The Journal of Real Estate Finance and Economics

Retirement, Unretirement, and Housing Wealth during the Great Recession

--Manuscript Draft--

Manuscript Number:	REAL-D-18-00192R2
Full Title:	Retirement, Unretirement, and Housing Wealth during the Great Recession
Article Type:	Original Research
Keywords:	Retirement and Unretirement, Property Taxes, Housing Wealth
Corresponding Author:	Greg S. Burge, Ph.D. University of Oklahoma Norman, Oklahoma UNITED STATES
Corresponding Author Secondary Information:	
Corresponding Author's Institution:	University of Oklahoma
Corresponding Author's Secondary Institution:	
First Author:	Greg S. Burge, Ph.D.
First Author Secondary Information:	
Order of Authors:	Greg S. Burge, Ph.D.
	Lingxiao Zhao, Ph.D.
Order of Authors Secondary Information:	
Funding Information:	

Retirement, Unretirement, and Housing Wealth during the Great Recession

Lingxiao Zhao, *Southwestern University of Finance and Economics*

Gregory Burge*, *University of Oklahoma*

Abstract: This paper explores the effect of housing wealth shocks and changes in property tax liabilities on the timing of retirement and unretirement (retirement reversals). Using longitudinal data from the Health and Retirement Study that spans the recent boom/bust cycle in the housing market, we exploit regional variation in the amplitude of housing price movements to identify the causal effect of unexpected housing wealth shocks, mitigating endogeneity concerns. We consistently find workers delay (hasten) retirement when they experience unexpected losses (gains) in housing wealth or have increased (decreased) property tax burdens. Extensions show these factors influence retirement reversals in the expected opposite directions. Importantly, we verify these effects remain even after controlling for early retirement expectations, providing additional evidence that the wealth shocks of the recent housing market cycle were unanticipated. Finally, we explore the nuanced role of gender and marital status in this context.

Keywords: Retirement, unretirement, property taxes, housing wealth.

* Corresponding author. gburge@ou.edu, (405)325-2358, 308 Cate Center, Norman, OK 73019.

We would like to thank Karen Conway, Michael Lovenheim, Jonathan Foreman, Qihong Liu, Byron Lutz, Dan Rickman, Cynthia Rogers, John Winters, Vincent Yao, Keith Ihlanfeldt, and conference participants at the American Economic Association, American Real Estate and Urban Economic Association, Missouri Valley Economic Association, Southern Regional Science Association, National Tax Association, and Oklahoma State Economics Seminar Series for their helpful comments. We also thank the Michigan Center on the Demography of Aging (MiCDA) for the access they provided to the restricted access HRS data. All remaining errors are of course our own.

1. Introduction

The recent boom/bust cycle in the housing market generated unprecedented price volatility that took many homeowners by surprise. Understanding the impact of housing wealth shocks on the retirement decisions of older workers is critical for at least four reasons. First, older workers supply labor far more elastically than their younger counterparts (Hanoch and Honig, 1983). Second, older workers are more likely to be longer term homeowners, and therefore to have accumulated more housing wealth, making them more vulnerable to these shocks (Lusardi and Michell, 2007). Perhaps most importantly, older workers play an increasingly vital role in the labor market and are less likely than before to have pensions. Currently, 1 of every 4 workers in the U.S. is 55 or older, compared to less than 1 of every 8 workers as recently as 2000. Finally, equity related concerns surface, as retirement portfolios for lower and middle income households often contain housing wealth paired with little or no financial wealth (Benjamin, Chinloy, and Jud, 2004b).

We use a 20 year panel of the restricted access version of the Health and Retirement Survey (HRS) to measure the causal effect of housing wealth on the timing of retirement and unretirement (retirement reversals). In doing so, we contribute to a literature that is filled mostly with studies investigating the effects financial wealth on retirement timing and the effects of housing wealth on other outcomes. We also account for the role of property taxes – an important liability for homeowners that co-varied with housing price shocks during the period (Lutz, 2008; Shan, 2010; Zhao and Burge, 2017).

Despite its prominent role in asset portfolios, the influence of housing wealth on retirement transitions has received relatively little attention. Only recently have studies moved towards identifying causal effects by focusing on geographic variation in the amplitude of the boom/bust – an important advancement given concerns that workers may accumulate housing wealth endogenously with respect to retirement plans (e.g., one desires to retire earlier, and therefore strives to accumulate more housing wealth at younger ages). Begley and Chan (2018) and Zhao (2018) both use regional price shocks during the housing bubble to investigate the effect of housing wealth on retirement. Begley and Chan use zip-code level price variation, finding adverse shocks cause men and certain subgroups of women to delay retirement. Zhao develops a structural model that shows workers experiencing gains (losses) in housing wealth will retire earlier (later), and uses variation in house price appreciation across the nine U.S. Census divisions in complementary regression analysis. Our study complements this emerging literature in several clear ways.

To our knowledge, we are the first study focusing on retirement timing that attempts to disentangle the potentially competing effects of housing wealth and property taxes on the dynamic aspects of retirement and unretirement decisions. Regarding this combination, the most closely related work comes from Zhao and Burge (2017), who explore the joint effects of housing wealth and property taxes on current labor supply, focusing on both the intensive and extensive margins. Our paper differs in its focus on longer term outcomes including initial retirement and the potential for retirement reversal (unretirement). Additionally, we complement existing findings with regard to gender, showing that married females are more responsive to changes in housing wealth than their male counterparts. Finally, we offer the first study in the retirement timing literature that employs two alternative measures of housing wealth – one relying on differential trends in housing prices across geographic regions and another built from households' self-reported measures. Using these two distinct measures of housing wealth, and employing multiple estimation strategies – including an instrumental variable robustness check – we consistently find evidence that positive (negative) shocks to housing wealth hastened (delayed) retirement timing, whereas the expected opposite impacts influence unretirement (i.e., retirement reversals).

