Do Older Workers Change Their Labor Supply In Response To Housing Wealth Shocks?

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Abstract

Do older workers change their labor supply in response to unexpected housing wealth losses (or gains)? Housing wealth is the largest component in most older Americans' portfolios, and they may seek to recoup losses by working longer to help smooth consumption in retirement. Despite its importance, prior studies have not arrived at a consensus answer. I perform three different analyses to re-approach this question using Health and Retirement Study (HRS) data: a descriptive analysis; two separate differences-in-differences-in-differences analyses exploiting the China Shock and the Great Recession; and an analysis employing autoregressive models for estimating unexpected shocks to housing price appreciation. All three analyses concur that older homeowners do not significantly change their labor supply or Social Security claiming behavior in response to unexpected housing wealth gains or losses. Subgroup analyses suggest that college-educated workers may be the most responsive, even though housing wealth makes up a lower share of their total wealth, probably due to their comparably greater employment resiliency in weak labor markets.

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Introduction

How do older workers respond to negative housing wealth shocks? Do they work longer? If already retired, do they return to work? What about positive housing wealth shocks? And lastly, what about older renters? Answering these questions is important for several reasons. Workforce aging puts pressure on Social Security and other entitlements, prompting policymakers to look for levers that encourage older workers to delay benefit claiming. Housing wealth deserves particular attention because most older workers are homeowners and it represents the largest source of non-Social Security and employer-provided pension wealth for many [Engelhardt and Eriksen, 2022]. Further, as pointed out by Farnham and Sevak [2016], the effects of housing wealth losses are more highly correlated with local labor market conditions than other asset shocks. Changes in older workers' labor supply in response to these losses could have important implications for local labor market hysteresis as well as older workers' job prospects in the shocked labor market. Yet, in spite of its importance, the existing empirical literature on these questions has failed to arrive at a consensus, with some papers finding they increase labor force attachment to recoup losses and others finding no significant effect on older workers' labor supply at all.

One answer comes from the lifecycle model of consumption, which argues that older workers respond to negative wealth shocks by reducing their leisure consumption and working more or even re-entering the workforce if already retired and vice versa for positive shocks. In spite of the life cycle model, it is not clear from first principles that older workers should respond to housing wealth gains or losses the way they do to other asset changes.[2] Older people do not seem to rely on housing wealth to fund their consumption. For example, very few take out a reverse mortgage [Cocco and Lopes, 2020, Davidoff, 2015]. Unlike stocks or bonds, houses sell in illiquid markets, have price movements that are hard to observe,

[2]Many papers have affirmed that older workers tend to respond to asset shocks as predicted under the lifecycle model of consumption, such as stock market shocks [Gustman et al., 2010, Coile and Levine, 2006, 2011, Hurd et al., 2009, McFall, 2011, Goda et al., 2012], inheritances [Brown et al., 2010], and lotteries [Cesarini et al., 2017, Imbens et al., 2001].

and necessitate a move upon sale which older workers do at the lowest rates of all age groups [Molloy et al., 2017]. Some older workers may also have a bequest motive for their housing wealth, making them less sensitive to short-term fluctuations in its value [Begley, 2017, Engelhardt and Eriksen, 2022]. Further, housing bundles together an investment asset and a consumption good, and older workers may chiefly value homeownership for its ability to stably consume housing and local amenities while living on a fixed income [Groot and Verberne, 1997].[3] If older workers' chiefly value homeownership for its hedge against rent risk, they could even theoretically be more likely than renters to reduce their labor supply if the housing wealth shock is caused by a local labor market shock which simultaneously reduces older workers' job prospects and lowers their wages. These cross-pressures may explain why there is conflicting evidence for whether homeowners respond to housing wealth shocks by changing their labor supply.

In this paper, I re-examine this question with several new approaches and data to attempt to arrive a more definitive answer to the questions posed above. I start by looking at the raw relationship in the data between changes in housing prices and changes in hours worked. Both scatter plots and simple first difference regressions show close to no relationship between either changes in housing prices or changes in net housing wealth on hours worked. Renters, on the other hand, appear to meaningfully shift their hours in response to changes in real rents, probably reflecting that they face rent risk while currently or prospectively living on a fixed income.

However, these relationships cannot be causally interpreted because many of the shifts in housing prices and hours might be jointly determined by local labor market changes, not "pure" responses to the shift in prices and housing wealth. One challenge all papers on this topic must overcome is that quasi-natural experiments of sufficient scale are few and far between. Exogenous idiosyncratic housing wealth shocks are rare, and the studies that

[3]See Sinai and Souleles [2005] for a good theoretical treatment on how homeownership's value as a hedge against rent risk.

do use them,[4] generally have sample sizes too small to focus just on the effects on older workers. Complicating matters further, shocks to housing markets often spillover into local labor markets and vice versa (E.g., Charles et al. [2018]; Mian and Sufi [2014]; Notowidigdo [2020]; Xu et al. [2023], among many others). Researchers must proceed carefully to make sure that they are isolating just the effect of the housing wealth shock while fully controlling for any corresponding labor market effect, a tall order.

The prior literature has typically surmounted this problem in two ways. One approach is to take a well-known housing wealth shock such as the Great Recession, exploit regional variation in the severity of the bust for treatment, and use the impact on same-location renters to control for the impact on the local labor market (e.g. Coile and Levine [2011], Goda et al. [2012], Farnham and Sevak [2016], Zhao [2018], Zhao and Burge [2017]). The identification assumption here is that the housing market shock has no wealth effect on renters, so they can be used to capture the "pure" effect of the local labor market shock under the assumption that the effect of the shock would be identical between renters and homeowners except through the housing wealth channel. The effect on homeowners is thus measured in relation to the effect on renters.

For my causal analysis, I start by exploiting the China Shock as my treatment of interest [Autor et al., 2013], using renters to control for its labor market impacts and an interaction term between homeownership and the China Shock to capture any differences between renters and homeowners induced by the Shock's effect on housing wealth. While Zhao and Burge [2017] and others use the Great Recession, the Great Recession was a severe national shock that impacted most local labor markets directly, making it difficult to establish any set of local labor markets as a proper control group.[5] The negative effects of the China Shock, by contrast, were mostly concentrated in a subset of local labor markets Autor et al. [2013],

[4]E.g., Rossi-Hansberg et al. [2010], Jud and Winkler [2006], and McMillen and McDonald [2004]

[5]Zhao [2018] also uses the Great Recession with renters to control for its direct, labor market effects, but uses coarse, Census Division-level variation in housing prices to generate a treatment effect. Empirically, Zhao and Burge [2017]'s approach of using more granular Metropolitan Statistical Area-level housing price fluctuation offers stronger identification, so I focus more on that article when comparing my results with the prior literature.

Acemoglu et al. [2016], Asquith et al. [2019], making it more credible to identify a group of unaffected local labor markets that can act as a control group for the treated renters.

One potential issue with the China Shock may be that affected homeowners underestimated their housing wealth losses because of the contemporaneous mid-Aughts housing boom. It is also true that local labor market shocks can be highly idiosyncratic, as even nationwide recessions have different impacts on different local labor markets or even the same local labor market may be more robust to some recessions than others [Hershbein and Stuart, 2024]. I thus also perform the analysis using the Great Recession to confirm that the pattern of results remains largely the same. Both analyses return largely null or small, statistically insignificant results. Robustness checks such as looking at just respondents with no other forms of wealth besides Social Security (to isolate the sample to people whom may be especially reliant on housing wealth), or removing mortgagees (because they might act more like renters and bias the estimate on the housing wealth effect toward zero) also yield no statistically significant differences between renters and homeowners.

I next use an approach pioneered by Begley and Chan [2018] and Farnham and Sevak [2016] of capturing homeowners' responses to unexpected housing price movements, which has the advantage of not relying on the "no housing wealth effect for renters" assumption. I focus on the approach of Begley and Chan [2018] in particular, who use ZIP code level unexpected housing price movements that allow them to control for local labor market conditions nonparametrically via metropolitan statistical area (MSA)-by-year fixed effects. However, Begley and Chan [2018] also use MSA-level housing price appreciation data to estimate the coefficients for their model and then fit them to their ZIP code-level data, which may inadvertently reintroduce local labor market variation. I innovate on their approach first by estimating ARMA models just at the ZIP code level using the Federal Housing Finance Agency's (FHFA) ZIP level housing price indices (HPIs). Begley and Chan [2018] use 20 years of data for each MSA when estimating their model, but Farnham and Sevak [2016] argued that homeowners really only form expectations on future price growth based on the

past couple of years. In that spirit, I also then generate a second set of ARMA models at the ZIP code level on 12 years of data using three lags and a time trend as a second-order polynomial to ensure my findings are robust to how these shocks are calculated.

I find that short-term unexpected housing price gains (or losses) largely do not change older people's propensity to change their retirement or Social Security claiming behavior. Effects tend to be null or not robust to changes in how these unexpected shocks are estimated or which controls are included, nor does using instead total wealth changes based on the unexpected housing prices movements yield statistically significant effects.

Analyses of subgroups (men versus women, college-educated versus non) reveals that the overall null effects in the main sample may be due to differences in responses by these subgroups. Men and women tend to have opposite signed effects and thus may offset each other in a pooled sample. College-educated homeowners do seem to respond to housing wealth losses by increasing their labor supply, but non-college workers do not. Since the latter are much more numerous than the former, the latter effect predominates. It is worth noting, however, that housing wealth tends to be a smaller part of college-educated workers' total portfolio, so this difference may reflect that college-educated workers have greater labor market resilience instead of a "pure" wealth effect.

Throughout, I also include controls for job displacements, which proxies for income shocks, and show consistently that homeowners reduce their labor supply more than renters in response. This further indicates that older renters may have greater labor supply elasticities because their access to stable housing consumption is more tenuous, and reinforces again that older homeowners are cross-pressured by more than just the desire to recoup housing losses when considering whether or not to increase their labor supply in response.

This paper joins a long literature on these questions. Several papers have investigated housing wealth shocks as one of several asset shocks under study. Perhaps the first was Coile and Levine [2011], who found no evidence in Current Population Survey (CPS) data that homeowners change their transitions to retirement in response to housing wealth shocks.

Goda et al. [2012] similarly found using Health and Retirement Study (HRS) data little relationship between housing wealth fluctuations and retirement intentions, but they did find that renters in rising housing markets decreased their expected retirement ages. Disney et al. [2015] found using UK data that local labor market conditions, particularly unemployment rates and earnings, matter far more than changes in asset prices including housing.

Among papers cited above that have focused specifically on housing wealth fluctuations, Farnham and Sevak [2016] use HRS data to find significant effects of housing wealth fluctuations on retirement expectations while finding no effect on observed retirement timing. Zhao and Burge [2017] find that labor supply decreased (relative to renters) in response to the housing boom of the Aughts, and increased (relative to renters) in response to the housing bust in 2008. Begley and Chan [2018] exploit changes in expected housing wealth to compare homeowners in ZIP codes who underperformed or overperformed expectations to homeowners in ZIP codes whose wealth appreciated as expected. They find that men experiencing negative shocks delay retirement, with stronger effects for mortgage holders. Other papers that have also studied this question directly have generally also found that older workers reduce or increase their labor supply in response to housing wealth changes in line with the life cycle model's predictions [Ondrich and Falevich, 2016, Zhao, 2018, Disney and Gathergood, 2018, Favilukis and Li, 2023]. Generating a more definitive answer that can reconcile these findings is thus a substantive contribution.

The rest of the paper proceeds as follows: Section 1 examines the raw data to establish at baseline what the relationship is between changes in housing prices or housing wealth and older workers' labor supply. Section 2 undertakes the China Shock and Great Recession differences-in-differences-in-differences analyses. Section 3 then reestimates older homeowners' and renters' labor supply responses to housing wealth shocks using the unexpected housing price movement approach. Section 4 concludes by recapitulating the findings.

1 Analysis I: Evidence from OLS

As a starting point, I examine the basic relationship between changes in housing prices, housing wealth, and rents and changes in older workers' labor supply using ordinary least squares (OLS). Here I simply regress the change in the log number of annual hours worked on either the change in log real housing price for homeowners; the change in log real net housing wealth; and the change in log real rents (for renters) subject to some controls. This is not a causal analysis, because it does not attempt to ensure that the changes in housing prices or rents are unconfounded by factors that might also be changing labor supply, like a negative local economic shock. However, it is still useful to see whether the expected relationship between housing prices and labor supply shows up by a casual examination of the data to give a sense of how detectable wealth effects are at baseline.

1.1 Empirical Strategy for Analysis I

All empirical analyses in this paper use HRS survey data to form the core sample. The HRS is a nationally-representative longitudinal survey of people aged 50 and over conducted since 1992, with extensive biannual interviews of respondents and their spouses. The HRS is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan. The HRS contains six cohorts: the original HRS cohort (born 1931-1941), the AHEAD cohort (born before 1924),[6] the Children of the Depression (added in 1998; born 1924-1930), War Babies (added 1998; born 1942-1947), Early Baby Boomers (added 2004; born 1948-1953), Mid Baby Boomers (added 2010; born 1954-1959), and Late Baby Boomers (added 2016; born 1960-1965). The survey asks respondents and spouses for information on their demographics, health status, employment history,

[6] AHEAD stands for Asset and Health Dynamics Among the Oldest Old, and was originally a companion study of the HRS that looked at people age 70 and over in 1993. AHEAD respondents were surveyed separately from the original HRS cohort in 1993 and 1995, and the survivors were merged on to the rest of the HRS in 1998.

income, Social Security and pensions, and retirement, financial, and housing wealth.[7]

In this analysis, I use respondents aged 50 and over to reflect the population of people of greatest interest.[8] I use 1990 commuting zones (CZs) as my definition of local labor market [Tolbert and Sizer, 1996] instead of Metropolitan Statistical Areas (MSAs) used by other authors. Since every county is assigned to a CZ, but some counties are too thinly populated or too remote to be included in an MSA, this allows me to include more rural respondents that would otherwise be mechanically excluded if I used MSAs.

I take the HRS sample from 1992-2018 and compute the change in log annual hours worked and changes in housing prices, housing wealth, and rents between survey waves. I code retirees as having log annual hours worked of zero if they do not report otherwise. Like Begley and Chan [2018], I drop people whose marital status changed between survey waves because these changes in marital status often involve large changes in respondents' wealth.

First, Figure 1 shows basic scatter plots between the change in log annual hours worked between survey waves and various measures of changes in housing prices: change in real housing prices, change in real net housing wealth, and real rents (for renters). I also include the line of best fit for each and scale each graph equally so that the slopes are easily comparable. Figure 1a shows little relationship between the change in housing prices and changes in hours worked and Figure 1b also similarly shows little relationship. In both cases, the line of best fit very modestly increases with changes in housing prices, which is the opposite of the lifecycle model's predictions. Figure 1c shows not an especially strong relationship between changes in rents and changes in hours worked, but the line of best fit is more obviously upward sloping and is in line with the lifecycle model's predictions for renters.

To see how robust the association is once other factors are controlled for, I regress the

[7]In particular, I used RAND's longitudinal datafile that reworks the individual HRS survey responses into a panel dataset, Health and Study [2024]. For needed variables that are not included in RAND's version, I retrieved them directly from the individual HRS public-use surveys.

[8]Respondents younger than 50 tend to be the spouses/partners of survey participants. I also intentionally exclude 2020, the focus of Favilukis and Li [2023], because of the unique circumstances of that recession. Zhao and Burge [2017] use people aged 44 and over, but that means the sample will include people who will be a bit too young (and less informative about older workers in general) and a bit on the older side (and have very, very low labor supply elasticities).

change in log annual hours worked on the three housing price change measures:

$$\Delta ln(Ann\ Hours\ Worked_{iw}) =$$
$$\alpha_0 + \alpha_1 \Delta H_{iw} + \alpha_2 Displaced_{iw} + X'_{iw}\Gamma + \delta_{c(i)} + \delta_{s(i)} \times \delta_w + \epsilon_{iw} \quad (1)$$

where ΔH_{iw} represents one of the three housing price change measures for respondent i between survey wave w and $w-1$. I take the additional step of also controlling for job displacements ($Displaced_{iw}$). These displacements are good proxies for negative income shocks and are an important benchmark for the relative importance of housing price changes. I identify respondents' income shocks from self-reported incidents of job displacement in the same spirit as Chan and Stevens [1999, 2001]. The literature on how older workers respond to job displacements has found that unanticipated job losses for men in their pre-retirement years (< 62) cause them to extend their working years Chan and Stevens [2001, 2004], but also cause them to increase their retirement propensity if it occurs in their retirement-eligible years [Chan and Stevens, 2004, Coile and Levine, 2007, Disney et al., 2015].

X_{iw} represents controls that might also influence the change in hours worked, including age as a 3rd order polynomial; dummies for being married; for being widowed; for being Medicare-eligible; for whether the respondent's husband or wife are Medicare age-eligible (if applicable); being age-eligible for claiming Social Security early; and for being age-eligible for claiming "full" Social Security; as well as dummies for whether a person's husband or wife are Social Security age-eligible. "Age-eligible" is defined here as being eligible for claiming Social Security based on one's age, either early at age 62 with payments actuarially discounted relative to the Primary Insurance Amount (PIA) or a monthly amount equal to the PIA at one's Normal Retirement Age (NRA). Actual eligibility is determined both by age, which varies by year of birth,[9] and earnings history. To avoid endogeneity bias, I measure eligibility based only on a respondent's (or their spouse's) birth year and age. I also include

[9]For those born before 1943, the PIA could be claimed at age 65. For those born between 1943-1954, the PIA could be claimed starting at age 66.

a dummy for whether the respondent self-reports being in "much better" or "much worse" health since the previous survey wave, which captures whether the respondent experienced a positive or negative health shock (respectively). Lastly, for the housing price and net housing wealth regressions, I include a dummy for whether the person had a mortgage and their implied property tax rate in survey wave $w-1$. The latter is calculated as the reported annual property tax paid over the self-assessed value of the house.

The first two columns of Table 1 show the means and standard deviations for the core sample of homeowners used in this analysis and the next two columns have means and standard deviations for renters.[10] The two samples are quite different on observables. Converting the log points back to hours, the average homeowner worked about 28 hours a week and the average renter worked just 14. About 70 percent of homeowners are married, as compared to just 25 percent of renters. Only 8 percent of homeowners are Black as compared to 23 percent of renters, and 31 percent of homeowners are college educated as opposed to 15 percent of renters. I note that net housing wealth is about $80,000 less than the self-reported value of the primary residence, due largely to outstanding mortgages and home equity loans.

1.2 Results for Analysis I

Table 2 confirms an overall weak relationship between changes in housing prices or net housing wealth for homeowners but a much clearer relationship for renters. Column 1 reports the results for how annual hours worked changed in response to changes in self-reported house values. Increasing the real house price by 1 log point would lead to an increase in hours worked of 0.017 log points, or by less than 1 percent over the mean of 3.36 log points. There may be somewhat asymmetric responses to housing wealth gains versus losses, so in Column 2, I look at how log hours changed in response to just negative housing price changes. A one log point decrease in housing prices causes hours to rise by 0.026 log points,[11] or less than a 1 percent increase over the mean. Notably, restricting the sample to just positive

[10] HRS disclosure rules forbid the release of medians, mins, and maxes.

