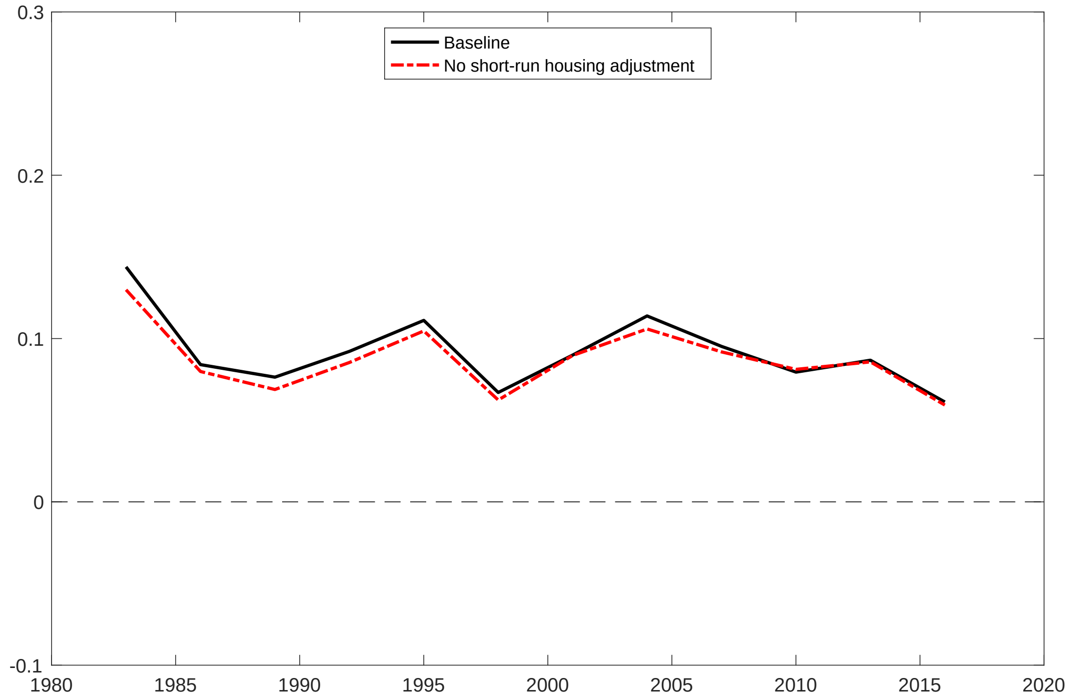

Figure A.29: No Short-Run Housing Adjustment.

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

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 effect 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 effect is very similar to in our 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).