2. Retirement Timing

A large literature investigates the determinants of retirement timing. Similarly, many studies have taken up questions related to how changes in housing wealth and/or financial wealth influence levels of current consumption (e.g., Benjamin, Chinloy, and Jud, 2004a; Guo and Hardin, 2014; Bhutta and Keys, 2016) or other household decisions including fertility (Lovenheim and Mumford, 2013), college enrollment (Lovenheim and Reynolds, 2013), and transitions into self-employment (Harding and Rosenthal, 2017). Since an exhaustive review of either of these large literatures lies beyond the scope of our paper, we highlight that our estimations control for many retirement determinants that have been previously identified. For example, we separately measure housing wealth from other financial and pension related assets, and all of our retirement timing models include not only housing wealth measures, but also control for: pension eligibility, type of pension plane, other financial wealth, age (which reflects Social Security eligibility thresholds), physical health, race/ethnicity, religion, marital status, parental status, educational attainment, regional economic conditions, and local tax rates. We consistently find these factors to exhibit effects as expected from previous work. For the sake of brevity, our literature review focuses on the relatively few studies that directly consider questions related to retirement timing and household wealth.

An early study on retirement timing and unexpected wealth shocks comes from Sevak (2002), who also uses panel data from the HRS. Forming an aggregated wealth measure that contains stocks, bonds, checking/savings, IRAs and other retirement savings accounts, all along with housing wealth, the study finds a $50,000 wealth shock increases the likelihood of early retirement by 1.9 percentage points. By design, the estimations in this study are linked closely with unexpected growth in the value of financial assets during the 1990s, as inflation adjusted home values were relatively stable over the period. A more recent study by Farnham and Sevak (2016) relates more closely related to our work, as they measure housing wealth separately from other financial wealth. Models identifying the effects of housing wealth based on cross-MSA variation in house prices indicate a 10 percent increase in housing wealth speeds up retirement by roughly four months. While they measure the "housing boom", their last HRS responses come from the early 2000s, meaning the most turbulent periods of the housing bubble were not captured.

Several other recent studies have considered the issue of housing wealth and retirement timing – each with distinct contributions. Begley and Chan (2018) offer the first investigation that, to our knowledge, relates housing wealth to both retirement and unretirement decisions. They find negative house price shocks delay the transition to retirement and increase the likelihood of experiencing a reversal of a previous retirement. Ondrich and Falevich (2016) use panel data from the HRS to reach similar conclusions using estimations that include only married male workers. They find that an average sized decline in housing wealth (measured over the recession period) lowers the probability of early retirement by 15 percent, but that the size of the effect is mitigated by the presence of pensions. Zhao (2018) constructs an incomplete-market life-cycle model with a risky housing asset and endogenous retirement timing. Counterfactual experiments are used to quantify the impact on retirement and non-durable consumption levels. Complementary regressions using the Rand version HRS data use time variation in house prices across the nine U.S. Census Divisions to show that near-retirement homeowners delay retirement by an average of 2.8 months when facing an unexpected 28 percent decline in house prices.

To our knowledge, the only post-housing bubble study in the literature finding no evidence that changes in housing wealth influence the timing of retirement in the UK comes from Disney et. al (2015). On the one hand, this difference is understandable, as institutional and cultural factors may cause effects to vary across contries. On the other, it is surprising, as Disney and Gathergood (2018) use the same UK data in a separate paper that did find significant impacts of housing wealth on labor supply outcomes for older male workers.

Additionally, previous studies of retirement timing and housing wealth rely *either* upon house price variation across geographies *or* on self-reported measures of housing wealth. Instead, we use *both* approaches to examine the same set of households, since each carries distinct advantages and disadvantages over the other. Self-reported values carry desirable precision, but may be subject to an endogeneity bias. Measures focusing on geographic variation in house prices are more plausibly exogenous to previous household decisions, but individual homes may experience different price trends than the regional average. Exploring both measures adds to our confidence that we are documenting a true effect.

Also, we further explore the role of gender in this context. On the whole, we find evidence that married females react more elastically to changes in housing wealth than married males or single females. Finally, and perhaps most importantly, our labor force re-entry models (unretirement) test whether price volatility in the housing market significantly influences labor supply outcomes for households *even after* they previously made an initial retirement decision.

We find consistent evidence that retirement and unretirement decisions are influenced by changes in housing wealth and property taxes, both in the expected opposing directions. We also show how unforeseen changes in retirement timing (defined as deviations between early retirement expectations and later actual retirement behaviors) are influenced by housing wealth shocks and property taxes, but not by changes in financial wealth. This implies that older households do not consider a dollar of financial wealth and a dollar of housing wealth to be identical assets – at least in terms of how they influence retirement relate decisions.