[11] Since the log point change is negative here, the negative coefficient on ΔH yields an increase in hours.

price increases (Column 3) shows that hours worked decrease in response by a quite small 0.013 log points. Columns 4-6 look at the same three specifications but for changes in net housing wealth instead. I similarly find very small, non-statistically significant changes in log hours worked, but the sign on negative and positive housing wealth changes are both positive instead of negative. Thus, one would find that older homeowners behave according to the lifecycle model when looking at just changes in housing prices (increasing hours when values decline and vice versa), while finding that they do not if looking at changes in net housing wealth (decreasing hours when values decline and vice versa). The fact that the effects are very small and indistinguishable from zero would lead to no solid conclusions either way. Across all specifications, the effect on hours of being displaced is large, negative, statistically significant, and (unsurprisingly) equivalent to going from working to not working.

In contrast, Columns 7-9 show that renters react significantly to changes in rents. Column 7 reports that a 1 log point change in rents causes renters to increase their hours worked by 0.16 log points (significant at the 5 percent level), or a 6 percent increase above the mean log hours worked. The effect is, if anything, stronger when rents decline, as Column 8 finds that renters reduce their hours by .21 log points (also significant at the 5 percent level), or an 8 percent decline, while Column 9 reports that rent increase are associated with renters increasing hours worked by just 0.12 log points (not statistically significant). It is notable that renters' labor supply elasticity in housing prices appears higher than homeowners', which intuitively would occur because older renters need to worry about paying for their housing while currently or prospectively living on a fixed income.

2 Analysis II: Exploiting Regional Economic Shocks

The analysis above suggests that there is not a strong association between changes in housing prices or net housing wealth and changes in hours worked. As noted, this evidence may just reflect that changes in both prices and hours often reflect changes in job prospects

in the local labor market. The challenge is thus to cleanly identify the effect of the housing wealth change separate from the impact of the local labor market shock.[12] My first solution is to identify any effects of the shock on the local labor market separately from the shock on an individual's housing wealth by including both a dummy for the shock itself and then an interaction term between the shock and a dummy for being a homeowner. The identifying assumption here is that the shock's effect on local labor market prospects is conditionally the same for both renters and homeowners except through the housing wealth channel. Renters are used to capture the net impact of the local labor market shock, and the interaction term between the shock and homeownership reflects the wealth effect (if any) on homeowners net of the local labor market effect. Since the impact of a negative labor demand shock on renters is not completely wealth-neutral, this effect will be somewhat biased, although it should be not unduly so.[13] Further, because the results in Table 2 give us some intuition for the direction of the bias, it is possible to account for it in interpreting the results.

2.1 Analysis IIA: The China Shock

The China Shock began after Congress granted China Permanent Normal Trading Relations (PNTR) status in October 2000, with most research finding that its strongest negative effects on employment were felt during the 2001-2007 period [Autor et al., 2013, Pierce and Schott, 2016]. Thus, I include all HRS respondents whose first survey was conducted no later than 1998 and end the study period of this analysis with the 2006 survey to avoid contamination with the Great Recession. Because the HRS refreshed their sample with the Children of the Depression cohort in 1998, I also drop all observations prior to 1998 to avoid shifts in pretrends that are unrelated to changes in the underlying conditions in either the

[12] Charles et al. [2018] provide a useful model for modeling the general equilibrium results of shocks to one market or the other. Xu et al. [2023] discuss this dynamic in the specific context of the China Shock, finding that spillover effects into housing markets exacerbated the labor market shock by 20-30%.

[13] Any positive wealth effect for renters is likely to be small. For example, Feler and Senses [2017] report that for each $1,000 increase in Chinese import penetration per worker decreased the median annualized rent by only $187 (p. 117, Table 5, Column 7), or 2.5%. By contrast, the median owner-occupied house declined by 5.4%.

treatment or control regions.

I measure the China Shock at the local level by using Feler and Senses [2017]'s replication package to estimate the Shock's effect on local housing price appreciation over the 2000-2007 period at the commuting zone (CZ) level.[14] I create CZ-specific housing price indices (HPIs) from the Federal Housing Finance Agency's (FHFA) county-level HPIs and creating 1990 population-weighted averages across the counties in the CZ using the crosswalk provided by David Dorn to map counties to 1990 CZs.[15] Feler and Senses [2017] use Census data to find that for every $1,000 increase in Chinese imports per worker, median self-reported house prices declined by $7,660 (nominally) or 5.4 percent. I find that every $1,000 increase in Chinese imports per worker lowered housing price appreciation relative to baseline by 9.7 percentage points over a 10 year period (Appendix Table B1), or a reduction in $24,381 in price appreciation for the median house in 2007 dollars. Appendix Figure B1 shows the treated CZs in darkest red. Appendix B gives additional information on how this is calculated.

Thus, while smaller in scope and scale than the Great Recession, the China Shock nonetheless created a meaningful negative wealth shock in the most exposed regions while having a much smaller impact in many areas. Appendix Table B2 shows the range of estimated housing price appreciation losses sorted by quartile. CZs in Quartile 1 had estimated 7-year cumulative losses ranging from -523 to -42 percent, with a median of -61 percent and a mean of -75 percent. These values are almost surely on the high side for at least those CZs in the farthest left tail, but nonetheless demonstrate that China Shock cost homeowners in heavily exposed regions a substantial amount of housing price appreciation that they might otherwise have enjoyed, particularly against the backdrop of the nationwide housing boom. Losses in CZs in the second quartile are smaller, ranging from -42 to -24 percent, but still substantial. Losses in the third quartile are much more modest, ranging from -23 and -11

[14] I thank the authors for making their replication package publicly available at `http://doi.org/10.3886/E114677V1`

[15] Available here: `https://ddorn.net/data/cw_cty_czone.zip`

percent, with a median of -17, and losses among CZs in the fourth quartile are negligible, ranging from -11 to 8 percent and a median of just -3.5 percent.

I measure treatment status by assigning a dummy to respondents whose 1998 CZ was later in the top quartile for most foregone housing wealth due to the China Shock. Assigning treatment by quartile captures that a group of CZs that all experienced relatively large losses, even if the exact rank ordering and size of the loss may not be precisely known. It also makes it easier identify which set of CZs can serve as the best control group. I thus drop the CZs in the second quartile, because while they were generally less severely impacted than the first quartile CZs, the estimated housing price appreciation foregone is large enough that they could possibly contaminate the control group.

This approach lends itself neatly to a differences-in-differences-in-differences estimation, similar to Zhao and Burge [2017], where people living in the most exposed CZs will serve as the treatment group. I use renters to capture the China Shock's effect on the local job market and an interaction term between the China Shock dummy and homeownership will then isolate the housing wealth effect. The identifying assumption here is that renters and homeowners, net of controls, reacted differently to the China Shock only through the housing wealth channel. This empirical method will only be feasible (1) it is clear in the data that the China Shock lowered home prices and (2) outcomes between the treatment and control respondents were evolving in parallel *ex ante*. The latter is widely used in the differences-in-differences literature to justify the assumption that the control group represents an adequate proxy for how the treatment group would have behaved but-for the treatment. Figure 2 shows that logged housing values for respondents living in CZs most exposed to the China Shock (dotted red line) had home prices that seemed to be evolving in parallel with those in the other CZs (solid red line) between 1998 and 2000. After 2000, however, logged housing price appreciation visibly lags in the China Shock CZs. Rents were also evolving roughly in parallel between the two groups of CZs, although after 2000, rents decline a bit more in treated CZs initially before returning to evolving in parallel with the control group. This is

encouraging because using renters to capture the labor market effect of the Shock is unlikely to be meaningfully biased by any wealth effect given that the Shock seemed to have such a negligible effect on rents.

Ideally, for a valid differences-in-differences-in-differences regression, there would be evidence for parallel pre-trends for the difference in outcome means between renters and homeowners in the treated CZs versus the control CZs [Olden and Møen, 2022].[16] Appendix Table A1 shows that the treatment and control groups are broadly similar at baseline in terms of both outcomes, controls, and demographics. No differences in means are significant at the 5% level, although the control group's labor force participation rate was about 4.3 percentage points higher (p-value=0.079) and their log annual hours worked is about 0.34 log points higher (p-value=0.055). However, Figure 3 shows the average owner-renter difference by China Shock region for log annual hours worked, being in the labor force, and claiming Social Security. All display reasonable evidence for parallel pre-trends over 1998 and 2000, and the treatment group (dotted red line) exhibits a sharper break in trend *ex post* than the control group (solid blue line).

Table 1's Columns 5 and 6 report the means and standard deviation the China Shock sample at the start of the study period (1998). The China Shock sample is restricted to respondents who were aged 50 to 78 at the start of the sample period.[17] The average respondent worked about 34 hours per year, reflecting that only about half of the sample is in the labor force at study start, with a bit over a third report being retired. 43 percent are already claiming Social Security (not surprising as the average age is 62.3). This highlights that one reason for why housing wealth changes may not yield large changes in labor supply

[16]In a differences-in-differences regression, you need parallel pre-trends in the simple outcome mean between the treatment versus the control group. While parallel pre-trends are neither necessary nor sufficient for the the experiment to generate the needed counterfactual parallel trends, they are strong evidence in favor of its validity. Right now, there is not a definitive pre-test one could perform to evaluate *ex ante* whether the differences-in-differences design is valid and plausible [Kahn-Lang and Lang, 2020].

[17]Zhao and Burge [2017] use all respondents 44 and over, while Begley and Chan [2018] use respondents aged 50 to 75. Using respondents aged 50 to 78 at sample baseline strikes a balance between not including too many people who are too old to work at all while making sure to capture the full spectrum of people who could be possibly transitioning to retirement.

is low labor force attachment at baseline. Most of the sample is either married (68 percent) or widowed (14 percent), 40 percent are age-eligible for Medicare (aged 65+), and 3 percent had been displaced since the last survey wave. The sample is 46 percent male at baseline, 86 percent white, 10 percent black, 20 percent of respondents had a college degree or more, and 73 percent were homeowners, with 35 percent of those having a mortgage. The average self-assessed house value was $249,917, the average net housing wealth was $190,849, and the average monthly rent was $876 (all in 2024 dollars).

I calculate a differences-in-differences-in-differences estimate of the effect of shocked respondents on their labor outcomes as:

$$\begin{aligned} Outcome_{it} = \beta_0 + \beta_1 Shock_{it} + \beta_2 Shock_{it} \times Homeowner_i + \beta_3 Displaced_{it} \\ + \beta_4 Displaced_{it} \times Homeowner_i + X_{it}\zeta + \iota_i + \gamma_t + \gamma_t \times Homeowner_i + \epsilon_{it} \end{aligned} \quad (2)$$

where $Outcome_{it}$ is either logged annual hours worked; whether the person is in the labor force; or is claiming Social Security. $Shock_{it}$ is the dummy for living in a top quartile CZ at the start of the sample period (1998, to avoid endogeneity bias from moves), set to one in the affected CZs from 2001 onward.[18] As above, I also control for job displacements with *Displaced*. Here, this variable helps strengthen my identification assumption by removing a potential source of confounding bias because strong, local negative shocks are also likely to cause spikes in job displacements.

The estimates of interest are β_1 as the effect of the trade shock on renters, which can be interpreted as the net effect of a minor positive wealth shock (lower rents) coupled with a weakening of outside employment prospects; β_2 is the effect of a housing wealth losses caused by the trade shock *relative* to the weakening of outside labor market prospects caused by the shock; β_3, which is the effect of an income shock caused by job displacement on a renter; β_4 is the net difference of *Displacement* on homeowners *relative* to renters. As explained

[18] In the 2000 survey wave, 96.5% of their interviews were conducted prior to the October 2000 start date for PNTR, so for all intents and purposes, the first treated year is the 2002 survey wave.

above, I start by assuming that β_1 is not biased by any positive wealth effect for renters, although I will discuss how violations of that assumption might influence my results.

There is no $\beta_5 Homeowner_i$ term because I fix homeownership status as the whether the person was a homeowner in their initial survey wave and is thus subsumed by the individual fixed effects which enter the equation as ι_i. This prevents endogeneity bias from entering the estimates, as whether a respondent owns a home is itself an outcome. I also include year fixed effects (γ_t) and year fixed effects interacted with $Homeowner_i$, which controls for shocks common to all homeowners. Concretely, $\gamma_t \times Homeowner$ controls for the effects of the contemporaneous nationwide housing boom on homeowners.

X_{it} represent other controls, largely similar to those used in Equation (1) with some minor exceptions that allow me to more fully control for how housing costs —as opposed to housing wealth fluctuations —influence older homeowners' labor supply. The first is that I redefine the dummy for having a mortgage as whether or not the respondent had a mortgage in the initial year (1998) to avoid endogeneity bias. This makes my measure for having a mortgage time-invariant (and thus collinear with the individual fixed effects), so I also include interactions between this mortgage measure and being age-eligible for early Social Security and for being age-eligible for full Social Security. These interactions help control for how respondents may need to pay for a mortgage on a fixed income could have differentially higher labor supply to cover these higher housing costs. I similarly include interactions with the initial homeownership dummy and the early Social Security and full Social Security dummies to help control for any age-specific differences in transitions to retirement by homeownership status. Lastly, I also interact the initial homeownership dummy with the health shock dummies, the married dummy, and the widowed dummy, as homeowners in all of these categories may have somewhat different labor supply elasticities than renters.[19]

[19]I do not, however, include controls for property taxes in these regressions. Self-reported property tax information in the HRS could change dramatically whenever the respondent moves, perhaps (endogenously) choosing a lower property tax rate *ex post*. Thus, it would be ideal to use the property tax rate of their initial jurisdiction, but this is also difficult to do in practice. The Census reports median property tax paid by county for 1990 and 2000, and then the American Community Survey does so for selected counties from 2005-onwards, leaving predominantly rural counties uncovered. It's also the case that homeowners' effective

2.1.1 China Shock Results

Results are shown in Table 3. I start by reporting results on the log annual hours worked. First, I simplify Equation (2) by swapping individual fixed effects for just CZ fixed effects and adding some controls that the individual fixed effects would have subsumed.[20] Panel A shows the estimated coefficients while Panel B reports the net effects of the housing wealth and income shocks on homeowners, which are equal to the sum of the coefficients *Shock* and *Shock*×*Homeowner* and *Displaced* and *Displaced*×*Homeowner*, respectively. Column 1 shows that renters decrease their annual hours worked by 0.06 log points and homeowners increase theirs by 0.12 log points relative to renters, but neither response is statistically significant. Panel B shows that consequently the net effect on homeowners is even smaller than the relative effect, or just 0.06 log points or less than a 2 percent increase over the mean of 3.53 log points. Displaced renters' hours worked fall by 0.10 log points while homeowners' fall an additional 0.51 log points, for a net effect of -0.61 log points (statistically significant at the 1 percent level) or about 20 percent from the mean. In Column 2, I add state-year fixed effects ($\gamma_t \times \delta_s$) to control for state-by-year economic shocks and policies, such as the property tax rebates targeted at seniors discussed in Zhao and Burge [2017] and Shan [2010] or minimum wage changes. This causes both Panel A coefficients to change slightly, the effect on renters to fall to 0.08 log points and the effect on homeowners to increase to 0.14, but Panel B shows that the net effect on homeowners remains unchanged at 0.06 log points. Effects on displacement remain roughly the same although now the difference between renters' and homeowners' responses is significant at the 10 percent level, but the

property tax rate can be significantly lower than the median based on tenure of residence due to homestead exemptions. Complicating matters further, many states have property tax relief programs for senior citizens [Shan, 2010]. I generated a best prediction of the median property tax rate by county from 1990-2018 by using observed rates where available, and then interpolating rates for missing years or using rates from other counties in the same CZ when interpolation was not possible. I include this best guess as a control in the regressions and this does not meaningfully change the results, but I will make these results available upon request (subject to the HRS' disclosure).

[20] These are a dummy for being initially a homeowner; a dummy for initially having a mortgage; race dummies (white, black); a dummy for being male; and then dummies for having a high school diploma, having some college, and having a four year college degree.

net effect is unchanged at -0.61 log points.

Lastly, I add back in the individual fixed effects and remove the time-invariant controls that they would otherwise subsume.[21] Column 3 shows that this causes the effects from *Shock* to attenuate to closer to zero while the effects on *Displaced* become larger in absolute terms. Now, the renters' change in hours is just -0.05 log points, homeowners' increase their hours worked by 0.09 log points, and the net effect is 0.05 log points, none of which are statistically significant. By contrast, the coefficient on *Displaced* falls to -1.61 log points and becomes statistically significant at the 1 percent level, while the interaction term is now 0.37 log points and loses statistically significance. On net, homeowners annual hours worked falls by 1.97 log points (also significant at the 1 percent level).

Results using the same pattern of specifications are reported for claiming Social Security (Columns 4-6) and the logged annual hours worked (Columns 7-9). In both sets of results, adding individual effects generally causes the coefficients to attenuate towards zero. The most saturated regression for being in the labor force (Column 6) shows that renters' labor force participation falls by 1.0 percentage points, while homeowners' rises relative to renters by 1.5 percentage points, for a net effect on homeowners of just a 0.6 percentage point increase. This translates to just a 1 percent increase over the pre-treatment mean. Similarly, Column 9 shows that treated renters have essentially no change in their propensity to claim Social Security, while treated homeowners decrease their propensity to claim Social Security by 0.9 percentage points relative to renters. As with annual hours worked, none of these coefficients are statistically significant. In contrast, Panel B shows being displaced lowers homeowners' net probability of being in the labor force by 15 percentage points (-10 percentage points for renters, both significant at the 1 percent level) but has small and not statistically significant effects on claiming Social Security for either renters or homeowners.

The fact that homeowners leave the labor force at higher rates than renters in response to a job displacement also hints at another reason why housing wealth losses do not seem to

[21]Note that the individual fixed effects also subsume the CZ fixed effects, because I fix individuals to their CZ of residence as of 1998 to prevent endogeneity bias.

change overall labor supply much. Homeowners have, *ceteris paribus*, more secure housing consumption than renters, and job losses are thus less of a threat to that consumption stream. Even homeowners who do not lose their jobs may decide that they prefer to retire or not increase their hours worked if the local labor market weakens and the jobs on offer are poor fits. This could offset, wholly or partially, the impulse to recoup housing wealth losses.

It is possible that there are no housing wealth effects in the overall sample, but certain subsamples may be more likely react in line with the lifecycle model. For example, housing wealth losses may be more keenly felt by workers with lower education levels, those with lower initial wealth, and these subgroups may be more responsive than other workers. Another possibility is that mortgagees may behave more like renters, both because they have less housing wealth and the need to pay the mortgage makes them similarly sensitive about payment risk, and could be biasing the interaction term towards zero. In Appendix A, I report results differentially for men versus women (Table A2), non-college versus college-educated respondents (Table A3), those with no initial wealth outside of housing and Social Security wealth (A4). In Table A4, I also report an analysis that drops mortgagees.