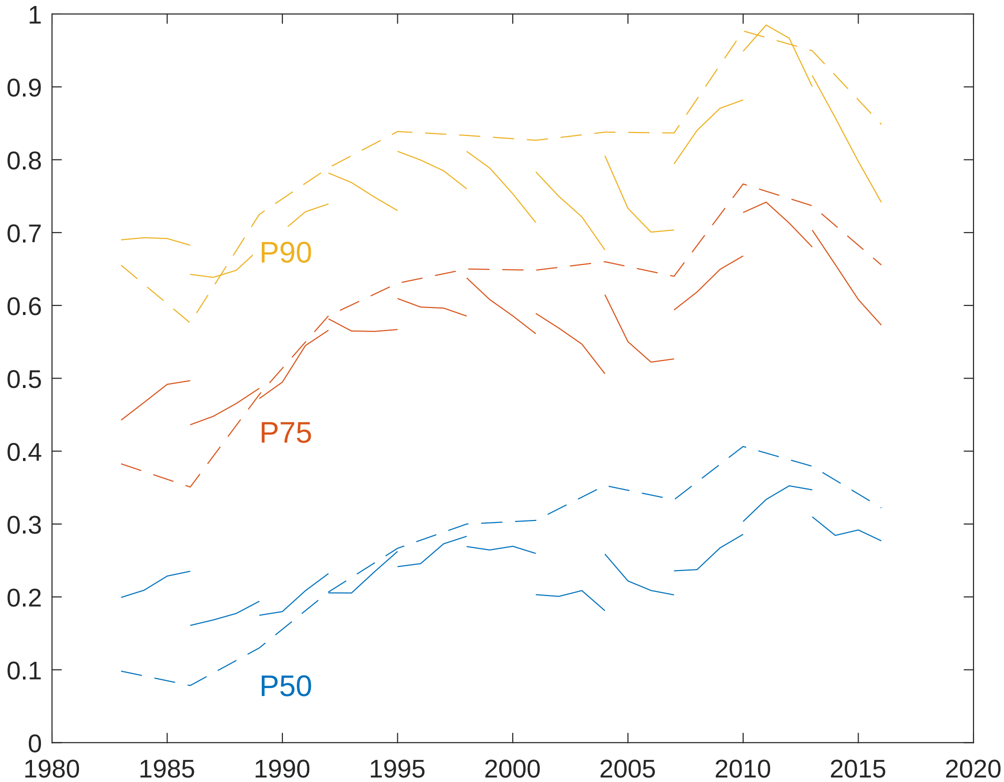

Figure A.30: Simulated LTV Distribution

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.

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 effect.

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 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

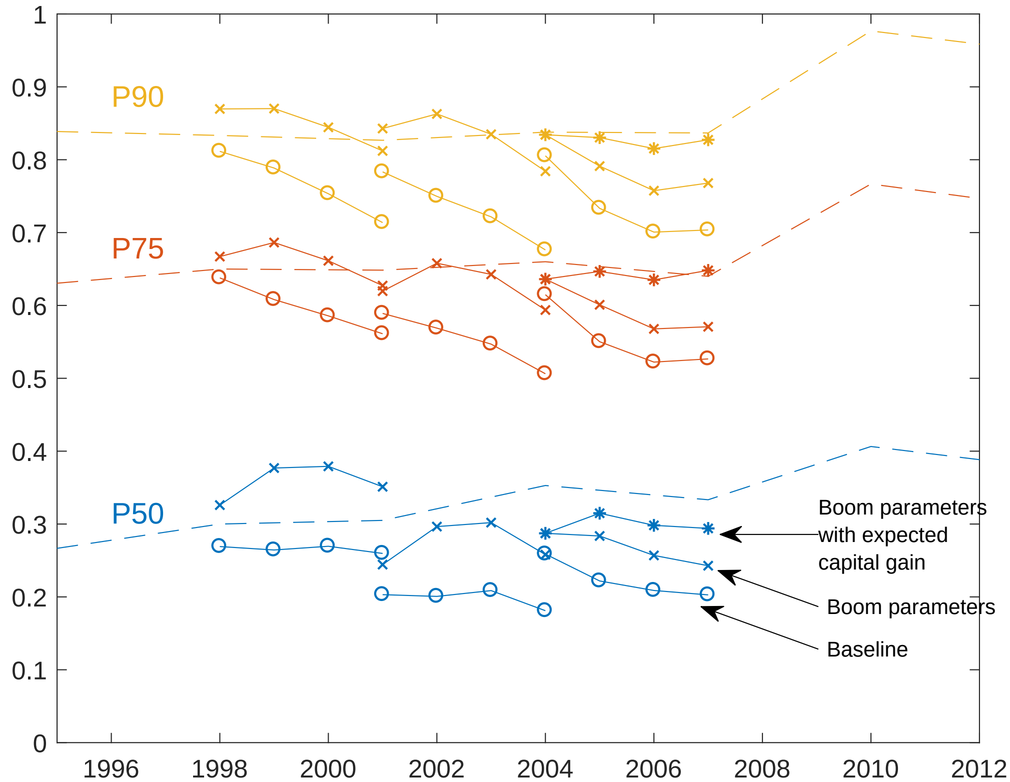

Figure A.31: Simulated LTV Distribution

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.

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 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 expectations of capital gains to be nonzero. Specifically, we assume the one-year expected capital gain rises from 0 to 2 percent in a linear fashion from 2004 to 2007. That is, in 2004 people expect home prices to be constant going forward and in 2007 they expect a 2 percent appreciation in 2008 followed by constant prices thereafter. Expected capital gains increase leverage for two reasons: first, homeowners feel richer and increase consumption due to a wealth effect 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 effect. Indeed, in 2007, the baseline parameters lead to a housing wealth effect of 0.095, the boom parameters lead to 0.098, and the boom parameters with the expected capital gain leads to 0.114.