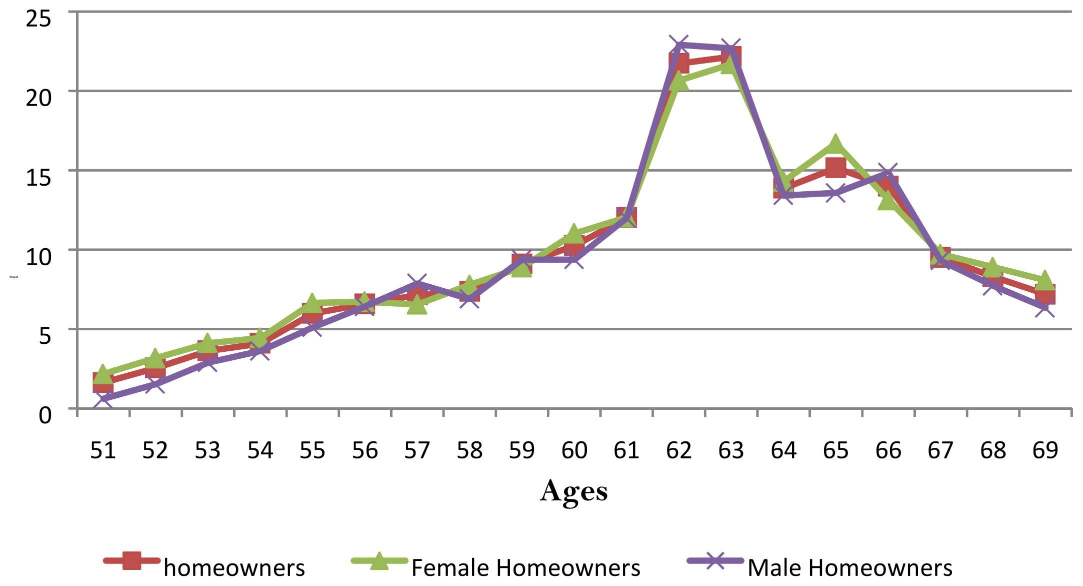
3. Data

We utilize household level data from the restricted access RAND version and pulic use version of the Health and Retirement Study (HRS). To this data, we merge MSA level house price measures from the Federal Housing Finance Authority (FHFA), state tax related burdens from the Tax Foundation, and various employment related outcomes at the MSA level from the Bureau of Labor Statistics (BLS). Defining an observation as an individual respondent at a particular HRS survey wave, we begin with 103,593 distinct observations that come from 33,807 different persons. To model changes in housing wealth over time, we link HRS respondent observations across waves using the unique HRS household identification variable. By construction, the HRS is a nationally representative sample that targets individuals over age 50 and their spouses. The survey reports self-assessed housing values and mortgage liabilities for primary residences, vacation homes, and rental property. In addition, the data contains comprehensive information regarding socio-economic and demographic variables including health status, marital status, parental status, financial wealth, benefits, social security, pensions, and employment history – allowing our later analysis to control for the various factors that have been identified as significant retirement determinants by previous work.

The HRS is a biennial longitudinal survey. We use the ten survey waves spanning 1991 through 2010 for our analysis, containing five cohorts born primarily during the 1920s, 1930s, and 1940s. Using the HRS nomenclature, these include the Assets and Health Dynamics cohort (born in 1924 or earlier), the Children of the Depression cohort (born between 1924 and 1930), the original Health and Retirement survey (OHRS) cohort (born between 1931 and 1941), the War Baby cohort (born between 1942 and 1947), and the Early Baby Boomer cohort (born between 1948 and 1953).

The survey asks detailed employment questions that are consistent across waves, allowing us to construct rich dependent variables regarding retirement and unretirement transitions for households. These include initial retirement, unretirement (i.e. a reversal of a previously reported retirement), and even early expectations over future retirement timing for younger workers. We define retirement based on self-reported work status, including respondents who are fully and partially retired. Figure 1 plots the proportion of homeowners experiencing an initial retirement transition at a given wave (i.e., a two year period) for ages between 51 and 69, using all respondents as the denominator. [Insert Figure 1 about here] While respondents can fall outside of this range, these ages cover the vast majority of our observations. Note that key baseline tendencies, including elevated retirement rates between age 62 and 66, are strikingly similar for female and male respondents.

Figure 1: Proportion of Retirement Transitions



While moving directly from work to retirement, and then staying retired, is clearly the modal path, we do observe significant fractions of the data partially retiring, or even eventually moving to unretirement (i.e., reversing retirement). We construct several variables to reflect these alternative pathways, defining unretirement transitions based on these categories. Table 1 shows the nine possible transitions, along with the percentage of observations in each category. [Insert Table 1 about here] Unsurprisingly, about two-thirds of wave-to-wave transitions reflect complete retirement at both survey waves. Fortunately the data are bulky enough to overcome this initial limitation, as even small fractions of the initial observation count are very large observation counts. Just over twelve percent come from respondents who are partially retired at both the current and previous waves. The three most common retirement related transitions – going from working to partial or complete retirement, going from partial to complete retirement – also account for roughly twelve percent. The remaining wave-to-wave transitions – representing about 7.5% of the data – contain the three "unretirement paths". These are: moving from complete to partial retirement (path 2), moving from complete retirement to working (path 3), and moving from partial retirement to working (path 6). The most severe retirement reversal (i.e., moving from complete retirement into full time) is the smallest of these three pathways. Fortunately, the HRS data is quite large, as 7.5% of the data still reflect 2,526 distinct wave-to-wave observations.

Table 1: Postretirement Paths

	Defined as unretirement	Obs.	Percent
1. Completely retired—> completely retired		21,775	64.41%
2. Completely retired—> partly retired	√	1,257	3.72%
3. Completely retired—> not retired	√	310	0.92%
4. Partly retired—> completely retired		2,237	6.62%
5. Partly retired—> partly retired		4,106	12.15%
6. Partly retired—> not retired	√	959	2.84%
7. Not retired—> completely retired		583	1.72%
8. Not retired—> partly retired		1,087	3.22%
9. Not retired—> not retired		1,493	4.42%
Total		33,807	