The appendix results help shed some light on why the main results are small and statistically insignificant. Table A2 shows that men tend to reduce labor supply relative to renters and women increase it, but Panel B shows that none of these differences are statistically significant. Presumably, the two offset each other, even in the same household. Non-college and college-educated homeowners both have no differences relative to renters that are statistically significant, although the net effects on college-educated homeowners' logged annual hours worked increases by 0.47 log points (statistically significant at the 5 percent level) and their probability of being in the labor force increases by 5.4 percentage points. Since the interaction term is actually negative for hours worked, this is largely ascribable to the greater power available to the much more numerous homeowners. This is still notable because, on average, housing wealth makes up a greater share of (the much more numerous) non-college workers' total wealth, but could be explained by college-educated workers' greater ability to

continue working even in adverse labor markets [Vuolo et al., 2016].

Lastly, in Table A4 Columns 1-3, I find no evidence that those with no other forms of wealth outside of housing and Social Security behave any differently than similarly situated renters. The net impacts on homeowners (Panel B) are actually quite close to zero for all three outcomes. In Columns 4-6, I report the results when I remove the mortgagees and similarly find no significant differences between homeowners and renters. I note that Panel B shows that, on net, homeowners decrease their propensity to claim Social Security by 1.5 percentage points. However, because the interaction term in Panel A is positive, this suggests that the negative effect on claiming is not driven by wealth effects, but by the greater power available to estimates on homeowners.

2.2 Analysis IIB: The Great Recession

The China Shock results suggest that homeowners do not react to housing wealth losses in a statistically detectable manner, but it is possible that the China Shock's impact on housing prices may not have been salient enough because of the concurrent national housing wealth boom. I thus next use the Great Recession as a testing ground for how older homeowners respond to a highly salient housing wealth shock.

The Great Recession was dated by the National Bureau of Economic Research (NBER) to have lasted from December 2007 to June 2009 whereafter the economy would not enter into a recession again until April 2020 during the early phase of the COVID-19 pandemic. However, as noted by the NBER itself in their press release announcing the end of the Great Recession, "the committee did not conclude that economic conditions since [June 2009] have been favorable or that the economy has returned to operating at normal capacity."[22] In fact, housing prices continued to fall even after the formal end of the recession. The non-seasonally adjusted, purchase-only FHFA HPI peaked nationally in May 2007 and reached its nadir in January 2012, although the exact timing of the peak and trough varies across

[22]Source: `https://www.nber.org/sites/default/files/2021-03/sept2010.pdf`, last accessed March 19, 2025.

cities and neighborhoods [Ferreira and Gyourko, 2012].

As mentioned above, several papers, most notably Zhao and Burge [2017] have already used the Great Recession in a differences-in-differences-in-differences framework to study how older workers respond to housing wealth losses. I depart from Zhao and Burge [2017] in two respects besides using CZs instead of MSAs. One is to stick with the same controls as the ones used for the China Shock analysis because I believe some of Zhao and Burge [2017]'s chosen controls may be endogenous.[23] The second is that I measure the treatment effect slightly differently. Zhao and Burge [2017] define an area as having experienced a housing "bust" if prices dropped by 2 percent or more (nominally) in annual terms. This has the effect of sometimes assigning treatment in some periods to a MSA and then sometimes subsequently reassigning the MSA to the control group. Recent research has shown that these kinds of regressions can yield invalid results [Goodman-Bacon, 2021, Callaway and Sant'Anna, 2021]. I start by extending the treatment period to 2012 to bring the study period closer to the trough of the housing market. For each CZ, I calculate in nominal terms how much the HPI changed from 2007 to 2012.[24] I then sort CZs into quartiles based on the size of the change, and then assign all CZs in the bottom quartile (*i.e.*, those with steepest price declines) to the treatment group.

Appendix Figure A1 shows the 2007-2012 price changes for all U.S. CZs with the bottom quartile (*i.e.*, treated) CZs outlined in darkest red. Appendix Table A6 shows the ranges, means, and medians of price declines for each quartile. Price declines in the treatment group ranged from -57.0 percent to -15.7 percent, with a mean of -27.3 percent and a median of -24.6 percent, which are steep declines by any objective definition. However, the second quartile's range of price declines span -15.7 to -6.6 percent with a mean and median of -10.8

[23]Specifically, I do not include their direct controls for financial wealth or for the number of children at home, because the underlying HRS variable does not specify whether these are minor or non-minor children. During the Great Recession, some adult children moved back in with their parents due to poor job prospects, so the number of children in the home *ex post* cannot be viewed as exogenous, even if the overall effect was relatively small [Bitler and Hoynes, 2015]. Lastly, I also don't include their state-level controls for local tax burden or property taxes, because these are coarse and can vary considerably within a state.

[24]Adjusting for annual inflation makes little difference, as during this time period, inflation rates were extremely modest.

percent, which is more modest but would still be viewed as a significant decline by many families. For this reason, I drop respondents from the second quartile from this analysis.

My base regression is:

$$Outcome_{it} = \beta_0 + \beta_1 Bust_{it} + \beta_2 Bust_{it} \times Homeowner_i + \beta_3 Displaced_{it} + \beta_4 Displaced_{it} \times Homeowner_i + X_{it}\zeta + \iota_i + \gamma_t + \gamma_t \times Homeowner_i + \epsilon_{it} \quad (3)$$

where $Outcome_{it}$ are the same three outcomes used in the China Shock regressions. $Bust_{it}$ is a dummy equal to one if the survey year is between 2008 and 2012 and the respondents' 2004 CZ was in the quartile that experienced the greatest price index declines from 2007 to 2012. Figure 4 shows that all outcomes except log hours worked exhibit behavior consistent with parallel pre-trends. While the lines cross in hours worked, the differences in means are not statistically significant in either 2004 or 2006, and there's a quite pronounced shift in the trend line *ex post* in the treated CZs compared to the control CZs that is in the opposite direction of the pre-trend. While these results should be interpreted with some caution, the control group is nonetheless probably acting as a reasonable proxy for the behavior of the treated group. Appendix Table A5 confirms that mean differences in 2004 for all of the outcome variables between the treatment and control groups are quite small, although there were some differences in the controls, such as a much higher rate of having a mortgage in the treatment CZs, a recent change in better health (perhaps associated with recent movers to the Sunbelt), a higher rate of having a bachelor's degree, and higher home values, housing wealth, and rents.

2.2.1 Great Recession Results

I start in Table 4 reporting results for the probability of being retired in response to the Great Recession. I proceed with the same set of specifications as in Table 3. The pattern of results here is what somewhat different. In Column 1, I use just CZ, survey wave, and

survey wave by (initial) homeownership status fixed effects. Now, older renters increased their hours worked by 0.24 log points and homeowners decreased their hours worked relative to renters by 0.33 log points, a difference statistically significant at the 10 percent level. However, the net effect on homeowners is a decrease in hours of 0.09 log points. Adding state-by-year fixed effects causes renters' hours to increase more in response to *Bust*, rising to 0.31 log points and is statistically significant at the 10 percent level, whereas interaction term falls slightly to -0.31 log points, loses statistical significance, but the resulting net effect on homeowners now rises to slightly above zero. Adding individual fixed effects strongly changes the results: both coefficients attenuate towards zero, with the effect on renters is now just 0.02 log points, the interaction term is now 0.06 log points, and the net effect on homeowners is now 0.09 log points, none of which are statistically significant.

For the other outcomes, *Bust* coefficients also tend to attenuate towards zero as more fixed effects are added. In the fully saturated specification (Column 6), *Bust* has essentially zero effect on renters' labor force participation (LFP), and increases homeowners' LFP by just 1.3 percentage points, where neither effect (nor the net effects) are statistically significant. Similarly, Column 9 reports an estimated effect of *Bust* on renters' probability to claim Social Security of just -0.3 percentage points, an increase for homeowners' relative to renters of 1.1 percentage point, and a net effect on homeowners of just 0.8 percentage points.

Ultimately, the results between the OLS analysis, the China Shock analysis, and this analysis are all quite similar. Homeowners appear not to change their labor supply much, if at all, in response to housing wealth losses. On the other hand, the two causal analyses both find that renters are much less likely to decrease their labor supply in response to rent decreases than the OLS analysis would suggest. This does not rule out that renters' do have a wealth responses, because *Shock* and *Bust* bundle together any wealth response with the effect of the local labor market shock. But it perhaps suggests that the wealth effect does not predominate for this group as well.

In Appendix A, I also report results from the same set of subgroup analyses and ro-

bustness checks that I used for the China Shock analysis. The results in subgroups are broadly the same. Table A7 shows that male and female homeowners have opposite-signed responses to the housing wealth shock: male homeowners decrease labor supply relative to renters and female homeowners increase it. College-educated homeowners have a strong, positive labor supply response, increasing their hours worked by 0.69 log points relative to renters (significant at the percent level), and increasing their hours worked on net by 0.58 log points (significant at the 1 percent level). Similarly, labor force participation rises relative to renters by 5.7 percentage points, and although this difference is not statistically significant, the net effect on homeowners is a large 7.9 percentage points, significant at the 1 percent level. Table A9 shows that effects among those with only housing or Social Security wealth are quite small and statistically insignificant, while the results from stripping out the mortgagees in Columns 4-6 show results fairly similar to those reported in Table 4, suggesting that mortgagees acting like renters are not attenuating the interaction term between *Bust* and *Homeowner* downwards.

3 Analysis III: Unanticipated Hyperlocal Shocks

Given that renters are a imperfect control group at best, I next adopt the approach of Begley and Chan [2018] (hereafter BC) of identifying unexpected housing shocks at the hyperlocal level while using CZ-by-year fixed effects to control for any labor market effects. Like them, I use ZIP codes as my definition of hyperlocal. They use a twice-lagged autoregressive model (AR2) with a time trend and estimate the model on logged metropolitan statistical area (MSA) housing prices. They then fit their model onto the ZIP code-level housing price data they had available covering 2000-2012. Unanticipated movements in housing prices are defined as the difference between the predicted housing price based on the AR2 model and the observed housing price in their ZIP code data:

$$\Delta H_{zw} = \Delta P_{zw} - \Delta \widehat{P_{zw}} \tag{4}$$

where P_{zw} is the observed housing price in ZIP code z in survey wave w. I make two changes to their approach. First, in the intervening years since BC was published, the Federal Housing Finance Agency has published ZIP code level housing price indices with a 1990 base year that cover about half of U.S. ZIP codes and a larger set of ZIP code-level indices with a 2000 base year that covers about 81 percent of ZIP codes. Unsurprisingly, this sample is skewed towards the more populous ZIP codes but its coverage of HRS respondents is reasonably broad: 68 percent of 1992 HRS observations have a ZIP code that has a 1990 based HPI and 86 percent of all observations had a 2000-based non-missing ZIP code-level HPI. I thus use ZIP code-level housing price indices directly instead of using MSA-level housing price data to train the model. One improvement this offers over BC is that my ZIP code-level estimates of housing price shocks will be less contaminated by the MSA-level covarying housing and labor market fluctuations that I am ultimately seeking to purge from the regressions.

To ensure that shocks are only identified from retroactively formed expectations, for each survey wave, I use 20 years worth of prior data to estimate when homeowners may have been surprised (either to the upside or downside) by a housing price shock in between waves. Similar to BC, I do this by estimating an ARMA with a time trend as a second-order polynomial in the form of:

$$ln(HPI_{zt}) = \pi_0 + \pi_1 ln(HPI_{z,t-1}) + \pi_2 ln(HPI_{z,t-2}) + \pi_3 t + \pi_4 t^2 + \nu_{zt} \tag{5}$$

One problem is that the HRS survey waves are biennial meaning that I need to estimate a change in expectations over a two year time period and only use information that the homeowner would have had themselves as of the previous survey wave. I use BC's solution which is to replace the $ln(HPI_{z,t-1})$ (corresponding to the unobserved year) with its predicted

value. This changes Equation 5 to:

$$\begin{aligned} ln(\widehat{HPI_{zt}}) =& \widehat{\pi_0} + \widehat{\pi_1}\left(\widehat{\pi_1} + \widehat{\pi_1} ln(HPI_{z,t-2}) + \widehat{\pi_2} ln(HPI_{z,t-3}) + \widehat{\pi_3}(t-1) + \widehat{\pi_4}(t-1)^2\right) \\ &+ \widehat{\pi_2} ln(HPI_{z,t-2}) + \widehat{\pi_3} t + \widehat{\pi_4} t^2 \end{aligned} \tag{6}$$

For each survey wave, I take the 2000-based housing price index and rebase it to have a value of 1 in $t-2$. Like BC, I exclude individual waves where the person experienced a divorce or a separation, because these often involve substantial asset and wealth changes as well as a change in residence and homeownership status that are nominally unrelated to housing price fluctuations. For people who move between survey waves, I also follow BC in using the estimated housing wealth shock from their originating ZIP code.

One other potential pitfall with the BC approach is that it may be over- or understating the degree of surprise by using a 20 year data window. A 20 year lookback period is going to naturally smooth out fluctuations caused by business cycles and other shocks, but people may pay more attention to more recent movements, such as homeowners who were reasoning during the Housing Boom that prices would continuously increase. As an alternative, I also estimate a second set of unanticipated price movements estimates using 12 years of data with an AR(3) model with a time trend as a second-order polynomial so that the alternative $ln(\widehat{HPI_{zt}})$ is calculated as:

$$\begin{aligned} ln(\widehat{HPI_{zt}}) = \widehat{\lambda_0} + \widehat{\lambda_1}(& \\ & \widehat{\lambda_0} + \widehat{\lambda_1} ln(HPI_{z,t-2}) + \widehat{\lambda_2} ln(HPI_{z,t-3}) + \widehat{\lambda_3} ln(HPI_{z,t-4}) + \widehat{\lambda_4}(t-1) + \widehat{\lambda_5}(t-1)^2 \\ &) + \widehat{\lambda_2} ln(HPI_{z,t-2}) + \widehat{\lambda_3} ln(HPI_{z,t-3}) + \widehat{\lambda_4} t + \widehat{\lambda_5} t^2 \end{aligned} \tag{7}$$

Appendix Figure A2 uses ZIP Codes from Cook County, Illinois to illustrate how these es-

timates work in practice. I show unexpected price shocks for 2004, 2006, and 2008 (spanning the middle of the Housing Boom to the start of the Bust) for both estimation approaches. Broadly speaking, the within-year estimates are pretty similar across the two methods. The AR(3) method more consistently finds both positive and negative unexpected price shocks, particularly prior to 2008, reflecting that its shorter data training period purposefully weights expectations towards more recent changes making surprises more likely in periods like this when housing prices made relatively quick surges and reversals.

Following BC, I group my housing price shocks from both models by size corresponding to an unanticipated between-wave hyperlocal price appreciation of -10 percent or less; between -10 to -5 percent; between -5 to 0 percent; between 5 to 10 percent; and more than 10 percent. Respondents realizing 0 to 5 percent price appreciation shocks serve as the reference group.

My base regression model is:

$$\begin{aligned} Y_{iwt} =& \theta_0 + \theta_1 \mathbb{1}\left\{\Delta H_{zw} < -10\%\right\} + \theta_2 \mathbb{1}\left\{-10\% \leq \Delta H_{zw} < -5\%\right\} \\ &+ \theta_3 \mathbb{1}\left\{-5\% \leq \Delta H_{zw} < 0\%\right\} + \theta_4 \mathbb{1}\left\{5\% \leq \Delta H_{zw} < 10\%\right\} \\ &+ \theta_5 \mathbb{1}\left\{\Delta H_{zw} \geq 10\%\right\} + Z'_{i,w-1}\psi + \sum_{a=50}^{75} \mathbb{1}\left\{Age_{it} = a\right\} + \nu_{it}^{CZ} + \eta_{itw} \end{aligned} \tag{8}$$

where Y_{iwt} is one of the three aforementioned outcomes: transition towards more retirement; reversing retirement and rejoining the labor force; and starting to claim Social Security. $Z_{i,w-1}$ is a matrix of controls that includes the same control variables that are in BC's base specifications (as reported in BC Table 3 on p. 188): a dummy for having a mortgage; having non-mortgage debt; being unemployed; having a fully retired spouse; having employer-sponsored health insurance; having access to retiree health insurance; having a pension at current job; and having a defined-benefit (DB) pension at current job. All of these are measured as of survey wave $w-1$. I similarly include basic demographic control dummies for race and education in $Z_{i,w-1}$ as well as single year of age dummies as of time t. ν_{it}^{CZ} are CZ-by-year fixed effects that absorb coincident local labor market shocks.

In Table 5, I display the summary statistics for the sample based on the AR(2) model. Table 6 shows the summary statistics for the sample based on the AR(3) model. The sample is changed to reflect each outcome variable's inherent dynamics. The "More Retirement" sample is comprised of respondents who self-reported not being fully retired in survey wave $w-1$ and were between ages 50-75 in survey wave w. The "Reverse Retirement" sample is comprised of respondents who self-report being partially or fully retired in $w-1$ and were between ages 50-75 in w. The "Start Claiming Social Security" sample is comprised of respondents who were not claiming Social Security in $w-1$ and were at least 62 in w. Sample means for the outcome variables for the two models have very similar means of the outcome variables: 19 percent of eligible observations transition to retirement; 23 percent reverse retirement; and about 40 percent start claiming Social Security. The average unanticipated housing price movement, ΔH_{zw}, is between -9.0 to -7.7 percentage points in the AR(2) samples, but a substantial minority of respondents (about 20-23 percent) enjoyed unexpected housing price appreciation of greater than 10 percentage points. In contrast, the average value of ΔH_{zw} is between 3.3 to 4 percent in the AR(3). I also report summary statistics for a new set of control variables than used above, because I start my analyses by using a similar set of controls as BC. The average age, share married, widowed, and other basic demographics across subsamples and are also broadly similar to the China Shock and Great Recession samples, although the college degree share is higher here because it includes more recent survey years of the HRS.

3.1 Results for Analysis III

I start by reporting results in Table 7 for transitioning to more retirement in Column 1 where I start with the same set of controls as BC. Column 1 reports that those experiencing a small negative appreciation shock (-5 to 0 percent range) increase their propensity to transition to more retirement by 3.6 percentage points while those experiencing a more than 10 percentage point appreciation shock increased their propensity to transitioning towards more

retirement by 3.4 percentage points, both significant at the 5 percent level. Other categories had small, non-significant estimated responses. In Column 2, I add some additional contemporary controls, including a control for whether the respondent was displaced from their job since the last survey wave as well as the health status dummies, the contemporaneous unemployment rate, and a dummy for becoming age-eligible for Medicare. This causes all of the coefficients on the previously statistically significant appreciation shock dummies to attenuate towards zero and lose significance. This result holds even if I only add *Displaced* as a control but not the other contemporaneous controls. Next, I try a set of controls similar to those I used in the China Shock and Great Recession analyses: dummies for having been displaced from your job since the last survey wave; being married, widowed, age eligible for the old age entitlement; race, educational attainment, as well as a shift-share (Bartik) instrument for total employment, age as a third order polynomial, and the CZ-level unemployment rate both contemporaneously and lagged one survey wave.