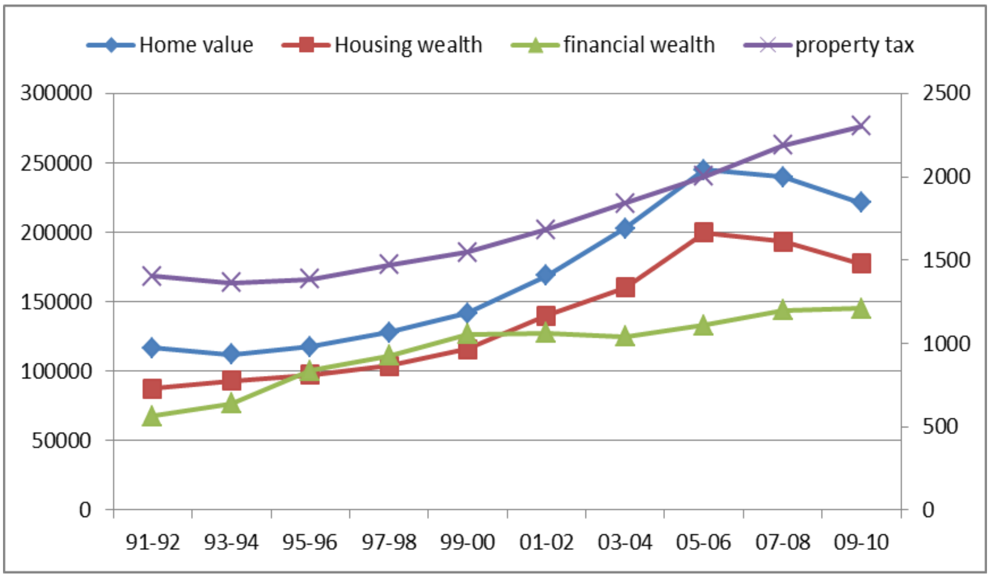
Housing wealth, financial wealth, and property taxes represent our three main independent variables of interest. The HRS asks questions about home ownership, self-assessed home value (for primary residence, rental properties, and vacation homes), mortgage liability (for each property), property tax liabilities (for each property), checking accounts, saving accounts, stocks, and other investments. We use the net value of home equity (total home values less total mortgage liabilities) to measure housing wealth. Only a small fraction of respondents own multiple homes, but importantly they represent a subsample carrying high levels of housing wealth. Figure 2 displays the trends in the average values for these nominal variables over our sample. [Insert Figure 2 about here] Home values, housing wealth, and financial wealth all reference the vertical axis on the left, while property taxes reference the vertical axis on the right. The housing boom and bust both surface in the figure. The trends show comovement up through the housing bust, at which point property taxes continue to rise while housing wealth falls. This is consistent with findings regarding connections between housing prices and property tax levels (Lutz, 2008). An advantage of using household level reported property tax liabilities is that they should reflect the various tax advantages offered by many states to older residents.

Importantly, 'average' changes mask variation across different parts of the U.S. For example, between the first quarter of 2007 and the last quarter of 2010, Miami experienced a 43.3% decline in nominal prices, whereas Atlanta only fell by 17.5%. Houston actually had small nominal price gains (4%). Our identification strategy assumes that, while renters and homeowners differ in countless ways, households did not sort themselves into the homeowner category *in systematically different ways across MSAs based upon future housing price trends.* Put another way, we assume households in Miami in 2005 were not more or less likely to avoid homeownership than similar households living in Houston, based upon the different house price movements that played out over the next several years. Statistically, this just means the probability of being a renter in the pre-bust period is uncorrelated to future housing price trends with the MSA. Similar assumptions are made by several well published papers in this literature (Lovenhein and Mumford, 2013; Lovenheim and Reynolds, 2013; Zhao and

Burge, 2017; and Harding and Rosenthal, 2017) and one that we feel comfortable with given the unexpected nature of the housing crisis.

Also note that we see property tax liabilities continue to rise over the duration of the recession – a finding that is now common in the literature. As discussed earlier, we face a potential validity issue associated with using the self-reported housing wealth. Since home values and housing wealth both reflect perceived price variations, as opposed to actual market conditions, we supplement the HRS data with MSA-specific home value indexes with the household survey data.

Figure 2: Nominal Assets and Property Taxes, 1991-2010.



By definition, these two alternative measures of housing wealth each carry certain advantages and disadvantages over the other. The MSA level HPIs are exogenous to households' individual decisions/planning, but they also show only blunted variation in housing wealth. That is, they fail to fully capture changes in housing wealth due to changing mortgage indebtedness, as well as heterogeneity in home prices at the neighborhood level. On the other hand, the self-reported measures capture richer and potentially more accurate variation in housing wealth, but they may be potentially endogenous to household level decisions like entering/exiting a marriage or sending a child to college. Our use of both measures stems from a desire to ensure our results are not artificially driven by either shortcoming.

Table 2 lists all our variables and their sources. [Insert Table 2 about here] Table 3 provides summary statistics for the 103,593 observations analyzed in our paper, along with subsample statistics for female and male respondent homeowners. [Insert Table 3 about here] This initial observation count is larger than the number of observations eventually included in any given later estimations due either to our choice of estimation method (e.g., the hazard model automatically drops observations occurring after the event of interest) or to purposeful decisions to focus on specific pathways (e.g., to analyze the determinants of unretirement decisions, we can only use observations from households who have previously reported retirement).

The initial 103,593 observations are those that remain after having already applyed four intuitive filters to trim original data. Specifically, we drop:

- Individuals younger than 44. HRS respondents must be 50 or older, but can have younger partners. Respondents younger than 44 are not representative of their cohorts. This filter trims the sample by less than 1 percent.
- Extremely wealthy households and households with very high debt are trimmed. We filter households reporting more than $1,000,000 in housing wealth or $2,000,000 in financial wealth, again accounting for less than one percent of the sample. Since bankruptcy and/or foreclosure are options for

those deeply in debt, we drop cases below -$50,000 for housing wealth or for financial wealth, again accounting for less than 1 percent of the sample. We acknowledge our results may fail to characterize these small groups.

- Respondents that fail to report any financial or housing assets. In these cases, we expect the true values are not consistently zeros, but instead that respondents skipped this HRS section. While this causes a 15% decline in our sample, it is consistent with the choices made by previous studies using the HRS to investigate wealth effects.
- Observations where property taxes are reported to be more than ten percent of home value. These are likely reporting errors, as no state actually levies property tax rates exceeding 4%-5%. This trims very few cases.

Table 2: Description of Variables.

Variable	Description	Data source
Retirement-related:		
Retired	Dummy equals one if the respondent is currently retired (either completely or partly).	RAND HRS
Postretirement type	Categorical variable that equals one if being completely retired, one if partly retired, and three if not retired.	RAND HRS
Unretired	Dummy equals one if the respondent does an unretirement transition, which include three possibilities: 1) completely retired to partly retired; 2) completely retired to not retired; 3) partly retired to not retired.	RAND HRS
Regarding the work status at age 62		
Actual work status	Dummy equals one if the respondent is currently working at age 62.	RAND HRS
Updating expectations	Updating self-reported probability of working full-time after age 62.	RAND HRS
Retirement Expectations	Probability of working full-time after age 62 reported one wave ago.	RAND HRS
Wealth-related:		
Home assets	The total value of the primary residence.	RAND HRS
Housing wealth	The value of the primary residence less mortgages and home loan.	RAND HRS
Property tax	Self-reported property tax liabilities paid last year.	RAND HRS
Financial assets	Sum of stocks, mutual funds, investment trusts, checking, savings, money market accounts, government saving bonds, other bonds and all other savings.	RAND HRS
Financial wealth	Net value of non-housing financial wealth, calculated by subtracting non-mortgage debts from the sum of stocks, mutual funds, investment trusts, checking, savings, money market accounts, government saving bonds, other bonds and all other savings.	RAND HRS
Demographics:		
Cohort	Five cohort dummies: HRS, AHEAD, CODA, WB and EBB.	RAND HRS
Age	Age in years.	RAND HRS
Squared age	Squared value of age.	
Health	Categorical variable that equals five if self-report health is poor, four if fair, three if good, four if very good, and five if excellent.	RAND HRS
Female	Dummy equals one if the respondent is female.	RAND HRS
Number of children	Number of children within the household.	RAND HRS
Married	Dummy equals one if the respondent is married.	RAND HRS
Race	Four race dummies of white, black, Hispanic and other racial group.	RAND HRS
Education years	Number of years that the respondent spent in school.	RAND HRS
Education degree	Four education degree dummies of no degree, high school, college and above, and other degree.	RAND HRS
Location & wave:		
Wave	Ten wave dummies 1991 through 2010.	RAND HRS
Housing price index	MSA specific housing price index	Federal Housing Finance Agency
Local tax burden rate	State specific local tax burden rate.	Tax Foundation
Unemployment rate	MSA specific unemployment rate aggregated from counties.	Bureau of Labor Statistics

Table 3: Summary Statistics of Observations in the Analysis.

	Homeowners			Female Homeowners			Male Homeowners		
	Obs.	Mean	Std.	Obs.	Mean	Std.	Obs.	Mean	Std.
Retirement-related:									
Retired	82,356	0.5867	0.4924	43,254	0.5506	0.4974	39,102	0.6267	0.4837
Postretirement type	48,951	1.3630	0.6283	24,412	1.3717	0.6535	24,539	1.3524	0.6021
Unretired	33,807	0.0748	0.2629	16,280	0.0746	0.2628	17,527	0.0748	0.2631
Regarding the work status at age 62									
Actual work status	6,704	0.3371	0.4728	3,632	0.3283	0.4697	3,073	0.3475	0.4763
Updating expectations (in%)	30,715	46.08	38.82	17,473	41.63	37.99	13,242	51.96	39.13
Expectations one wave ago (in%)	26,997	45.62	38.80	15,378	41.43	37.95	11,619	51.16	39.22
Expectations two waves ago (in%)	22,859	45.09	38.76	12,976	40.76	37.86	9,883	50.78	39.20
Expectations three waves ago (in%)	18,945	45.62	38.96	10,694	41.36	38.03	8,251	51.14	39.45
Wealth-related:									
Home assets	103,593	167,619	156,257	57,216	163,918	154,152	46,377	172,185.7	158,699
Housing wealth	103,593	135,872	135,139	57,216	133,799	133,830	46,377	138,114.8	137,045
∆ log(Housing wealth)	83,045	0.2992	2.1349	46,100	0.3057	2.1411	36,945	0.2911	2.1271
Property tax	103.593	1,700.8	1,810.1	57,216	1,659.99	188.15	46,377	1,751.102	1,821
∆ log(property tax)	75,781	0.1734	1.4507	41.869	0.1753	1.4584	33,912	0.1709	1.4411
Financial assets	103,593	115,769	221,919	57,216	111,308.9	216,990	46,377	121,271.4	227,735
∆ log(Financial assets)	78,863	0.0847	1.6635	43,663	0.0808	1.6966	35,200	0.0897	1.6216
Demographics:									
Age (in years)	103,593	65.827	10.15	57,216	65.29	10.51	46,377	66.49	9.66
Health (in a 5-point scale)	103,593	3.3491	1.0909	57,216	3.3775	1.0821	46,377	3.3139	1.1008
Female	103,593	0.5523	0.4973						
Number of children	103,593	3.1075	1.9826	57,216	3.0829	1.9780	46,377	3.1378	1.9877
Married	103,593	0.7750	0.4176	57,216	0.6940	0.4608	46,377	0.8750	0.3307
Race dummies									
White	103,593	0.8533	0.3538	57,216	0.8475	0.3595	46,377	0.8606	0.3464
Black	103,593	0.0811	0.2730	57,216	0.0863	0.2807	46,377	0.0747	0.2629
Hispanic	103,593	0.0485	0.2148	57,216	0.0493	0.2164	46,377	0.0475	0.2127
Other race	103,593	0.0171	0.1296	57,216	0.0170	0.1292	46,377	0.0172	0.1300
Education (in years)	103,315	12.7792	2.8276	57,100	12.7161	2.6185	46,215	12.8572	3.0646
Education degree dummies									
No degree	103,593	0.1771	0.3818	57,216	0.1666	0.3726	46,377	0.1901	0.3924
High school	103,593	0.6039	0.4891	57,216	0.6500	0.4770	46,377	0.5471	0.4978
College & above	103,593	0.2179	0.4128	57,216	0.1829	0.3866	46,377	0.2609	0.4392
Other degree	103,593	0.0011	0.0302	57,216	0.0005	0.0229	46,377	0.0018	0.0425
Year	103,593	2000.86	5.5349	57,216	2000.9	5.5217	46,377	2000.76	5.5496
MSA level housing price index growth (in%)	98,267	3.7997	6.5218	54,308	3.7910	6.5659	43,959	3.8104	6.4670
MSA level unemployment rate (in%)	103,342	5.8720	2.3756	57,082	5.8693	2.3733	46,260	5.8753	2.3784
State level local tax burden (in%)	103,354	9.5789	1.1747	57,089	9.5737	1.1714	46,265	9.5853	1.1787