The results reported in Column 3 now shows that there are significant declines in the probability of transitioning towards more retirement if the respondent experiences a relatively large negative price appreciation shock. Those experiencing a -10 percent or less appreciation shock decreased their transition probability by 2.4 percentage points while those experiencing declines in the -10 to -5 percent range decreased their transition probability by 3.0 percentage points. Shocks of other magnitudes had non-significant effects of less than 1 percentage point. However, Column 4 shows that if individual fixed effects are added, these effects from relatively large negative appreciation shocks attenuate towards zero and lose significance.[25]

I then repeat the exercise using the results of the AR(3) model, starting again with base set of BC controls. Column 5 shows that this generates the opposite finding as the AR(2) approach: now, a greater than 10 percent appreciation shock decreased the transition probability by 3.6 percentage points. Adding the additional contemporaneous controls causes the effect to fall to -3.7 percentage points, although I note that all of the estimated coefficients

[25]Note that adding individual fixed effects effectively turns these regressions into first differences estimations and removes any individuals who are not observed within-sample for at least two periods.

on appreciation shocks are negative, i.e., relative to the reference group, people experiencing all kinds of shocks became less likely to transition towards more retirement, although only with those experiencing a large, positive shock have a statistically significant response. In Column 7, I switch to the other set of controls and almost all effects attenuate towards zero and the effect on the large, positive shock is no longer statistically significant. Column 8 shows that adding individual fixed effects does not meaningfully change the results here.

The take away from this set of results is that while some specifications show significant effects, the results are unstable and sensitive to which controls are included. Further, different models for how homeowners form housing price expectations can yield opposing results. Further, the most saturated specifications (Columns 4 and 8) show no significant results.

Table 8 shows that this pattern largely holds even when looking at the other two outcomes of interest. For concision, I present results from both the AR(2) and AR(3) models on just two specifications per outcome: the BC controls plus the additional contemporaneous controls mentioned above and the "new" controls plus individual fixed effects. The AR(2) model on reversing retirement using the BC controls+ (Column 1) yields positive coefficients on all housing price shock dummies, but none are statistically significant. Switching control sets and adding individual fixed effects (Column 2) reveals that those experiencing a 5-10 percent unexpected positive housing wealth shock became 3.6 percentage points less likely to reverse retirement (significant at the 5 percent level), but this result might be an aberration because retirement reversal declines also occur among those experiencing a negative housing wealth shock in excess of -10 percent. When instead using the AR(3) model for estimating ΔH_{zw}, Column 3 shows that, compared to Column 1, all of the coefficients flip from positive to negative, although the estimates are small (all between -2 to 0 percentage points) and non-significant. Using instead the new controls and individual fixed effects, Column 4 shows that the negative effects from positive housing wealth shocks on retirement reversal probabilities reported in Column 2 disappear. The effect on retirement reversal from a 5 to 10 percent unexpected housing wealth shock is now just 0.3 percentage points and is not statistically

significant.

The results of housing wealth shocks on starting to claim Social Security are similarly sensitive to the controls used and the method for estimating housing wealth shocks. Columns 5 and 6 show that results from the AR(2) model are not robust to the controls and fixed effects used, with Column 6 (individual fixed effects and the "new" controls) reporting consistently small, not significant results. The AR(3) sample yields a very different set of results. Now, positive housing wealth shocks cause a decline in claiming: Column 7 shows that a 5 to 10 percent unexpected housing wealth gain decreases the probability of claiming by 5.4 percentage points and a greater than 10 percent positive shock by -4.3 percentage points. Column 8 shows that this result, at least, is robust to changing the set of controls and adding individual fixed effects, as the effect on a 5 to 10 percent positive shock becomes -7.4 percentage points while the effect on a greater than 10 percent positive shock is -3.1 percentage points, although this estimate is not statistically significant. It is noteworthy, however, that this is the opposite of the lifecycle model's predictions, as people should become *more* likely to start claiming Social Security in response to these positive shocks.

In Appendix A, I explore whether instead there are subgroups of older workers who are more sensitive to unexpected housing wealth movements than the full sample. I report subsample analyses using the new controls and individual fixed effects for men (Appendix Table A10), women (Appendix Table A11), respondents with a college degree (Appendix Table A13), and those without a college degree (Appendix Table A12). Generally speaking, these subsample analyses find very little evidence that older workers across these subgroups are particularly responsive to housing wealth shocks that is robust to which method is used. The "Start Claiming SS" model using the AR(3) method does find some relatively large effects in the range of -8 to -12 percentage points for men, women, and college grads to become less likely to start claiming Social Security in response to unexpected price increases of 5-10 percent, but also finds that those without a college degree become 9 percentage points less likely to start claiming in the face of unexpected housing wealth losses of 10 percent

or less. Given how implausibly large these results are, the fact the underlying samples are small (between about two to four thousand observations), and that this result appears from just one method, a relatively low credibility weight should be placed on these findings.

The strongest and most robust evidence from these subsamples for a response to housing price changes are that college-educated respondents become less likely to transition to more retirement in response to housing wealth losses. An unexpected price drop of less than -10 percent identified from the AR(2) model causes a 4.2 percent drop in the probability one of these respondents transitioned to more retirement, and a very similar 3.9 percentage point drop in a loss identified by the AR(3) model, both significant at the 5 percent level. This matches the China Shock and Great Recession analyses' findings that college-educated older homeowners increase their labor supply both in relative and absolute terms. These findings collectively suggest that as the share of college-educated workers grows among older Americans, their elasticity of labor supply to changes in housing wealth will rise.

Another possibility for why the main results were mostly null (or contradictory) is that older workers may only respond to significant housing wealth losses (or gains) if these changes are a salient portion of their total wealth. I translate shocks to housing wealth into total wealth shocks by using the inflation-adjusted RAND-constructed measure for net total wealth from $w - 1$ in my denominator.[26] The results are reported in Table A14. For concision, I report results only on the specifications using the "new" controls and alternate between excluding or including individual fixed effects. Generally speaking, effects are small and not significant, particularly once individual fixed effects are added. One exception is that in both the AR(2) and AR(3) samples, positive wealth shocks cause workers to become less likely to start claiming Social Security and this is robust to adding the individual fixed effects: respondents experiencing a 5 to 10 percent positive wealth shock become 4.8 percentage points less likely to start claiming Social Security. This is the opposite of what would be expected from the lifecycle model, which would hold that unexpected total wealth increases

[26]This is "h*atotb" variable in the RAND Fat File [Health and Study, 2024].

should increase older workers' propensity to claim Social Security (which reduces potential wealth). However, I once again caution that this analyses works off a smaller observation count than the other analyses and so may suffer from a small cell problem.

Lastly, I apply test whether unexpected housing price fluctuations affect older renters. While there is not a 1-1 correlation between changes in housing prices and changes in rents, there is a general positive covariance. However Table A15 shows that only the results for transitioning to more retirement in the AR(3) sample are robust to adding fixed effects (Column 8), so I focus on those. Renters experiencing a -5 to 0 percent decline in local housing prices see their probability of transitioning to more retirement decline by 13.7 percentage points (significant at the 1 percent level) and those experiencing a 5 to 10 percent increase in housing prices saw their retirement probabilities also decline by about 8 percentage points (significant at the 5 percent level). These are contradictory results and the relatively small sample sizes might once again be causing type I errors. A more definitive answer to this may need to await better data.

4 Conclusion

This paper finds little conclusive evidence that older homeowners respond to unexpected short-run changes in housing wealth appreciation by changing their labor supply. All three undertaken analyses return essentially the same answer: responses are null or small and not statistically significant. One reason could be that older workers care both about housing wealth and stable housing consumption in retirement, and homeownership may just as readily enable older workers to retire or reduce hours if the local job prospects are unsuitable to their skills or personal circumstances as spur them to recoup losses. The consistent finding that job displacements cause homeowners to reduce their hours and labor force attachment more than renters is suggestive evidence for this idea.

This does not mean that there are no circumstances where older workers would seek to

recoup housing wealth losses via greater labor force attachment. The results in fact suggest that future older Americans may be more responsive as a group than current and previous cohorts. Subgroup analyses consistently show that the college-educated are responsive to housing wealth losses and increase their labor supply accordingly. As they grow as a share of older Americans, future policymakers should start to expect that more older homeowners will stay in the workforce longer to recoup losses. Countervailing that trend is that the growing number of older renters may be especially responsive to changes in rents —increasing their labor supply when rents rise and vice versa. Renters 50 and over have grown as a share of older Americans from 16.3 percent in 2000 to 20.9 percent in 2022 according to Current Population Survey data, so they too may produce larger shifts in the future labor supply of older workers in response to housing market changes. Since current data on older renters are typically comprised of relatively small samples, studying them in more detail would be a fruitful area for future analysis.

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Figure 1: Raw Relationship Between Changes in Various Housing Price Measures and Hours Worked

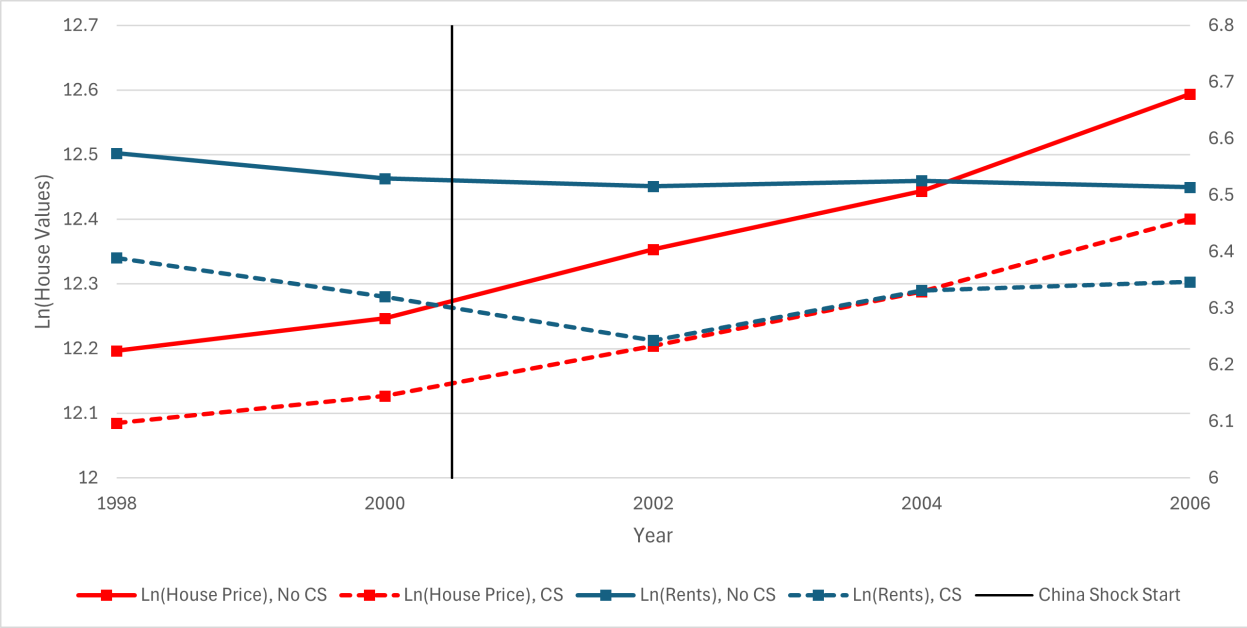


(a) ΔLn(Real House Value) **(b) ΔLn(Real Net Housing Wealth)** **(c) ΔLn(Real Rents)**

SOURCE: Author's calculations using the HRS data.

NOTES: Figure 1 reports the relationship between the change in logged annual hours worked between survey waves in response to changes inflation-adjusted house prices and net housing wealth for homeowners and inflation-adjusted rents among renters. The sample is restricted to HRS respondents aged 50 and over who did not move between survey waves or have a change in marital status.

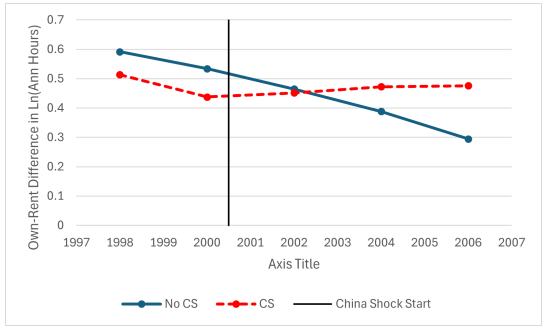
Figure 2: Trends in Housing Prices and Rents by China Shock Status



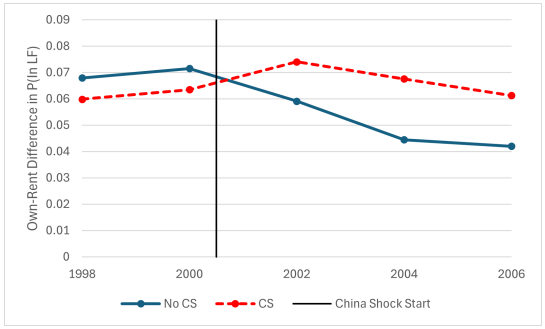
SOURCE: Author's calculations using the HRS data.

NOTES: Figure 2 uses HRS data for respondents who were aged 50 to 78 in 1998 to show trends in logged housing prices (for homeowners) and logged rents (for renters) by China Shock status, defined as whether or not the respondents' 1998 CZ was in the top quartile for greatest exposure to the China Shock (CS) or not (No CS).

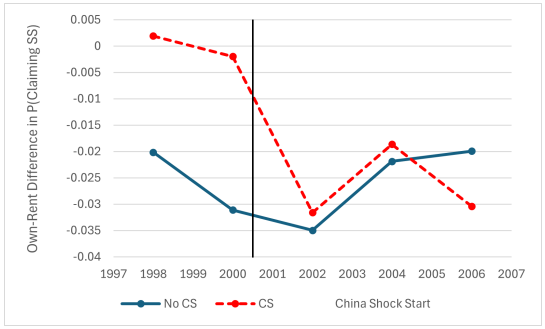
Figure 3: Average Owner-Renter Difference by China Shock Treatment Status



(a) Panel A: Ln(Real House Value)



(b) Panel B: In LF

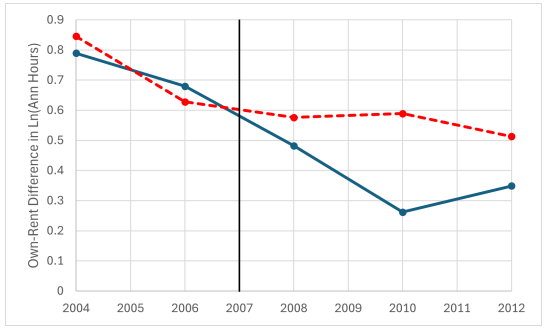


(c) Panel C: Claim Social Security

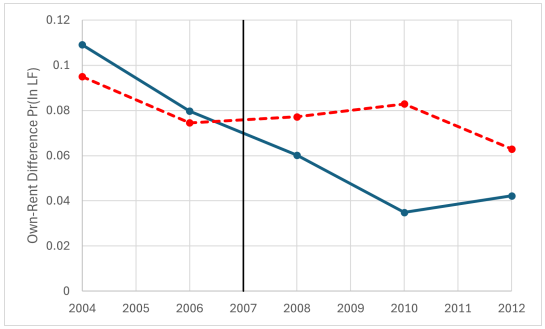
SOURCE: Author's calculations using the HRS data.

NOTES: Figure 3 reports the difference in averages between homeowners and renters in either the treatment region ("CS") or the control region ("No CS"), defined as whether or not the respondents' 1998 CZ was in the top quartile for greatest exposure to the China Shock or not, among HRS respondents aged 50 to 78 in 1998 for three outcomes: the logged annual hours worked; being in the labor force; and claiming Social Security.

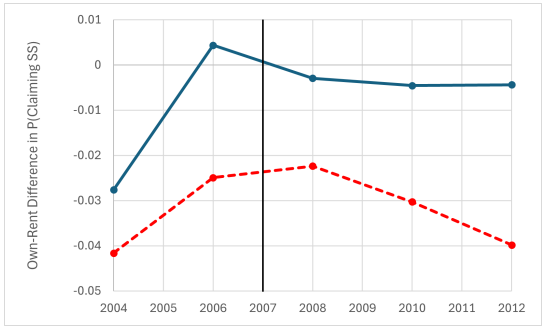
Figure 4: Average Owner-Renter Difference by Change in Housing Prices During Great Recession



(a) Panel A: Ln(Real House Value)



(b) Panel B: In LF



(c) Panel C: Claim Social Security

SOURCE: Author's calculations using the HRS data.

NOTES: Figure 4 reports the difference in averages between homeowners and renters in either the treatment region ("Strong GR") or the control region ("Weak GR"), assigned as whether or not the respondents' 2004 CZ was in the top quartile for greatest housing price declines during the Great Recession and its aftermath (2007-2012), among HRS respondents aged 50 to 78 in 1998 for three outcomes: logged annual hours worked; being in the labor force; and claiming Social Security.

TABLE 1
Summary Statistics for the OLS, China Shock, and Great Recession Samples

	OLS: Homeowners ($N = 104,279$)		**OLS: Renters** ($N = 20,172$)		**China Shock** ($N = 11,847$)		**Great Recession** ($N = 11,160$)	
	mean	sd	mean	sd	mean	sd	mean	sd
Outcomes								
Ln(Ann. Hours Worked)	3.36	3.70	2.61	3.56	3.53	3.74	3.20	3.67
𝟙{In LF}					0.497	0.500	0.462	0.499
𝟙{Retired}					0.374	0.484	0.411	0.492
𝟙{Claim SS}					0.432	0.495	0.503	0.500
Controls								
𝟙{Treated}					0.330	0.470	0.677	0.468
Age	65.8	9.5	65.6	11.2	62.3	8.3	64.4	7.1
𝟙{Displaced}	0.031	0.172	0.033	0.178	0.025	0.158	0.032	0.176
𝟙{Married}	0.696	0.460	0.247	0.431	0.676	0.468	0.683	0.465
𝟙{Widowed}	0.122	0.327	0.183	0.387	0.140	0.347	0.132	0.339
𝟙{Medicare}	0.475	0.499	0.432	0.495	0.398	0.489	0.458	0.498
𝟙{Health: Worse}	0.211	0.408	0.259	0.438	0.144	0.351	0.249	0.432
𝟙{Health: Better}	0.102	0.303	0.105	0.307	0.059	0.235	0.112	0.316
𝟙{Mortgage}	0.435	0.496			0.346	0.476	0.363	0.481
Other								
𝟙{Male}	0.467	0.499	0.410	0.492	0.460	0.498	0.447	0.497
𝟙{White}	0.873	0.333	0.664	0.472	0.862	0.344	0.852	0.355
𝟙{Black}	0.080	0.271	0.233	0.423	0.100	0.300	0.099	0.299
𝟙{College-Educated}	0.309	0.462	0.151	0.358	0.199	0.399	0.242	0.428
𝟙{Homeowner}					0.726	0.446	0.742	0.437
House Value (2024 $)	355,925	586,993			249,917	307,036	396,365	894,658
Net Housing Wealth (2024 $)	275,413	560,359			190,849	279,878	313,168	856,457
Monthly Rent (2024 $)			1,015	2,141	876	940	953	738

Table 1 shows selected summary statistics for all Health and Retirement Study (HRS) respondents by selected characteristics for four analysis samples. The OLS Homeowners sample are homeowners aged 50 and over who did not move in between survey waves and did not have a change in marital status. OLS Renters sample is comprised of renters subject to the same restrictions. The China Shock sample is for respondents aged 50-78 for just 1998, the initial year for that study period. The Great Recession is similarly for respondents aged 50-78 in 2004. Means of indicator variables can be interpreted as shares of the population in question.

TABLE 2
OLS Relationship Between Changes in Housing Prices and Changes in Annual Hours Worked

	$\Delta Ln(House\ Price)$			$\Delta Ln(Net\ Housing\ Wealth)$			$\Delta Ln(Rent)$		
	All	< 0	> 0	All	< 0	> 0	All	< 0	> 0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{iw}	0.017	-0.026	-0.013	0.020	0.012	0.019	0.156**	0.206**	0.118
	(0.024)	(0.032)	(0.048)	(0.014)	(0.019)	(0.021)	(0.060)	(0.094)	(0.099)
Displaced	-3.439***	-3.585***	-3.264***	-3.431***	-3.619***	-3.254***	-3.691***	-3.462***	-4.028***
	(0.122)	(0.164)	(0.162)	(0.128)	(0.170)	(0.158)	(0.239)	(0.303)	(0.325)
Adj. R^2	0.077	0.086	0.076	0.077	0.088	0.075	0.129	0.131	0.163
F	95.39	49.69	36.79	92.52	37.05	47.28	21.69	14.09	12.67
N	76,227	39,702	36,455	74,257	35,462	38,697	13,313	7,821	5,376

Table 2 reports the results from estimating Equation (1) on three different measures of housing price changes (ΔH_{iw}) for respondents aged 50 and over: change in log real housing price and change in log net real housing wealth for homeowners; and change in real rents for renters. Income shocks are identified through job displacements (*Displaced*). Observations where the respondents moved between survey waves or changed marital status are dropped. All regressions use respondent survey weights and are estimated with heteroskedasticity-robust standard errors.

* p<0.10, ** p<0.05, *** p<0.01

TABLE 3
Labor Supply Responses to the China Shock and Job Displacements

	Ln(Ann. Hours Worked)			In Labor Force			Claims SS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Panel A: Coefficient Estimates									
Shock	-0.0605	-0.0786	-0.0466	-0.0117	-0.0132	-0.0095	-0.0036	-0.0101	0.0018
	(0.1093)	(0.1242)	(0.1089)	(0.0147)	(0.0169)	(0.0160)	(0.0114)	(0.0108)	(0.0109)
×Homeowner	0.1219	0.1352	0.0918	0.0192	0.0199	0.0154	0.0055	0.0038	-0.0086
	(0.1392)	(0.1405)	(0.1172)	(0.0200)	(0.0201)	(0.0171)	(0.0110)	(0.0110)	(0.0109)
Displaced	-0.1021	-0.0805	-1.6081***	0.1318***	0.1350***	-0.1016***	0.0066	0.0072	0.0177
	(0.2967)	(0.2985)	(0.2356)	(0.0315)	(0.0312)	(0.0276)	(0.0211)	(0.0213)	(0.0217)
×Homeowner	-0.5053	-0.5334*	-0.3669	-0.0898***	-0.0930***	-0.0478	-0.0214	-0.0218	-0.0297
	(0.3154)	(0.3164)	(0.2528)	(0.0336)	(0.0333)	(0.0327)	(0.0250)	(0.0251)	(0.0281)
Adj. R^2	0.335	0.335	0.728	0.315	0.315	0.707	0.723	0.723	0.808
F	484.4	446.3	46.8	450.1	455.4	36.4	4,405.0	5,164.7	336.6
N	52,885	52,871	51,937	53,795	53,783	52,871	53,795	53,783	52,871
Panel B: Net Effects on Homeowners									
$\frac{dy}{dx}$ Shock	0.0614	0.0566	0.0453	0.0076	0.0067	0.0059	0.0019	-0.0063	-0.0068
	(0.0685)	(0.0811)	(0.0754)	(0.0109)	(0.0116)	(0.0105)	(0.0069)	(0.0088)	(0.0081)
$\frac{dy}{dx}$ Displaced	-0.6074***	-0.6139***	-1.9749***	0.0420**	0.0420**	-0.1493***	-0.0148*	-0.0145*	-0.012
	(0.1181)	(0.1169)	(0.1198)	(0.0166)	(0.0165)	(0.0172)	(0.0088)	(0.0088)	(0.0107)
State-Year FEs	N	Y	Y	N	Y	Y	N	Y	Y
Individual FEs	N	N	Y	N	N	Y	N	N	Y

SOURCE: Author's calculations using the HRS data.

NOTES: Table 3 reports the results from estimating Equation 2 on a housing wealth shock identified through being in a CZ highly exposed to Chinese import competition (*Shock*). *Displaced* identifies income shocks via job displacements. Coefficients in rows without the *Homeowner* term can be interpreted as the effect on renters, and coefficients in rows with that term can be interpreted as the effect on homeowners relative to renters. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE 4
Labor Supply Responses to the Great Recession and Job Displacements

	Ln(Ann. Hours Worked)			In Labor Force			Claims SS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Panel A: Coefficient Estimates									
Bust	0.2414	0.3145*	0.0242	0.0318	0.0370	-0.0006	-0.0137	-0.0127	-0.0028
	(0.1538)	(0.1788)	(0.1504)	(0.0204)	(0.0261)	(0.0210)	(0.0138)	(0.0155)	(0.0158)
×Homeowner	-0.3286*	-0.3096	0.0629	-0.0395	-0.0369	0.0130	0.0173	0.0152	0.0111
	(0.1862)	(0.1904)	(0.1652)	(0.0247)	(0.0252)	(0.0203)	(0.0173)	(0.0175)	(0.0172)
Displaced	-0.9836***	-0.9795***	-2.4280***	0.0756***	0.0765***	-0.1478***	-0.0044	-0.0043	-0.0135
	(0.2058)	(0.2076)	(0.1800)	(0.0258)	(0.0260)	(0.0264)	(0.0183)	(0.0185)	(0.0175)
×Homeowner	-0.2698	-0.2616	-0.0101	-0.0405	-0.0397	-0.0058	0.0180	0.0172	0.0267
	(0.2250)	(0.2287)	(0.2165)	(0.0297)	(0.0301)	(0.0329)	(0.0215)	(0.0216)	(0.0212)
Adj. R^2	0.324	0.325	0.725	0.312	0.314	0.705	0.706	0.707	0.794
F	275.4	229.3	56.3	269.8	250.5	33.5	1,854.2	1,876.8	306.8
N	53,885	53,881	53,055	54,962	54,958	54,157	54,962	54,958	54,157
Panel B: Net Effects on Homeowners									
$\frac{dy}{dx}$ Bust	-0.0871	0.0049	0.0871	-0.0076	0.0001	0.0124	0.0036	0.0025	0.0082
	(0.0936)	(0.1106)	(0.0992)	(0.0121)	(0.0185)	(0.0153)	(0.0085)	(0.0086)	(0.0092)
$\frac{dy}{dx}$ Displaced	-1.2534***	-1.2411***	-2.4381***	0.0351**	0.0367**	-0.1535***	0.0136	0.0129	0.0132
	(0.1242)	(0.1248)	(0.1161)	(0.0159)	(0.0159)	(0.0156)	(0.0099)	(0.0099)	(0.0103)
State-Year FEs	N	Y	Y	N	Y	Y	N	Y	Y
Individual FEs	N	N	Y	N	N	Y	N	N	Y

SOURCE: Author's calculations using the HRS data.

NOTES: Table 4 reports the results from estimating Equation 2 on a housing wealth shock identified through being in a CZ in the top quartile of housing price declines between 2007-2012 (*Bust*). *Displaced* identifies income shocks through job displacements. Coefficients in rows without the *Homeowner* term can be interpreted as the effect on renters, and coefficients in rows with that term can be interpreted as the effect on homeowners relative to the renters' response. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE 5
Summary Statistics for the AR(2) on 20 Years of Data Subsamples

	More Retirement N=42,819		**Reverse Retirement** N=26,788		**Start Claiming Social Security** N=13,437	
	mean	sd	mean	sd	mean	sd
Outcome	0.187	0.390	0.228	0.420	0.392	0.488
Housing Price Shock Treatments						
ΔH_{zw}	-0.090	0.309	-0.087	0.295	-0.077	0.310
$\mathbb{1}\{< -10\%\}$	0.364	0.481	0.359	0.480	0.341	0.474
$\mathbb{1}\{-10\% \text{ to } -5\%\}$	0.122	0.327	0.131	0.337	0.115	0.319
$\mathbb{1}\{-5\% \text{ to } 0\%\}$	0.112	0.316	0.116	0.320	0.113	0.316
$\mathbb{1}\{5\% \text{ to } 10\%\}$	0.079	0.270	0.078	0.268	0.079	0.270
$\mathbb{1}\{> 10\%\}$	0.208	0.406	0.202	0.401	0.231	0.422
Controls						
Age	61.8	5.9	67.6	5.3	64.6	2.5
$\mathbb{1}\{\text{Male}\}$	0.427	0.495	0.459	0.498	0.466	0.499
$\mathbb{1}\{\text{White}\}$	0.790	0.407	0.815	0.388	0.799	0.401
$\mathbb{1}\{\text{Black}\}$	0.132	0.338	0.141	0.348	0.133	0.340
$\mathbb{1}\{\text{College Educated}\}$	0.339	0.474	0.299	0.458	0.379	0.485
$\mathbb{1}\{\text{Married}\}$	0.762	0.426	0.750	0.433	0.773	0.419
$\mathbb{1}\{\text{Widowed}\}$	0.055	0.229	0.091	0.288	0.049	0.217
$\mathbb{1}\{\text{Displaced}\}$	0.048	0.213	0.021	0.144	0.036	0.187
$\mathbb{1}\{\text{Unemployed}\}$	0.028	0.165	0.019	0.135	0.021	0.144
No. Children in HH	2.89	1.84	3.03	1.90	2.93	1.83
$\mathbb{1}\{\text{Non-Housing Debt}\}$	0.384	0.486	0.308	0.462	0.341	0.474
$\mathbb{1}\{\text{Medicare}\}$	0.313	0.464	0.734	0.442	0.403	0.490
$\mathbb{1}\{\text{Has Mortgage}\}$	0.556	0.497	0.378	0.485	0.500	0.500
$\mathbb{1}\{\text{Pension At Current Job}\}$	0.346	0.476	0.038	0.192	0.252	0.434
$\mathbb{1}\{\text{DB Pension At Current Job}\}$	0.168	0.374	0.017	0.129	0.118	0.323
$\mathbb{1}\{\text{Health Insurance At Current Job}\}$	0.445	0.497	0.265	0.441	0.462	0.499
$\mathbb{1}\{\text{Has Retiree Health Insurance}\}$	0.095	0.293	0.058	0.233	0.124	0.330
$\mathbb{1}\{\text{Spouse Retired}\}$	0.318	0.466	0.537	0.499	0.412	0.492
Health Status						
Good	0.313	0.464	0.330	0.470	0.315	0.464
Fair	0.365	0.481	0.337	0.473	0.367	0.482
Poor	0.143	0.350	0.103	0.304	0.137	0.343

SOURCE: Author's calculations using the HRS data.

NOTES: Table 5 shows selected summary statistics for people who were homeowners in survey wave $w-1$ and met each subsamples' inclusion criterion. The "More Retirement" subsample comprises respondents who self-reported as not being fully retired in survey wave $w-1$ and were between the ages of 50-75 in survey wave w. "Reverse Retirement" comprises respondents who self-report as being partially or fully retired in survey wave $w-1$ and were between the ages of 50-75 in survey wave w. "Start Claiming Social Security" comprises respondents who were at least 62 in w and were not claiming Social Security in $w-1$. All subsamples drop respondents who got divorced, married, or separated between $w-1$ and w and respondents whose ZIP Code in $w-1$ did not have an HPI consistently observed for at least 20 years up to that point.

TABLE 6
Summary Statistics for the AR(3) on 12 Years of Data Sample

	More Retirement N=42,819		Reverse Retirement N=26,788		Start Claiming Social Security N=13,437	
	mean	sd	mean	sd	mean	sd
Outcome	0.188	0.391	0.233	0.423	0.410	0.492
Housing Price Shock Treatments						
ΔH_{zw}	0.037	0.272	0.033	0.264	0.040	0.269
$\mathbb{1}\{< -10\%\}$	0.182	0.386	0.185	0.388	0.175	0.380
$\mathbb{1}\{-10\% \text{ to } -5\%\}$	0.073	0.260	0.077	0.266	0.072	0.259
$\mathbb{1}\{-5\% \text{ to } 0\%\}$	0.103	0.304	0.107	0.309	0.106	0.308
$\mathbb{1}\{5\% \text{ to } 10\%\}$	0.143	0.351	0.142	0.349	0.145	0.352
$\mathbb{1}\{> 10\%\}$	0.362	0.481	0.346	0.476	0.364	0.481
Controls						
Age	61.6	5.8	67.2	5.3	64.4	2.5
$\mathbb{1}\{\text{Male}\}$	0.424	0.494	0.477	0.499	0.477	0.499
$\mathbb{1}\{\text{White}\}$	0.804	0.397	0.824	0.381	0.805	0.396
$\mathbb{1}\{\text{Black}\}$	0.134	0.340	0.140	0.347	0.140	0.347
$\mathbb{1}\{\text{College Educated}\}$	0.299	0.458	0.274	0.446	0.334	0.472
$\mathbb{1}\{\text{Married}\}$	0.776	0.417	0.766	0.424	0.788	0.409
$\mathbb{1}\{\text{Widowed}\}$	0.059	0.235	0.089	0.285	0.050	0.218
$\mathbb{1}\{\text{Displaced}\}$	0.044	0.206	0.022	0.146	0.036	0.186
$\mathbb{1}\{\text{Unemployed}\}$	0.024	0.152	0.017	0.131	0.018	0.133
No. Children in HH	3.02	1.89	3.11	1.94	3.06	1.89
$\mathbb{1}\{\text{Non-Housing Debt}\}$	0.371	0.483	0.299	0.458	0.330	0.470
$\mathbb{1}\{\text{Medicare}\}$	0.291	0.454	0.706	0.456	0.372	0.483
$\mathbb{1}\{\text{Has Mortgage}\}$	0.525	0.499	0.359	0.480	0.464	0.499
$\mathbb{1}\{\text{Pension At Current Job}\}$	0.342	0.474	0.039	0.193	0.244	0.430
$\mathbb{1}\{\text{DB Pension At Current Job}\}$	0.176	0.381	0.018	0.131	0.120	0.325
$\mathbb{1}\{\text{Health Insurance At Current Job}\}$	0.451	0.498	0.286	0.452	0.472	0.499
$\mathbb{1}\{\text{Has Retiree Health Insurance}\}$	0.109	0.311	0.066	0.248	0.131	0.337
$\mathbb{1}\{\text{Spouse Retired}\}$	0.316	0.465	0.528	0.499	0.411	0.492
Health Status						
Good	0.313	0.464	0.329	0.470	0.318	0.466
Fair	0.352	0.478	0.327	0.469	0.350	0.477
Poor	0.144	0.351	0.106	0.308	0.133	0.340

SOURCE: Author's calculations using the HRS data.

NOTES: Table 6 shows selected summary statistics for people who were homeowners in survey wave $w-1$ and met each subsamples' inclusion criterion. The "More Retirement" subsample comprises respondents who self-reported as not being fully retired in survey wave $w-1$ and were between the ages of 50-75 in survey wave w. "Reverse Retirement" comprises respondents who self-report as being partially or fully retired in survey wave $w-1$ and were between the ages of 50-75 in survey wave w. "Start Claiming Social Security" comprises respondents who were at least 62 in w and were not claiming Social Security in $w-1$. All subsamples drop respondents who got divorced, married, or separated between $w-1$ and w and respondents whose ZIP Code in $w-1$ did not have an HPI consistently observed for at least 12 years up to that point.

TABLE 7
Results Using Unexpected ZIP Code-Level Price Shocks on More Retirement

	AR(2) Sample				AR(3) Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{zw} (Unexpected Housing Wealth Shock)								
$< -10\%$	-0.0092	-0.0189	-0.0243*	-0.0153	0.0046	-0.0184	-0.0022	-0.0139
	(0.0175)	(0.0190)	(0.0135)	(0.0174)	(0.0183)	(0.0199)	(0.0137)	(0.0151)
-10% to -5%	-0.0006	-0.0112	-0.0299**	-0.0248	-0.0082	-0.0127	-0.0161	-0.0047
	(0.0155)	(0.0184)	(0.0141)	(0.0178)	(0.0217)	(0.0227)	(0.0150)	(0.0158)
-5% to 0%	0.0355**	0.0274	0.0017	0.0135	0.0118	-0.0031	0.0003	-0.0037
	(0.0175)	(0.0187)	(0.0149)	(0.0173)	(0.0170)	(0.0162)	(0.0122)	(0.0144)
5% to 10%	0.0132	-0.0069	-0.0046	-0.0115	-0.0193	-0.0225	-0.0070	-0.0012
	(0.0191)	(0.0207)	(0.0159)	(0.0198)	(0.0152)	(0.0163)	(0.0107)	(0.0126)
$> 10\%$	0.0342**	0.0079	0.0026	0.0203	-0.0355**	-0.0371**	-0.0177	-0.015
	(0.0165)	(0.0184)	(0.0159)	(0.0153)	(0.0148)	(0.0153)	(0.0129)	(0.0141)
Adj. R^2	0.093	0.096	0.088	0.114	0.09	0.093	0.089	0.126
F	8.4	18.3	94.9	20.7	13.8	19.6	89.2	15.1
N	19,789	17,813	25,563	22,770	22,572	20,704	29,416	25,506
BC Controls	Y	Y	N	N	Y	Y	N	N
BC Controls+	N	Y	N	N	N	Y	N	N
New Controls	N	N	Y	Y	N	N	Y	Y
Individual FEs	N	N	N	Y	N	N	N	Y

SOURCE: Author's calculations using the HRS data.