4. Retirement Decisions

Following the strategy of previous work in the retirement literature, we estimate a series of hazard models that examine the effects of housing wealth, property taxes, financial wealth, and a host of other control variables (e.g., gender, race, education, marital and parental status, and health) on our HRS respondents' retirement transitions. Recall that our expectation is that during the housing boom, greater housing wealth accumulated through rising prices should hasten retirement, whereas the heavier burden of rising property taxes should delay retirement.

In these models, *survival* occurs if a respondent continues to work. The hazard model assumes the event of interest (retirement) only occurs once. Since they have no reported pre-retirement measures, we exclude individuals who were already retired at their initial appearance in the HRS survey, accounting for about 17% of the sample. We specify an individual's transition to retirement with a discrete time Cox proportional hazard model (Cox, 1972). While the status of being retired includes partially retired and completely retired, robustness checks show very similar findings using only completely retired. The retirement hazard function, $\theta(t|X_i)$, gives the probability respondent *i* retires in period *t*, conditional on not having already retired in a previous wave:

$$(1)\ \theta(t|X_i) = \frac{f(t|X_i)}{1 - F(t|X_i)}$$

We specify a proportional hazards model of retirement as

$$(2)\ \theta(t|X_i) = \theta_0 \exp\{X_i{'}\beta\}$$

in which age in years, *t* is the relevant duration. $\theta_0(t)$ is the baseline hazard function common to all individuals at time *t*, and is estimated non-parametrically. The baseline hazard function cancels out once a proportion is formed by separate hazards in the same time period. Hence, we have:

$$(3)\ \frac{\theta(t|X_i)}{\theta(t|X_j)} = \frac{e^{(X_i{}'\beta)}}{e^{(X_j{}'\beta)}} = e^{(X_i{}'-X_j{}')\beta}$$ where

$$(4)\ \beta X = \beta_0 + \beta_1 \text{housing wealth}_{it} + \beta_2 \text{property taxes}_{it} + \beta_3 \text{financial wealth}_{it} + \beta_4 \text{health}_{it} + \beta_5 \text{demographics}_{it} + \beta_6 \text{unemployment rate}_{mt} + \beta_7 \text{local tax burden}_{st} + \beta_8 \text{wave}_t + \varepsilon_{it}$$

The right hand side variables include logged values of self-reported housing wealth, property taxes, financial wealth, a categorical health status indicator, the local unemployment rate, and local tax burden, demographic characteristics including gender, race, education, and marital and parental status, and we include survey year (wave) fixed effects.

One concern in estimating the hazard model using self-reported housing wealth is the potential endogeneity issue. This occurs since households choose their housing consumption and mortgage indebtedness levels, potentially accounting for early retirement related goals as they make those choices. To mitigate this concern, we use aggregated MSA housing price changes instead of respondent specific wealth changes, to capture plausibly quasi-experimental variation in housing wealth. This approach has been used successfully by Lovenheim and Mumford (2013), Lovenheim and Reynolds (2013), Zhao and Burge (2017) and others. Thus, after accounting for this concern, we have:

$$(5)\ \beta X = \beta_0 + \beta_1 \mathit{hpi\_growth}_{mt} + \beta_2 \text{property taxes}_{it} + \beta_3 \text{financial wealth}_{it} + \beta_4 \text{health}_{it} + \beta_5 \text{demographics}_{it} + \beta_6 \text{unemployment rate}_{mt} + \beta_7 \text{local tax burden}_{st} + \beta_4 \text{wave}_t + \varepsilon_{it}$$

Contrary to the assumptions of the hazard model, retirement is not irreversible. In our sample, six percent of observed retirements turn into later reversals. However, none of our main results change if we drop these cases. Another concern relates to pension eligibility. Perhaps pension eligible workers – representing over 60% of the HRS sample – simply work until eligible and then retire (i.e., other factors play no role). We verified this was not the case, as our results are always robust to dropping any individually relevant type of pension plan from the sample (e.g., defined benefit or defined contribution). Furthermove, since moving respresents a shock to housing wealth – and could independently influence retirement decisions – we run models dropping movers from the sample, finding again that our main results carry over.

Table 4a and 4b present the results from estimation equation (5) above. [Insert Table 4a and 4b about here] Table 4a highlights the role of gender, whereas Table 4b explores the differences between married and unmarried respondents. Note that in a hazard model, significant negative coeficients (i.e., housing wealth, financial wealth, higher local unemployment rates) represent factors that *hasten* the outcome of interest, whereas significant positive coeficients (i.e., better health or having a college degree) cause the event to occur *later*. We see consistent evidence that individuals retire earlier if they experience gains in housing wealth. In these estimations, the effect remains robust across both measures of housing wealth, across both genders, and across both possible marital status outcomes.

Interestingly, an additional dollar of housing wealth seems to carry a smaller impact than an additional dollar of financial wealth, as the estimated coeficients are typically different based on an F-test. A counter-example comes from the estimation restricted to using only unmarried respondents. Given that housing wealth is often held jointly between spouces, whereas financial wealth may not be, we consider this to be a reasonable outcome. Conversely, retirement is delayed when property taxes go up (an outcome that is directly linked to rising home values). Presumably the many models that estimate the impact of housing wealth *without* also accounting for the impact of property taxes end up in a position where the joint/total effect of the two distinct changes is lumped together into a single coeficient.

Consistent with prior studies, we see that health and marital status are two of the strongest predictors of retirement timing decisions. Workers retire earlier if their health detoriates and later if they are married. Regarding potential interactions between marital status and retirment timing, Table 4b suggests that gains (losses) in housing wealth hasten (delay) initial retirement for both married and non-married workers. The results also suggest how local unemployment and tax burden would affect older homeowners' retirement behaviors. Homeowners are predicted to experience earlier retirement with higher unemployment rates and higer local tax burdens.[1] Although demographic variables are not a main focus of our study, we do find that black, Hispanic, and asian workers all retire at slightly older average ages than white workers on average. Also, having children carries the same effect. More highly educated respondents also retire later – a result most likely driven by wage

[1] Since variation in local tax burdens reflects higher income and/or sales taxes, its effect likely stems from the reduction in purchasing power associated with lower after-tax earnings.

effects. [In models where we replace education with wages, we see similar results for our variables of interest and that higher wages delay retirement.] Finally, after controlling for other factors, females retire slightly earlier than males. Since it is not possible to include age dummies in hazard models as we have for our other estimations, we provided Appendix A, which presents the results on the main housing wealth variable of interest for different sub-groups by age.

Since the gender variable was significant, and since much of the work in the literature on labor supply explores the role of gender, Table 4a considers the possibility that males and females may be subject to different effects. Overall, reactions across genders to wealth shocks retain the same direction and have similar magnitudes, but we do see some evidence that gender matters. Measuring housing wealth with self-reported data, we find the effect to be similar for both genders. However, when using MSA level house price changes, the negative coefficient doubles for females, but shrinks and becomes insignificant for males. Recalling that the regional HPI measure was the more plausibly exogenous measure of housing wealth, we place a higher degree of confidence in the results suggesting females are more strongly influenced by housing wealth, but the matter is open to some debate given the inconsistency. The effects of changes in property taxes and financial wealth, other key variables of interest, are also roughtly similar across both genders. In other robustness checks that are not reported, we use an alternative (narrower) measure of retirement that excludes partial retirement from the retirement variable designation. The key results all remain largely similar in those estimations.

Table 4a: Retirement hazard model across genders.

Variable	All		Females		Males	
	(1)	(2)	(3)	(4)	(5)	(6)
Housing wealth	-0.03288***		-0.03383***		-0.03009***	
	(0.0043)		(0.0060)		(0.0063)	
Hpi_growth		-0.00382***		-0.00583***		-0.00156
		(0.0011)		(0.0016)		(0.0017)
Mortgage		0.04546***		0.04800***		0.04336***
		(0.0013)		(0.0017)		(0.0018)
Property tax	0.03505***	0.01580***	0.03753***	0.01984***	0.02903***	0.00808
	(0.0036)	(0.0035)	(0.0048)	(0.0046)	(0.0055)	(0.0052)
Financial wealth	-0.06026***	-0.04564***	-0.05459***	-0.04117***	-0.07187***	-0.05542***
	(0.0029)	(0.0029)	(0.0038)	(0.0038)	(0.0043)	(0.0043)
Health	0.12693***	0.11485***	0.12174***	0.11472***	0.13376***	0.11799***
	(0.0053)	(0.0053)	(0.0075)	(0.0075)	(0.0076)	(0.0077)
Unemployment rate	-0.01690***	-0.01886***	-0.01932***	-0.02232***	-0.01430***	-0.01518***
	(0.0033)	(0.0035)	(0.0046)	(0.0048)	(0.0049)	(0.0051)
Local tax burden	-0.01218**	-0.01498***	-0.02229***	-0.02615***	-0.00413	-0.00599
	(0.0051)	(0.0051)	(0.0070)	(0.0071)	(0.0073)	(0.0075)
N of children	0.02205***	0.01556***	0.02809***	0.02208***	0.01564***	0.00830**
	(0.0027)	(0.0028)	(0.0038)	(0.0038)	(0.0040)	(0.0040)
Married	0.60207***	0.57326***	0.67529***	0.64286***	0.42242***	0.39831***
	(0.0137)	(0.0138)	(0.0170)	(0.0171)	(0.0228)	(0.0229)
Female	-0.03109***	-0.01406				
	(0.0118)	(0.0119)				
Hispanic	0.12021***	0.08629***	0.07972*	0.06021	0.16912***	0.12198***
	(0.0296)	(0.0297)	(0.0414)	(0.0417)	(0.0424)	(0.0426)
Black	0.28158***	0.25054***	0.32573***	0.30968***	0.21122***	0.16163***
	(0.0213)	(0.0214)	(0.0281)	(0.0282)	(0.0328)	(0.0329)
Other race	0.19626***	0.13043***	0.36078***	0.31350***	0.03071	-0.04225
	0.0471	0.0474	(0.0655)	(0.0659)	(0.0679)	(0.0682)
Education dummies	controlled	controlled	controlled	controlled	controlled	controlled
Age group dummies	not possible	not possible	not possible	not possible	not possible	not possible
Wave dummies	controlled	controlled	controlled	controlled	controlled	controlled
N of obs	85,060	80,758	48,968	46,480	36,092	34,278

*Significance at the 10% level. **Significance at the 5% level. ***Significance at the 1% level.

Note: Housing wealth, property taxes, and financial assets are naturally logged.

Table 4b: Retirement hazard model across marital statuses.