NOTES: Table 7 presents results on how unexpected housing wealth shocks change older workers' propensity to transition to either partial or full retirement. The sample is restricted to respondents aged 50-75 who self-reported not being "fully" retired in survey wave $w-1$ and who did not experience a change in marital status. The AR(2) Sample corresponds to ΔH_{zw} calculated using Equation (6) only on respondents whose ZIP code in $w-1$ had 20 years of prior data reported, while the AR(3) Sample corresponds to ΔH_{zw} calculated using Equation (7) only on respondents whose ZIP code in $w-1$ had 12 years of prior data reported. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE 8
Results Using Unexpected ZIP Code-Level Price Shocks on Other Outcomes

	Reverse Retirement				Start Claiming SS			
	AR(2) Subsample		AR(3) Subsample		AR(2) Subsample		AR(3) Subsample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{zw} (Unexpected Housing Wealth Shock)								
$< -10\%$	0.0291	-0.0143	-0.0156	-0.0233	-0.0430	-0.0131	-0.0060	-0.0199
	(0.0207)	(0.0150)	(0.0214)	(0.0165)	(0.0310)	(0.0402)	(0.0304)	(0.0289)
-10% to -5%	0.0264	0.0044	-0.0154	-0.0185	-0.0046	0.0053	-0.0232	-0.0147
	(0.0191)	(0.0142)	(0.0197)	(0.0141)	(0.0287)	(0.0385)	(0.0290)	(0.0382)
-5% to 0%	0.0019	-0.0207	-0.0174	0.0090	-0.0607*	-0.0038	-0.0395	-0.0050
	(0.0188)	(0.0133)	(0.0184)	(0.0152)	(0.0334)	(0.0420)	(0.0319)	(0.0315)
5% to 10%	0.0236	-0.0362**	-0.0108	0.0031	-0.0287	-0.0146	-0.0543**	-0.0743**
	(0.0220)	(0.0174)	(0.0144)	(0.0122)	(0.0371)	(0.0443)	(0.0252)	(0.0336)
$> 10\%$	0.0102	-0.0229	-0.0002	-0.0077	-0.0385	-0.0204	-0.0432*	-0.0312
	(0.0187)	(0.0179)	(0.0123)	(0.0120)	(0.0301)	(0.0406)	(0.0258)	(0.0345)
Adj. R^2	0.167	0.611	0.167	0.604	0.242	0.374	0.237	0.407
F	94.946	8.592	105.148	9.296	37.736	71.391	24.971	75.538
N	15,945	16,665	17,650	17,418	7,061	6,254	8,442	6,694
BC Controls+	Y	N	Y	N	Y	N	Y	N
New Controls	N	Y	N	Y	N	Y	N	Y
Individual FEs	N	Y	N	Y	N	Y	N	Y

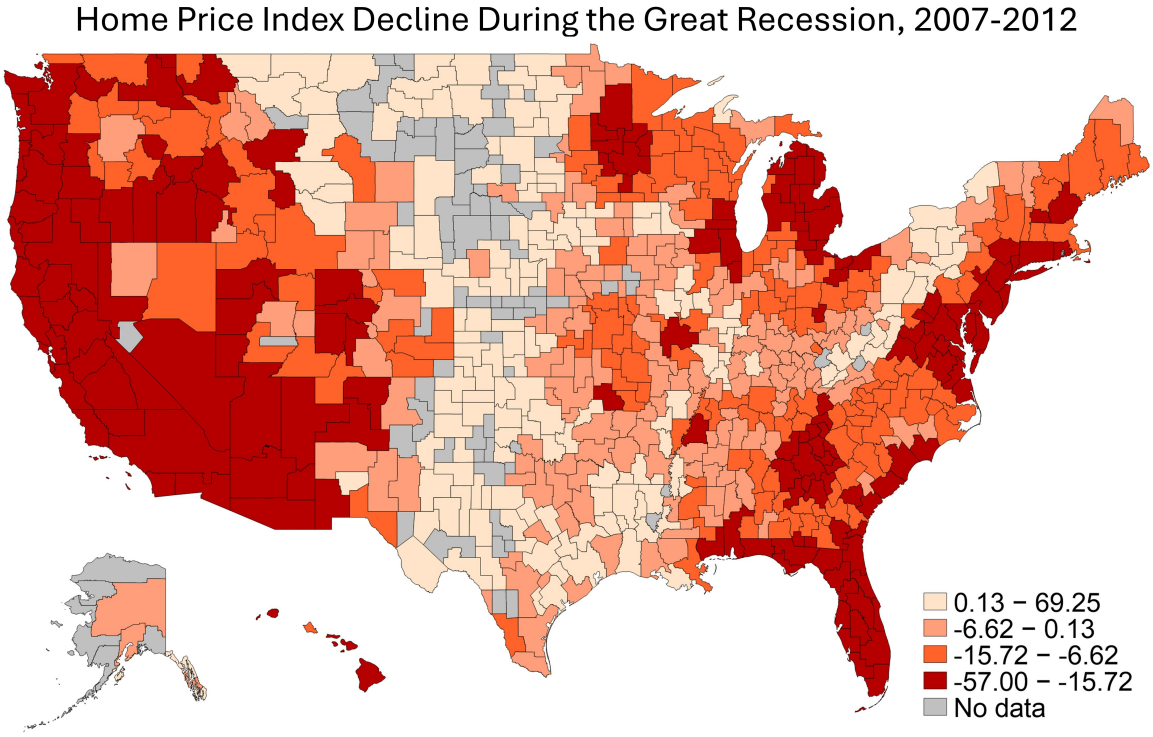
SOURCE: Author's calculations using the HRS data.

NOTES: Table 8 presents results on how unexpected housing wealth shocks change older workers' propensity to reverse retirement or to start claiming Social Security. The "Reverse Retirement" sample is restricted to respondents aged 50-75 who self-reported as being "fully" retired in survey wave $w-1$. The "Start Claiming SS" sample is restricted to respondents aged 62 and over in survey wave w and who were not already claiming Social Security in survey wave $w-1$. Both samples only include respondents who did not experience a change in marital status. The AR(2) Sample corresponds to ΔH_{zw} calculated using Equation (6) only on respondents whose ZIP code in $w-1$ had 20 years of prior data reported, while the AR(3) Sample corresponds to ΔH_{zw} calculated using Equation (7) only on respondents whose ZIP code in $w-1$ had 12 years of prior data reported. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

A Appendix: Additional Figures and Tables

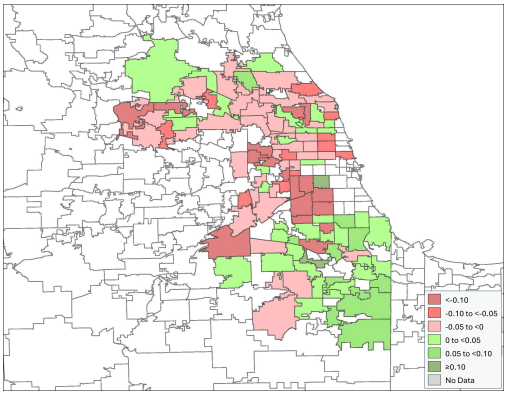
Figure A1: Average Housing Price Decline by Commuting Zone During the Great Recession and Its Immediate Aftermath (2007-2012).



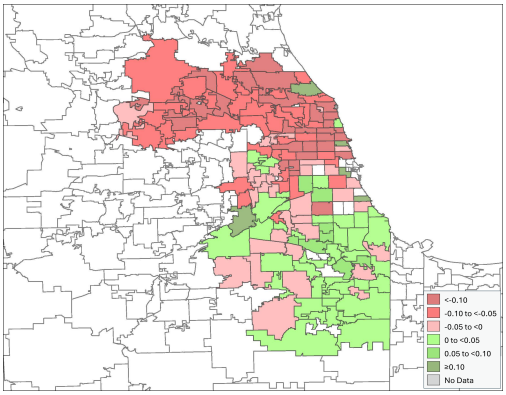
SOURCE: Author's calculations using the FHFA's county-level Housing Price Index (HPI) data and generating CZ-level HPIs as the 1990 population-weighted average of the county-level HPIs.
NOTES: Figure A1 shows commuting zone (shaded by quartile) of the change in the FHFA's Housing Price Index (HPI) from national peak (2007) to trough (2012).

Figure A2: Unexpected Price Shocks by Estimation Model for Cook County, IL

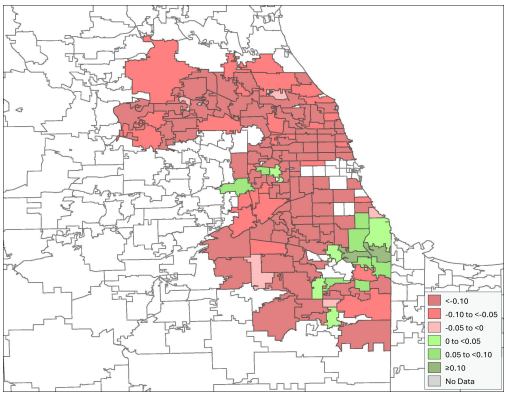
Panel A: AR(2), 2004



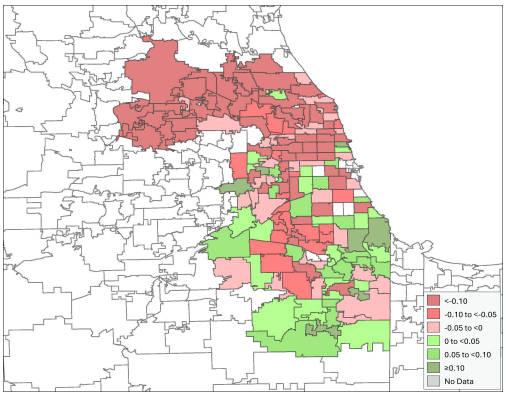
Panel B: AR(3), 2004



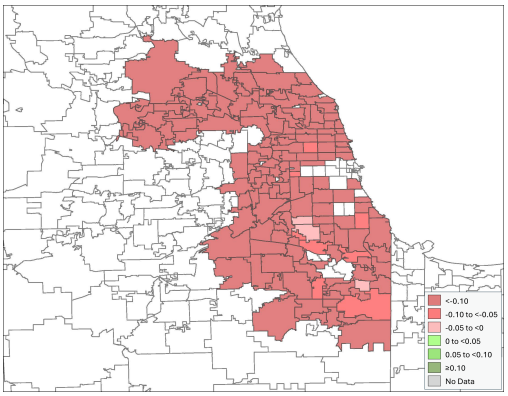
Panel C: AR(2), 2006



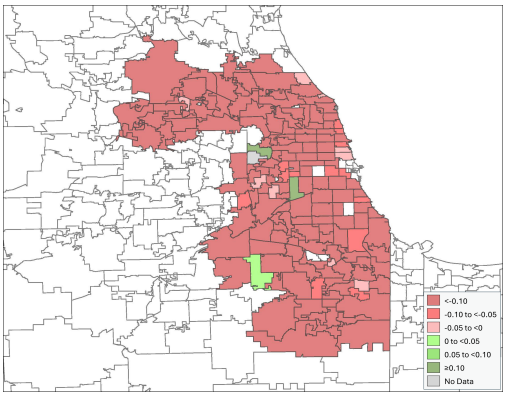
Panel D: AR(3), 2006



Panel E: AR(2), 2008



Panel F: AR(3), 2008



SOURCE: Author's calculations using the FHFA's ZIP-code level HPI data.

NOTE: Figure A2 shows the ZIP code-level unexpected price movements for Cook County for 2004, 2006, and 2008, spanning both the mid-2000's housing boom and the start of the bust.

TABLE A1
Treatment vs Control Difference in Means in China Shock Sample

	Treatment Mean	Control Mean	Mean Difference	t-test	P-value
Outcomes					
Ln(Ann. Hours Worked)	3.300	3.645	-0.344	-1.930*	0.055
𝟙{In LF}	0.468	0.511	-0.043	-1.765*	0.079
𝟙{Retired}	0.363	0.379	-0.016	-0.908	0.365
𝟙{Claim SS}	0.431	0.432	-0.001	-0.031	0.975
Controls					
Age	62.29	62.32	-0.03	-0.066	0.947
𝟙{Displaced}	0.026	0.025	0.001	0.182	0.856
𝟙{Married}	0.682	0.672	0.010	0.551	0.582
𝟙{Widowed}	0.150	0.135	0.014	1.301	0.195
𝟙{Medicare}	0.392	0.401	-0.008	-0.368	0.713
𝟙{Health: Worse}	0.165	0.133	0.031	3.926***	0.000
𝟙{Health: Better}	0.056	0.060	-0.004	-0.548	0.584
𝟙{Mortgage}	0.315	0.361	-0.045	-1.114	0.267
Other					
𝟙{Male}	0.464	0.458	0.006	0.556	0.579
𝟙{White}	0.868	0.860	0.009	0.237	0.813
𝟙{Black}	0.091	0.105	-0.014	-0.449	0.654
𝟙{College-Educated}	0.170	0.214	-0.044	-1.491	0.137
𝟙{Homeowner}	0.708	0.734	-0.026	-1.051	0.294
House Value (2024 $)	238,299	255,436	-17137	-0.317	0.752
Net Housing Value (2024 $)	180,670	195,683	-15013	-0.440	0.660
Monthly Rent (2024 $)	850	887	-37	-0.197	0.844

SOURCE: Author's calculations using HRS data.
NOTES: Table A1 shows the treatment and control means at sample start (1998), the difference between the means, and the t-test and corresponding p-value for whether that difference in means is statistically significant.
* p<0.10, ** p<0.05, *** p<0.01

TABLE A2
Subgroup Responses to the China Shock and Job Displacement
Men versus Women

	Men			Women		
	Ln(Ann. Hrs Worked)	In LF	Claim SS	Ln(Ann. Hrs Worked)	In LF	Claim SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Panel A: Coefficient Estimates						
Shock	0.2532	0.0358	0.0145	-0.1278	-0.0129	0.0019
	(0.2464)	(0.0336)	(0.0186)	(0.1386)	(0.0203)	(0.0149)
×Homeowner	-0.1248	-0.0185	0.0032	0.1116	0.0124	-0.0117
	(0.2660)	(0.0340)	(0.0170)	(0.1570)	(0.0232)	(0.0148)
Displaced	-2.3827***	-0.1505***	-0.0045	-1.5673***	-0.1032**	0.0390
	(0.2588)	(0.0472)	(0.0281)	(0.3155)	(0.0432)	(0.0333)
×Homeowner	0.1622	0.0679	0.0156	-0.3738	-0.0548	-0.0450
	(0.3213)	(0.0531)	(0.0348)	(0.3255)	(0.0455)	(0.0402)
Adj. R^2	0.713	0.689	0.801	0.721	0.702	0.809
F	423.5	423.2	202.9	43.8	70.7	1,266.1
N	22,645	23,158	23,158	29,565	30,040	30,040
Panel B: Net Effects on Homeowners						
$\frac{dy}{dx}$ Shock	0.1284	0.0173	0.0176	-0.0162	-0.0005	-0.0098
	(0.1296)	(0.0200)	(0.0162)	(0.0965)	(0.0144)	(0.0116)
$\frac{dy}{dx}$ Displaced	-2.2206***	-0.0827***	0.0111	-1.9412***	-0.1580***	-0.0059
	(0.1808)	(0.0212)	(0.0160)	(0.2034)	(0.0233)	(0.0141)

SOURCE: Author's calculations using HRS data.

NOTES: Table A2 reports the results from estimating Equation 2 on a housing wealth shock identified through being in a CZ highly exposed to Chinese import competition (*Shock*) for men (Columns 1-3) and women (Columns 4-6) separately. *Displaced* identifies income shocks via job displacements. Coefficients in rows without the *Homeowner* term can be interpreted as the effect on renters, and coefficients in rows with that term can be interpreted as the effect on homeowners relative to renters. All regressions include individual fixed effects and state-by-year fixed effects and use respondent survey weights. Standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A3
Subgroup Responses to the China Shock and Job Displacement
Non-College Versus College-Educated Respondents

	No BA			BA		
	Ln(Ann. Hrs Worked)	In LF	Claim SS	Ln(Ann. Hrs Worked)	In LF	Claim SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Panel A: Coefficient Estimates						
Shock	-0.1446	-0.0159	0.0017	0.5247	0.0294	0.0230
	(0.1161)	(0.0170)	(0.0118)	(0.3797)	(0.0572)	(0.0308)
×Homeowner	0.1059	0.0134	-0.0064	-0.0534	0.0249	-0.0099
	(0.1236)	(0.0185)	(0.0110)	(0.3693)	(0.0503)	(0.0276)
Displaced	-1.5871***	-0.1003***	0.0379*	-1.6418**	-0.1037	-0.1213**
	(0.2330)	(0.0308)	(0.0207)	(0.8229)	(0.0900)	(0.0527)
×Homeowner	-0.4315*	-0.0477	-0.0581**	-0.1572	-0.0511	0.1429**
	(0.2473)	(0.0338)	(0.0270)	(0.8622)	(0.0949)	(0.0582)
Adj. R^2	0.714	0.692	0.805	0.749	0.729	0.813
F	43.7	37.7	298.4	10.5	7.1	135.4
N	42,219	42,946	42,946	9,704	9,909	9,909
Panel B: Net Effects on Homeowners						
$\frac{dy}{dx}$ Shock	-0.0388	-0.0026	-0.0047	0.4712**	0.0543*	0.0131
	(0.0841)	(0.0130)	(0.0087)	(0.1910)	(0.0307)	(0.0201)
$\frac{dy}{dx}$ Displaced	-2.0187***	-0.1480***	-0.0202*	-1.7990***	-0.1549***	0.0215
	(0.1421)	(0.0192)	(0.0121)	(0.2866)	(0.0365)	(0.0177)

SOURCE: Author's calculations using HRS data.

NOTES: Table A3 reports the results from estimating Equation 2 on a housing wealth shock identified through being in a CZ highly exposed to Chinese import competition (*Shock*) for respondents without a BA (Columns 1-3) and those with a BA (Columns 4-6) separately. *Displaced* identifies income shocks via job displacements. Coefficients in rows without the *Homeowner* term can be interpreted as the effect on renters, and coefficients in rows with that term can be interpreted as the effect on homeowners relative to renters. All regressions include individual fixed effects and state-by-year fixed effects and use respondent survey weights. Standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A4
Wealth and Mortgage Analyses of the China Shock and Job Displacement

	No Initial Non-Housing, Non-SS Wealth			Renters vs Non-Mortgage Holders		
	Ln(Ann. Hrs Worked)	In LF	Claim SS	Ln(Ann. Hrs Worked)	In LF	Claim SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Panel A: Coefficient Estimates						
Shock	-0.1763	-0.0207	-0.0046	-0.1781	-0.0187	-0.0183
	(0.1490)	(0.0185)	(0.0198)	(0.1789)	(0.0247)	(0.0183)
×Homeowner	0.1820	0.0305	-0.0032	0.1940	0.0245	0.0033
	(0.1762)	(0.0232)	(0.0195)	(0.1779)	(0.0229)	(0.0185)
Displaced	-1.3642***	-0.0704**	0.0237	-1.6335***	-0.0904**	0.0028
	(0.2768)	(0.0312)	(0.0251)	(0.3314)	(0.0374)	(0.0292)
×Homeowner	-0.6488*	-0.0671*	-0.0482	-0.1888	-0.0603	-0.0200
	(0.3518)	(0.0388)	(0.0336)	(0.3442)	(0.0406)	(0.0360)
Adj. R^2	0.724	0.706	0.774	0.713	0.693	0.781
F	18.1	12.7	132.0	21.1	12.5	256.7
N	18,747	19,086	19,086	25,325	25,812	25,812
Panel B: Net Effects on Homeowners						
$\frac{dy}{dx}$ Shock	0.0057	0.0099	-0.0078	0.0159	0.0058	-0.0150*
	(0.1402)	(0.0182)	(0.0182)	(0.1098)	(0.0144)	(0.0083)
$\frac{dy}{dx}$ Displaced	-2.0130***	-0.1375***	-0.0245	-1.8223***	-0.1506***	-0.0172
	(0.1941)	(0.0242)	(0.0169)	(0.1555)	(0.0219)	(0.0144)

SOURCE: Author's calculations using HRS data.

NOTES: Table A4 reports the results from estimating Equation 2 on a housing wealth shock identified through being in a CZ highly exposed to Chinese import competition (*Shock*) for respondents with only housing or Social Security wealth (Columns 1-3) and comparing renters to non-mortgage holders (Columns 4-6) separately. *Displaced* identifies income shocks via job displacements. Coefficients in rows without the *Homeowner* term can be interpreted as the effect on renters, and coefficients in rows with that term can be interpreted as the effect on homeowners relative to renters. All regressions include individual fixed effects and state-by-year fixed effects and use respondent survey weights. Standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A5
Treatment vs Control Difference in Means in Great Recession Sample

	Treatment Mean	Control Mean	Mean Difference	t-test	P-value
Outcomes					
Ln(Ann. Hours Worked)	3.257	3.088	0.169	1.059	0.290
𝟙{In LF}	0.469	0.448	0.021	0.959	0.338
𝟙{Retired}	0.418	0.397	0.020	1.178	0.240
𝟙{Claim SS}	0.494	0.522	-0.028	-1.423	0.156
Controls					
𝟙{Displaced}	0.035	0.026	0.009	2.082**	0.038
Age	64.46	64.39	0.072	0.263	0.793
𝟙{Married}	0.671	0.706	-0.034	-2.046**	0.042
𝟙{Widowed}	0.131	0.135	-0.004	-0.343	0.732
𝟙{Medicare}	0.457	0.461	-0.004	-0.195	0.845
𝟙{Health: Worse}	0.244	0.259	-0.015	-1.158	0.248
𝟙{Health: Better}	0.117	0.101	0.016	1.915*	0.056
𝟙{Mortgage}	0.407	0.272	0.136	4.983***	0.000
Other					
𝟙{Male}	0.445	0.452	-0.007	-0.947	0.344
𝟙{White}	0.839	0.880	-0.041	-1.330	0.184
𝟙{Black}	0.114	0.068	0.046	2.050**	0.041
𝟙{College-Educated}	0.268	0.189	0.079	3.520***	0.000
𝟙{Homeowner}	0.745	0.736	0.009	0.359	0.720
House Value (2024 $)	485,217	208,017	277,200	5.937***	0.000
Net Housing Value (2024 $)	383,349	164,397	218,952	5.501***	0.000
Monthly Rent (2024 $)	1,035	699	336	4.568***	0.000

SOURCE: Author's calculations using HRS data.
NOTES: Table A5 shows the treatment and control means at sample start (2004), the difference between the means, and the t-test and corresponding p-value for whether that difference in means is statistically significant.
* p<0.10, ** p<0.05, *** p<0.01

TABLE A6
Summary Statistics of the Great Recession's Effect On Housing Price Appreciation Sorted by Quartile

	Mean	Minimum	Median	Maximum
Quartile				
1	-27.26	-57.01	-24.56	-15.72
2	-10.81	-15.68	-10.83	-6.62
3	-3.16	-6.57	-3.03	0.13
4	7.02	0.17	4.16	69.25

Table A6 shows the mean, median, minimum, and maximum estimated treatment effect of the Great Recession on CZ-level housing price appreciation by quartile.

TABLE A7
Subgroup Responses to the Great Recession and Job Displacement Shocks
Men Versus Women

	Men			Women		
	Ln(Ann. Hrs Worked)	In LF	Claim SS	Ln(Ann. Hrs Worked)	In LF	Claim SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Panel A: Coefficient Estimates						
Bust	0.2532	0.0358	0.0145	-0.1092	-0.0166	0.0045
	(0.2464)	(0.0336)	(0.0186)	(0.1639)	(0.0233)	(0.0203)
×Homeowner	-0.1248	-0.0185	0.0032	0.1295	0.0230	0.0034
	(0.2660)	(0.0340)	(0.0170)	(0.1798)	(0.0232)	(0.0219)
Displaced	-2.3827***	-0.1505***	-0.0045	-2.4952***	-0.1504***	-0.0252
	(0.2588)	(0.0472)	(0.0281)	(0.2676)	(0.0398)	(0.0218)
×Homeowner	0.1622	0.0679	0.0156	-0.1554	-0.0761	0.0421
	(0.3213)	(0.0531)	(0.0348)	(0.3361)	(0.0483)	(0.0266)
Adj. R^2	0.713	0.689	0.801	0.729	0.712	0.791
F	423.5	423.2	202.9	38.7	16.8	136.1
N	22,645	23,158	23,158	30,389	30,978	30,978
Panel B: Net Effects on Homeowners						
$\frac{dy}{dx}$ Bust	0.1284	0.0173	0.0176	0.0203	0.0064	0.0079
	(0.1296)	(0.0200)	(0.0162)	(0.1220)	(0.0178)	(0.0094)
$\frac{dy}{dx}$ Displaced	-2.2206***	-0.0827***	0.0111	-2.6506***	-0.2265***	0.0168
	(0.1808)	(0.0212)	(0.0160)	(0.1808)	(0.0213)	(0.0140)

SOURCE: Author's calculations using HRS data.

Table A7 on a housing wealth shock identified through being in a CZ in the top quartile of housing price declines between 2007-2012 (*Bust*) for men (Columns 1-3) and women (Columns 4-6) separately. *Displaced* identifies income shocks via job displacements. Coefficients in rows without the *Homeowner* term can be interpreted as the effect on renters, and coefficients in rows with that term can be interpreted as the effect on homeowners relative to renters. All regressions include individual fixed effects and state-by-year fixed effects and use respondent survey weights. Standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A8
Subgroup Responses to the Great Recession and Job Displacement Shocks
Non-College Versus College-Educated Respondents

	No BA			BA		
	Ln(Ann. Hrs Worked)	In LF	Claim SS	Ln(Ann. Hrs Worked)	In LF	Claim SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Panel A: Coefficient Estimates						
Bust	-0.0404	-0.0095	0.0174	-0.1096	0.0213	-0.0098
	(0.1579)	(0.0218)	(0.0163)	(0.4126)	(0.0495)	(0.0390)
×Homeowner	-0.0426	0.0009	0.0024	0.6912*	0.0573	0.0098
	(0.1548)	(0.0189)	(0.0190)	(0.4119)	(0.0484)	(0.0404)
Displaced	-2.2547***	-0.1399***	-0.0175	-3.3177***	-0.1786**	0.0067
	(0.1882)	(0.0239)	(0.0161)	(0.4819)	(0.0865)	(0.0706)
×Homeowner	-0.3117	-0.0189	0.0295	1.1447**	0.0384	0.0078
	(0.2240)	(0.0320)	(0.0205)	(0.5289)	(0.0916)	(0.0754)
Adj. R^2	0.716	0.695	0.788	0.719	0.698	0.802
F	41.3	26.4	348.9	19.4	7.0	90.1
N	40,651	41,442	41,442	12,389	12,701	12,701
Panel B: Net Effects on Homeowners						
$\frac{dy}{dx}$ Bust	-0.0830	-0.0086	0.0198*	0.5815***	0.0786***	0.0000
	(0.1255)	(0.0185)	(0.0102)	(0.1826)	(0.0207)	(0.0157)
$\frac{dy}{dx}$ Displaced	-2.5664***	-0.1588***	0.0120	-2.1730***	-0.1402***	0.0145
	(0.1404)	(0.0187)	(0.0132)	(0.2021)	(0.0263)	(0.0175)

SOURCE: Author's calculations using HRS data.
NOTES: Table A8 on a housing wealth shock identified through being in a CZ in the top quartile of housing price declines between 2007-2012 (*Bust*) for respondents without a BA (Columns 1-3) and those with a BA (Columns 4-6) separately. *Displaced* identifies income shocks via job displacements. Coefficients in rows without the *Homeowner* term can be interpreted as the effect on renters, and coefficients in rows with that term can be interpreted as the effect on homeowners relative to renters. All regressions include individual fixed effects and state-by-year fixed effects and use respondent survey weights. Standard errors are clustered at the CZ level.
* p<0.10, ** p<0.05, *** p<0.01

TABLE A9
Wealth and Mortgage Analyses of the Great Recession and Job Displacement

	No Initial Non-Housing, Non-SS Wealth			Renters vs Non-Mortgage Holders		
	Ln(Ann. Hrs Worked)	In LF	Claim SS	Ln(Ann. Hrs Worked)	In LF	Claim SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Panel A: Coefficient Estimates						
Bust	0.0782	-0.0071	-0.0221	-0.1017	-0.0183	0.0107
	(0.1862)	(0.0226)	(0.0165)	(0.1764)	(0.0254)	(0.0192)
×Homeowner	-0.0041	0.0223	0.0043	0.1666	0.0165	0.0078
	(0.1889)	(0.0251)	(0.0164)	(0.1781)	(0.0247)	(0.0206)
Displaced	-2.1304***	-0.1245***	-0.0146	-2.4985***	-0.1280***	-0.0306
	(0.2373)	(0.0325)	(0.0133)	(0.2373)	(0.0309)	(0.0225)
×Homeowner	-0.4313	0.0202	0.0130	-0.0423	-0.0115	0.0081
	(0.2801)	(0.0403)	(0.0173)	(0.3047)	(0.0420)	(0.0306)
Adj. R^2	0.708	0.695	0.743	0.733	0.722	0.796
F	30.7	11.8	11.3	21.4	9.9	174.3
N	20,723	21,210	21,210	22,586	23,000	23,000
Panel B: Net Effects on Homeowners						
$\frac{dy}{dx}$ Bust	0.0740	0.0152	-0.0178	0.0649	-0.0018	0.0185
	(0.2040)	(0.0252)	(0.0145)	(0.1424)	(0.0188)	(0.0131)
$\frac{dy}{dx}$ Displaced	-2.5617***	-0.1043***	-0.0016	-2.5408***	-0.1395***	-0.0225
	(0.1896)	(0.0250)	(0.0105)	(0.1938)	(0.0294)	(0.0223)

SOURCE: Author's calculations using HRS data.

Table A9 on a housing wealth shock identified through being in a CZ in the top quartile of housing price declines between 2007-2012 (*Bust*) for respondents with only housing or Social Security wealth (Columns 1-3) and comparing renters to non-mortgage holders (Columns 4-6) separately. *Displaced* identifies income shocks via job displacements. Coefficients in rows without the *Homeowner* term can be interpreted as the effect on renters, and coefficients in rows with that term can be interpreted as the effect on homeowners relative to renters. All regressions include individual fixed effects and state-by-year fixed effects and use respondent survey weights. Standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A10
Hyperlocal Unexpected Shocks Results Among Men Only

	AR(2) Sample			AR(3) Sample		
	More Retirement	Reverse Retirement	Start Claiming SS	More Retirement	Reverse Retirement	Start Claiming SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{zw} (Unexpected Housing Wealth Shock)						
$< -10\%$	-0.0120	-0.0155	0.0071	-0.0194	-0.0118	-0.0333
	(0.0260)	(0.0201)	(0.0600)	(0.0238)	(0.0204)	(0.0518)
-10% to -5%	0.0003	0.0090	0.0082	0.0008	-0.0145	-0.0587
	(0.0274)	(0.0228)	(0.0685)	(0.0272)	(0.0198)	(0.0569)
-5% to 0%	0.0108	-0.0215	-0.0070	-0.0101	0.0157	-0.0833*
	(0.0261)	(0.0222)	(0.0685)	(0.0286)	(0.0204)	(0.0487)
5% to 10%	-0.0128	-0.0338	-0.0011	0.0141	0.0015	-0.0642
	(0.0264)	(0.0253)	(0.0794)	(0.0182)	(0.0225)	(0.0477)
$\geq 10\%$	0.0209	-0.0223	0.0313	-0.0235	-0.0105	-0.0683
	(0.0274)	(0.0286)	(0.0650)	(0.0197)	(0.0164)	(0.0536)
Adj. R^2	0.127	0.639	0.355	0.136	0.636	0.401
F	9.1	6.1	44.5	.	13.1	35.8
N	10,114	7,740	2,894	11,263	8,294	3,150

SOURCE: Author's calculations using HRS data.

NOTES: Table A10 presents results on how unexpected housing wealth shocks change men's propensity to transition to more retirement, reverse retirement, or to start claiming Social Security. The "More Retirement" sample is restricted to respondents aged 50-75 who self-reported not being "fully" retired in survey wave $w-1$. The "Reverse Retirement" sample is restricted to respondents aged 50-75 who self-reported as being "fully" retired in survey wave $w-1$. The "Start Claiming SS" sample is restricted to respondents aged 62 and over in survey wave w and who were not already claiming Social Security in survey wave $w-1$. All samples only include respondents who did not experience a change in marital status. The AR(2) Sample corresponds to ΔH_{zw} calculated using Equation (6) only on respondents whose ZIP code in $w-1$ had 20 years of prior data reported, while the AR(3) Sample corresponds to ΔH_{zw} calculated using Equation (7) only on respondents whose ZIP code in $w-1$ had 12 years of prior data reported. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A11
Hyperlocal Unexpected Shocks Results Among Women Only

	AR(2) Sample			AR(3) Sample		
	More Retirement	Reverse Retirement	Start Claiming SS	More Retirement	Reverse Retirement	Start Claiming SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{zw} (Unexpected Housing Wealth Shock)						
$< -10\%$	-0.0219	-0.0134	-0.0339	-0.0113	-0.0333	-0.0249
	(0.0252)	(0.0205)	(0.0554)	(0.0218)	(0.0218)	(0.0485)
-10% to -5%	-0.0445*	0.0009	-0.0273	-0.0071	-0.0184	0.0295
	(0.0227)	(0.0205)	(0.0481)	(0.0237)	(0.0185)	(0.0500)
-5% to 0%	0.0134	-0.0190	-0.0076	-0.0036	0.0131	0.0478
	(0.0242)	(0.0197)	(0.0498)	(0.0180)	(0.0176)	(0.0497)
5% to 10%	-0.0120	-0.0292	-0.0272	-0.0226	0.0134	-0.0820*
	(0.0274)	(0.0264)	(0.0619)	(0.0183)	(0.0181)	(0.0411)
$\geq 10\%$	0.0254	-0.0061	-0.0796*	-0.0154	-0.0004	-0.0018
	(0.0229)	(0.0194)	(0.0435)	(0.0199)	(0.0167)	(0.0386)
Adj. R^2	0.105	0.589	0.375	0.117	0.574	0.389
F	.	.	24.1	.	.	37.2
N	12,456	8,787	3,103	13,984	8,923	3,232

SOURCE: Author's calculations using HRS data.

NOTES: Table A11 presents results on how unexpected housing wealth shocks change women's propensity to transition to more retirement, reverse retirement, or to start claiming Social Security. The "More Retirement" sample is restricted to respondents aged 50-75 who self-reported not being "fully" retired in survey wave $w-1$. The "Reverse Retirement" sample is restricted to respondents aged 50-75 who self-reported as being "fully" retired in survey wave $w-1$. The "Start Claiming SS" sample is restricted to respondents aged 62 and over in survey wave w and who were not already claiming Social Security in survey wave $w-1$. All samples only include respondents who did not experience a change in marital status. The AR(2) Sample corresponds to ΔH_{zw} calculated using Equation (6) only on respondents whose ZIP code in $w-1$ had 20 years of prior data reported, while the AR(3) Sample corresponds to ΔH_{zw} calculated using Equation (7) only on respondents whose ZIP code in $w-1$ had 12 years of prior data reported. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A12
Hyperlocal Unexpected Shocks Results on Respondents Without a BA

	AR(2) Sample			AR(3) Sample		
	More Retirement	Reverse Retirement	Start Claiming SS	More Retirement	Reverse Retirement	Start Claiming SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{zw} (Unexpected Housing Wealth Shock)						
$< -10\%$	0.0072	-0.0120	-0.0207	-0.0011	-0.0219	-0.0923***
	(0.0242)	(0.0187)	(0.0549)	(0.0204)	(0.0192)	(0.0342)
-10% to -5%	-0.0032	-0.0046	0.0177	0.0116	-0.0214	-0.0326
	(0.0235)	(0.0180)	(0.0613)	(0.0186)	(0.0144)	(0.0420)
-5% to 0%	-0.0020	-0.0257	-0.0364	0.0094	0.0018	-0.0216
	(0.0193)	(0.0175)	(0.0513)	(0.0167)	(0.0191)	(0.0395)
5% to 10%	-0.0174	-0.0480**	0.0473	-0.0018	0.0065	-0.0288
	(0.0301)	(0.0208)	(0.0539)	(0.0145)	(0.0132)	(0.0481)
$\geq 10\%$	0.0070	-0.0305*	0.0015	-0.0028	-0.0072	-0.0451
	(0.0248)	(0.0183)	(0.0499)	(0.0162)	(0.0133)	(0.0365)
Adj. R^2	0.103	0.565	0.442	0.127	0.570	0.450
F	19.2	4.6	23.8	15.9	9.5	15.5
N	14,360	11,562	3,601	17,136	12,546	4,219

SOURCE: Author's calculations using HRS data.

NOTES: Table A12 presents results on how unexpected housing wealth shocks change the propensity of respondents without a college degree to transition to more retirement, reverse retirement, or to start claiming Social Security. The "More Retirement" sample is restricted to respondents aged 50-75 who self-reported not being "fully" retired in survey wave $w-1$. The "Reverse Retirement" sample is restricted to respondents aged 50-75 who self-reported as being "fully" retired in survey wave $w-1$. The "Start Claiming SS" sample is restricted to respondents aged 62 and over in survey wave w and who were not already claiming Social Security in survey wave $w-1$. All samples only include respondents who did not experience a change in marital status. The AR(2) Sample corresponds to ΔH_{zw} calculated using Equation (6) only on respondents whose ZIP code in $w-1$ had 20 years of prior data reported, while the AR(3) Sample corresponds to ΔH_{zw} calculated using Equation (7) only on respondents whose ZIP code in $w-1$ had 12 years of prior data reported. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A13
Hyperlocal Unexpected Shocks Results on Respondents With a BA

	AR(2) Sample			AR(3) Sample		
	More Retirement	Reverse Retirement	Start Claiming SS	More Retirement	Reverse Retirement	Start Claiming SS
	(1)	(2)	(3)	(4)	(5)	(6)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{zw} (Unexpected Housing Wealth Shock)						
$< -10\%$	-0.0422*	-0.0156	0.0105	-0.0392*	-0.0121	0.0745
	(0.0241)	(0.0261)	(0.0492)	(0.0230)	(0.0278)	(0.0647)
-10% to -5%	-0.0514*	0.0309	-0.0236	-0.0337	0.0098	-0.0535
	(0.0262)	(0.0321)	(0.0703)	(0.0289)	(0.0264)	(0.0646)
-5% to 0%	0.0529	0.0115	0.0487	-0.0269	0.0300	-0.0224
	(0.0348)	(0.0261)	(0.0608)	(0.0336)	(0.0276)	(0.0643)
5% to 10%	-0.0155	-0.0030	-0.1098	-0.0026	-0.0010	-0.1215*
	(0.0349)	(0.0356)	(0.0825)	(0.0274)	(0.0302)	(0.0651)
$\geq 10\%$	0.0119	0.0161	-0.0885	-0.0374	-0.0149	0.0122
	(0.0259)	(0.0334)	(0.0629)	(0.0282)	(0.0254)	(0.0684)
Adj. R^2	0.144	0.681	0.348	0.133	0.672	0.380
F	10.4	9.1	41.2	8.6	6.3	58.9
N	8,283	4,992	2,457	8,214	4,693	2,217

SOURCE: Author's calculations using HRS data.

NOTES: Table A13 presents results on how unexpected housing wealth shocks change the propensity of respondents with a college degree to transition to more retirement, reverse retirement, or to start claiming Social Security. The "More Retirement" sample is restricted to respondents aged 50-75 who self-reported not being "fully" retired in survey wave $w-1$. The "Reverse Retirement" sample is restricted to respondents aged 50-75 who self-reported as being "fully" retired in survey wave $w-1$. The "Start Claiming SS" sample is restricted to respondents aged 62 and over in survey wave w and who were not already claiming Social Security in survey wave $w-1$. All samples only include respondents who did not experience a change in marital status. The AR(2) Sample corresponds to ΔH_{zw} calculated using Equation (6) only on respondents whose ZIP code in $w-1$ had 20 years of prior data reported, while the AR(3) Sample corresponds to ΔH_{zw} calculated using Equation (7) only on respondents whose ZIP code in $w-1$ had 12 years of prior data reported. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A14
Results Using Housing-Induced Shocks to Total Wealth

	AR(2) Sample						AR(3) Sample					
	More Retirement		Reverse Retirement		Start Claiming SS		More Retirement		Reverse Retirement		Start Claiming SS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{zw} (Unexpected Housing Wealth Shock)												
$< -10\%$	-0.031**	-0.005	0.002	-0.002	-0.017	0.005	-0.016	-0.014	-0.038*	-0.010	-0.032	0.011
	(0.012)	(0.014)	(0.019)	(0.014)	(0.027)	(0.039)	(0.012)	(0.014)	(0.020)	(0.017)	(0.028)	(0.034)
-10% to -5%	-0.022	-0.004	0.007	-0.012	-0.030	-0.030	-0.005	-0.003	-0.019	-0.015	0.036	0.086***
	(0.014)	(0.015)	(0.017)	(0.013)	(0.027)	(0.042)	(0.013)	(0.015)	(0.018)	(0.017)	(0.028)	(0.033)
-5% to 0%	0.005	0.011	0.002	-0.006	0.006	-0.014	-0.012	-0.010	-0.009	0.001	-0.013	-0.014
	(0.013)	(0.016)	(0.014)	(0.010)	(0.023)	(0.033)	(0.011)	(0.011)	(0.014)	(0.011)	(0.022)	(0.024)
5% to 10%	0.005	0.020	0.024	-0.017	-0.023	-0.065	-0.016	-0.015	-0.007	0.008	-0.042**	-0.048*
	(0.019)	(0.022)	(0.020)	(0.017)	(0.024)	(0.045)	(0.011)	(0.011)	(0.013)	(0.011)	(0.020)	(0.028)
$\geq 10\%$	-0.016	0.006	-0.002	-0.004	-0.043*	-0.023	-0.030***	-0.018	-0.009	0.007	-0.038**	0.014
	(0.017)	(0.017)	(0.019)	(0.013)	(0.025)	(0.033)	(0.011)	(0.014)	(0.016)	(0.015)	(0.019)	(0.028)
Adj. R^2	0.089	0.114	0.074	0.61	0.187	0.375	0.089	0.126	0.077	0.604	0.192	0.409
F	106.2	20.0	48.1	7.2	115.1	87.2	90.8	18.2	45.0	9.5	129.3	92.8
N	25,537	22,747	19,009	16,655	9,069	6,250	29,390	25,478	20,550	17,411	10,556	6,690
Individual FEs	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y

SOURCE: Author's calculations using HRS data.

NOTES: Table A14 presents results on how unexpected total wealth shocks via unexpected housing price movements change respondents' propensity to transition to more retirement, reverse retirement, or to start claiming Social Security. The "More Retirement" sample is restricted to respondents aged 50-75 who self-reported not being "fully" retired in survey wave $w-1$. The "Reverse Retirement" sample is restricted to respondents aged 50-75 who self-reported as being "fully" retired in survey wave $w-1$. The "Start Claiming SS" sample is restricted to respondents aged 62 and over in survey wave w and who were not already claiming Social Security in survey wave $w-1$. All samples only include respondents who did not experience a change in marital status. The AR(2) Sample corresponds to ΔH_{zw} calculated using Equation (6) only on respondents whose ZIP code in $w-1$ had 20 years of prior data reported, while the AR(3) Sample corresponds to ΔH_{zw} calculated using Equation (7) only on respondents whose ZIP code in $w-1$ had 12 years of prior data reported. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE A15
Results on Renters Experiencing Unanticipated Housing Price Shocks

	AR(2) Sample						AR(3) Sample					
	More Retirement		Reverse Retirement		Start Claiming SS		More Retirement		Reverse Retirement		Start Claiming SS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
ΔH_{zw} (Unexpected Housing Wealth Shock)												
$< -10\%$	-0.003	-0.066	0.051	0.005	-0.094	0.188	0.002	-0.002	0.055	0.008	0.051	0.059
	(0.025)	(0.047)	(0.045)	(0.043)	(0.083)	(0.120)	(0.021)	(0.045)	(0.036)	(0.030)	(0.079)	(0.140)
-10% to -5%	-0.008	-0.046	0.014	0.038	-0.169*	0.137	-0.048*	-0.042	0.048	0.041	-0.016	0.107
	(0.029)	(0.047)	(0.042)	(0.056)	(0.090)	(0.150)	(0.026)	(0.044)	(0.042)	(0.041)	(0.074)	(0.097)
-5% to 0%	-0.009	0.009	0.062	0.034	-0.181**	-0.129	-0.030	-0.137***	0.046	-0.017	0.057	-0.000
	(0.035)	(0.046)	(0.051)	(0.035)	(0.091)	(0.150)	(0.025)	(0.038)	(0.046)	(0.043)	(0.071)	(0.152)
5% to 10%	-0.031	-0.045	-0.002	0.036	-0.073	-0.027	-0.024	-0.080**	0.050	-0.030	0.094	0.087
	(0.041)	(0.062)	(0.051)	(0.042)	(0.139)	(0.210)	(0.027)	(0.039)	(0.038)	(0.043)	(0.076)	(0.121)
$\geq$10%	-0.011	-0.073	0.023	0.007	-0.044	-0.025	0.006	-0.021	0.036	0.028	0.102	0.092
	(0.036)	(0.054)	(0.044)	(0.042)	(0.091)	(0.166)	(0.021)	(0.035)	(0.030)	(0.027)	(0.065)	(0.130)
Adj. R^2	0.108	0.088	0.155	0.632	0.243	0.445	0.088	0.126	0.146	0.587	0.235	0.433
F	32.0	17.6	12.1	10.3	37.3	779.2	24.7	11.5	6.7	10.4	34.4	.
N	5,764	4,547	3,195	2,263	1,567	827	5,786	4,081	3,286	2,154	1,641	733
Indiv. FEs	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y

SOURCE: Author's calculations using HRS data.

NOTES: Table A15 presents results on how unexpected housing price movements affect renters' propensity to transition to more retirement, reverse retirement, or to start claiming Social Security. The "More Retirement" sample is restricted to respondents aged 50-75 who self-reported not being "fully" retired in survey wave $w-1$. The "Reverse Retirement" sample is restricted to respondents aged 50-75 who self-reported as being "fully" retired in survey wave $w-1$. The "Start Claiming SS" sample is restricted to respondents aged 62 and over in survey wave w and who were not already claiming Social Security in survey wave $w-1$. All samples only include respondents who did not experience a change in marital status. The AR(2) Sample corresponds to ΔH_{zw} calculated using Equation (6) only on respondents whose ZIP code in $w-1$ had 20 years of prior data reported, while the AR(3) Sample corresponds to ΔH_{zw} calculated using Equation (7) only on respondents whose ZIP code in $w-1$ had 12 years of prior data reported. All regressions use respondent survey weights and standard errors are clustered at the CZ level.

* p<0.10, ** p<0.05, *** p<0.01

B Appendix: China Shock Estimation

The empirical strategy I use to estimate which areas received the strongest negative shocks to their housing value appreciations mimics the approach used in FS who also estimated the impact of trade shocks on local housing prices. The key difference between my work and theirs is that they sought to look at changes to median housing values to ultimately examine the impact on public good provision by way of changes in property tax collections, which are sensitive to changes in levels. By contrast, I am seeking to estimate foregone housing wealth appreciation, given that homeowners already factor in their initial housing wealth stock into retirement decision making, and instead have to worry more about their appreciation prospects. The change in import penetration per worker in local labor markets is measured as:

$$\Delta IPW_{it} = \sum_j \left(\frac{L_{ijt_{initial}}}{L_{jt_{initial}}} \right) \frac{\Delta M_{jt}^C}{L_{it_{initial}}}, \tag{9}$$

where ΔM_{jt}^C is the change in the real value (in 2007 dollars) of imports from China in sector j in $t = \{2000, 2007\}$. $L_{ijt_{initial}}$ is the total employment during the initial time period, $t_{initial}$, in commuting zone i and sector j, where j is defined as either tradeable exposed, tradeable nonexposed, and nontradeable nonexposed.[27] Correspondingly, $L_{jt_{initial}}$ is the total initial employment in sector j and $L_{it_{initial}}$ is the total initial employment in commuting zone i.

To address concerns that unobserved local demand shocks occurred concurrently with the introduction of Chinese import competition that would occlude identification of the causal effects of freer trade with China, I follow the ADH and FS convention and instrument for US import penetration per worker using the previous period's import penetration per worker from eight other high-income countries.[28] This instrument takes the form:

$$\Delta IPW_{oit} = \sum_j \frac{L_{ijt_{initial-1}}}{L_{jt_{initial-1}}} \frac{\Delta M_{jt}^O}{L_{it_{initial-1}}} \tag{10}$$

where ΔM_{jt}^O is the change in the real value of goods imported from China to the 8 other high-income countries in sector j. Using 1980 employment figures instead of 1990 employment figures mitigates the possibility that contemporary employment was not reacting to anticipated changes in Chinese trade terms.

I then estimate the impacts of Chinese import competition on housing price appreciation as:

$$\Delta HPI_{it} = \alpha + \beta \Delta IPW_{it} + X_{it}\theta + \delta_t + \epsilon_{it} \tag{11}$$

where ΔHPI_{it} is the change in housing price appreciation over two time periods, 1990-2000

[27] See ADH and FS for more information how these classifications were performed.

[28] These are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland. See ADH for more information.

and 2000-2007,[29] where the latter period is rescaled to yield a ten-year equivalent change. X_{it} is composed of the same controls used by FS.[30] I also include Census division timetrends to capture region-specific trends. Standard errors are clustered at the commuting zone level.

HPI_{it} is drawn from the Federal Housing Finance Agency's Housing Price Appreciation County Indices.[31] For many counties, they report the indices normalized to 1990, which I use to be synchronous with the FS approach. As the FHFA do not generate these indices for commuting zones, I create commuting zone-level indices by taking the annual average across all counties in a commuting zone, weighted by the number of owner-occupied housing units in each county in 1990. For 199 commuting zones, no counties had 1990 normalized indices, so these observations are dropped from the regressions.

The results from estimating Equation (11) are in Table B1. I present the OLS and IV results as (a) without X_{it}, (b) with X_{it} controls, and (c) with X_{it} and Census division-specific time trends. It is clear that in all cases, a naive OLS regression would understate (in absolute value) the impact of Chinese trade competition, which would likely occur if (as theorized by ADH and FS) confounding increases in US demand for sector-specific goods occurred at the same time as there was rising Chinese import competition. These demand increases would boost both Chinese importers and domestic producers, allaying some of the negative impact of increased competition. The full model with controls and timetrends yields the smallest point estimates, so I will refer from here on out only to the results from (c).

All coefficients are interpreted as the impact of a $1,000 increase in Chinese imports per worker on the housing price appreciation in a commuting zone relative to the base year of 1990. I find here that a $1,000 increase in Chinese imports per worker lowered appreciation relative to the baseline by 9.7 percentage points over a 10 year period. The median home in the U.S. in 1990 was valued at $125,676 (in 2007 dollars),[32] so that for a commuting zone that saw a $1,000 increase in Chinese import penetration per worker per decade would have the median house be worth (on average) $24,381 less.

Using the same strategy as ADH, I then construct the counterfactual housing price appreciation scenario by commuting zone using:

$$\Delta HPI_{it}^{cf} = -\beta_1 \Delta I\bar{P}W_{it} \tag{12}$$

[29] 2007 chosen both to avoid the confounding effects of the Great Recession and the limitations created by the replication files made available by FS and ADH. Further, additional research has shown that the local labor market effects of the China Shock largely plateaued after 2010 (Autor, Dorn, and Hanson, 2021), so 2007 remains a reasonable end point.

[30] These include the initial share of employment in manufacturing, the share of the population that is college-educated, the foreign-born share, the share of women in the workforce, the routine occupation share in employment, and the index for the average offshorability of occupations in commuting zone i.

[31] `https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx#mpo`

[32] Source: NHGIS, Steven Manson, Jonathan Schroeder, David Van Riper, and Steven Ruggles. IPUMS National Historical Geographic Information System: Version 13.0 [Database]. Minneapolis: University of Minnesota. 2018. http://doi.org/10.18128/D050.V13.0

where

$$\Delta I\bar{P}W_{it} = 0.6063 \times \Delta IPW_{it}, \tag{13}$$

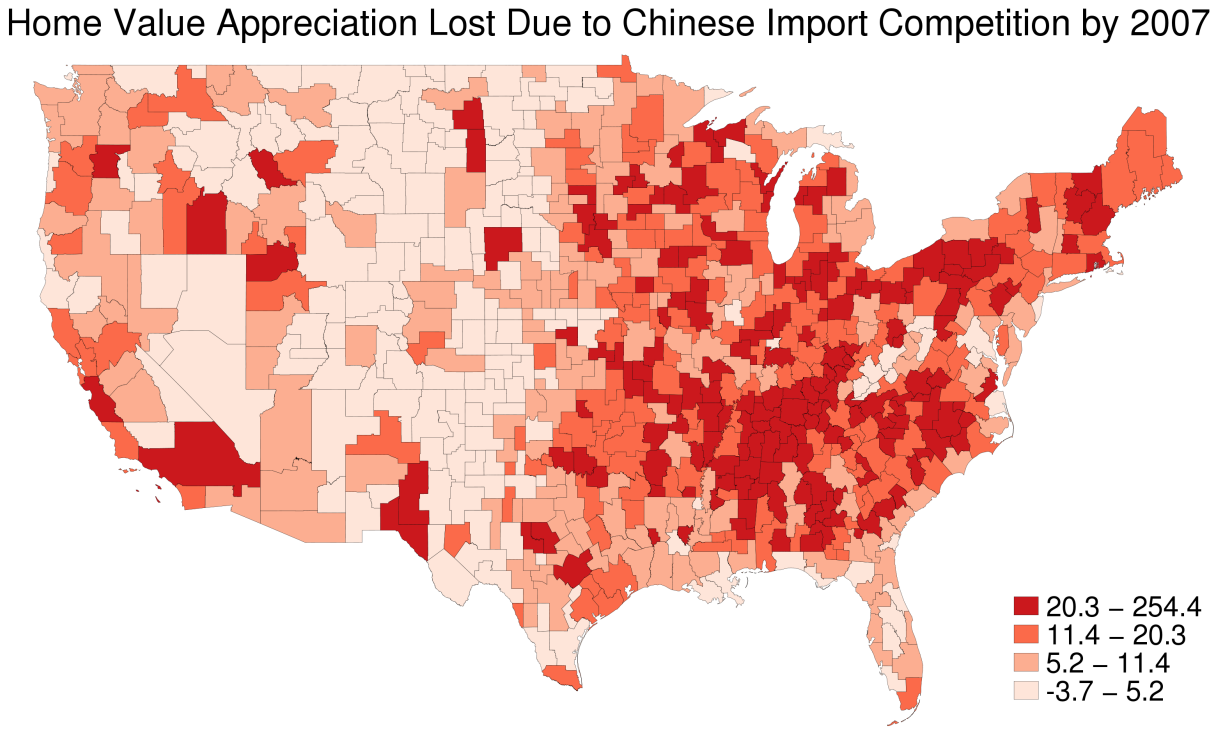
which is the observed change in import competition per worker times the R^2 from the first stage of instrumenting for ΔIPW_{it} on ΔIPW_{oit}. Figure B1 presents the results by quartile of foregone price appreciation, along with the intra-quartile range. Several observations emerge from Figure B1. The first is that with some notable exceptions, more rural, whiter, and areas with older populations suffered greater losses in wealth appreciation. Secondly, the intra-quartile range in the top quartile is much larger than the intra-quartile range in the other three quartiles. However, this is a bit misleading, since the 99th percentile is 78.78, meaning that the right tail in foregone housing price appreciation is quite long.[33] Nonetheless it is clear that homeowners in the top quartile were disproportionately more adversely affected than people in the bottom three quartiles, even when accounting for outliers.

References

Autor, David H., David Dorn, and Gordon H. Hanson, 2021. "On the Persistence of the China Shock," *Brookings Papers on Economic Activity* Fall 2021, 381-447.

[33]Only two commuting zones have foregone price appreciation above 100 percentage points: the first is 25402, which is a one-county commuting zone in southwest Kentucky encompassing Calloway County. The other is the Sioux City, Iowa area, which experienced extraordinarily high import penetration per worker, almost twice as high as the next highest commuting zone. Thus, the very end of the intra-quartile range are dominated by unmistakable outliers.

Figure B1: Average Housing Price Appreciation Foregone Due to the China Shock by Commuting Zone



SOURCE: Author's calculations using the replication package provided by Feler and Senses [2017], accessible here: `http://doi.org/10.3886/E114677V1`.
NOTES: Figure B1 shows areas by quartile of foregone housing price appreciation due to Chinese import competition by 2007, calculated from Equation (12).

TABLE B1
Estimated Lost Housing Price Appreciation
From Chinese Trade Shock in Local Labor Markets ($N = 1,046$)

	OLS			IV		
	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)	(b/se)
Δ Chinese imports per worker	-6.08***	-4.70***	-3.96***	-10.82***	-12.19***	-9.74***
	(1.96)	(1.55)	(1.40)	(3.73)	(2.70)	(2.48)
Controls	N	Y	Y	N	Y	Y
Region-Specific Time Trends	N	N	Y	N	N	Y

SOURCE: Author's calculations using the replication package provided by Feler and Senses [2017], accessible here: `http://doi.org/10.3886/E114677V1`.

NOTES: Table B1 reports the results from estimating the three iterations of the Feler and Senses (2017) approach given in Equation (11). The coefficients report the impact on the FHFA's housing price appreciation index (1990 = 100) of increasing Chinese import penetration per worker by $1,000. Regressions are weighted with each commuting zone's share of the 1990 national population, and standard errors are clustered at the commuting level.

* p<0.10, ** p<0.05, *** p<0.01

TABLE B2
Summary Statistics on the China Shock's Effect On Housing Price Appreciation Sorted by Quartile

	Mean	Minimum	Median	Maximum
Quartile				
1	-74.73	-522.90	-60.98	-41.89
2	-32.16	-41.78	-31.50	-23.52
3	-17.00	-23.46	-16.77	-10.73
4	-4.21	-10.65	-3.52	7.63

SOURCE: Author's calculations using the replication package provided by Feler and Senses [2017], accessible here: `http://doi.org/10.3886/E114677V1`.

NOTES: Table B2 shows the mean, median, minimum, and maximum estimated treatment effect of the China Shock on CZ-level housing price appreciation by quartile. The change in housing price appreciation caused by the China Shock is calculated using Equation (12).