Variable	Married		Non-married	
	(1)	(2)	(3)	(4)
Housing wealth	-0.02214***		-0.05323***	
	(0.0054)		(0.0073)	
Hpi_growth		-0.00331**		-0.00553***
		(0.0013)		(0.0022)
Mortgage		0.04296***		0.05508***
		(0.0014)		(0.0026)
Property tax	0.02823***	0.00965**	0.04384***	0.02334***
	(0.0045)	(0.0043)	(0.0060)	(0.0058)
Financial wealth	-0.06639***	-0.04936***	-0.04998***	-0.04027***
	(0.0034)	(0.0035)	(0.0051)	(0.0051)
Health	0.13741***	0.12611***	0.10156***	0.09234***
	(0.0063)	(0.0063)	(0.0102)	(0.0102)
Unemployment rate	-0.01741***	-0.01955***	-0.01693**	-0.01771**
	(0.0038)	(0.0040)	(0.0067)	(0.0069)
Local tax burden	-0.01193**	-0.01331**	-0.01560	-0.01986**
	(0.0059)	(0.0060)	(0.0098)	(0.0098)
N of children	0.02551***	0.01821***	0.01131**	0.00702
	(0.0032)	(0.0032)	(0.0054)	(0.0054)
Female	0.02873**	0.04489***	-0.24205	-0.22665***
	(0.0133)	(0.0134)	(0.0243)	(0.0243)
Hispanic	0.11221***	0.07426**	0.11210*	0.10418*
	(0.0339)	(0.0341)	(0.0611)	(0.0610)
Black	0.22456***	0.18964***	0.38297***	0.35871***
	(0.0274)	(0.0276)	(0.0343)	(0.0344)
Other race	0.11113***	0.02292	0.37759***	0.37247***
	0.0585	(0.0590)	(0.0796)	(0.07953)
Education dummies	controlled	controlled	controlled	controlled
Age group dummies	controlled	controlled	controlled	controlled
Wave dummies	controlled	controlled	controlled	controlled
N of obs	65,335	19,725	48,968	18,894

*Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Note: Housing wealth, property taxes, and financial assets are naturally logged.

We also use a difference-in-differences approach to explore this relationship, essentially comparing retirement outcomes of otherwise similar homeowners and renters that live in the same city, across these periods of dramatic positive and negative swings in home prices. This quasi-experimental approach also utilizes the FHFA regional housing price index and relies on the identifying assumption that households did not systematically sort themselves into the homeowner group (i.e., a treatment group that is given a housing wealth shock) and renter group (i.e., a control that does not receive a housing wealth shock) based significantly upon their (correct) expectations prior to the housing market boom/bust about how severely their particular city/region was about to be hit by the housing market cycle. If households 'saw it coming' in some areas compared to others, and those expectations correlated with their early plans over working decisions and future retirement timing, then we would have a threat to our identification strategy in these models. As mentioned above, this assumption has been used a number of times successfully in previous research and we feel very comfortable making it.

Table 5 displays the estimated coefficients for the interaction term of interest, as well as the other variables we control for. Before moving on, we note a potential confusion that may stem from discussing the hazard model followed by the more traditional did-in-dif OLS estimation. Moving between these two approaches, the sign of each determinant should 'flip'. Something that sped up retirement timing in the hazard model should here make retirement more likely to occur at any given observation, controlling for age and other factors as the estimations all do.

From the interaction term of interest we see that the gap between renters and homeowners was largest in the MSAs that experienced the largest and most violent swings in home prices – exactly as one would expect if this was actually a causal effect and not simple a spurious artifact of otherwise meaningful baseline differences between renters and homeowners. Areas experiencing larger gains (losses) in housing wealth saw homeowners retire more (less) frequently than areas experiencing less pronounced swings. Importantly, the loss in significance of this coefficient of interest when moving to the male subsample seems almost entirely due to a loss of statistical power, rather than an actual removal of the effect. Note the magnitude of the effect remains essentially the same, but standard error increases. As always, we would consistently note that although we have uncovered a strong case for having identified an effect of interest, housing wealth is by no means a primary/first-order type influence on retirement decisions. Still, the fact that these effects are small, and that they take a predictable back seat to factors like health, marital status, age, race/ethnicity, and levels of education and/or wages, is not something that invalidates the importance of our work. On the contrary, uncovering these causal effects requires careful attention and large data undertakings, making the findings all the more important for scholars and policy makers to be aware of.

Many other findings from Table 4a and 4b are also seen in Table 5, suggesting the two different empirical approaches produce largely the same set of conclusions. Intuatively, once we control for status as a homeowner and the magnitude of house price appreciation, those with larger (smaller) mortgages are less (more) likely to retitre. Additionally, while we are excited about the importance of these results, the previously provided literature review suggests our investigation of 'unretirement' (i.e., retirement reversals) represent the most novel elements of our study.

Table 5 : Difference-in-difference estimation of housing price index growth effect on retirement.

Variable	All	Females	Males	Married	Non-married
	(1)	(2)	(3)	(4)	(5)
Hpi_growth	-0.00181***	-0.00186***	-0.00160**	-0.00202***	-0.00163***
	(0.0004)	(0.0005)	(0.0007)	(0.0007)	(0.0006)
Homeowner	0.06847***	0.04068***	0.10838***	0.11885***	0.01208
	(0.0067)	(0.0090)	(0.0102)	(0.0090)	(0.0107)
Hpi_growth*homeowner	0.00116***	0.00108**	0.00101	0.00141**	0.00077
	(0.0004)	(0.0005)	(0.0007)	(0.0007)	(0.0006)
Mortgage	-0.00684***	-0.00690***	-0.00687	-0.00675***	-0.00703***
	(0.0003)	(0.0004)	(0.0004)	(0.0003)	(0.0005)
Property tax	-0.00564***	-0.00366***	-0.00797***	-0.00919***	0.00069
	(0.0009)	(0.0012)	(0.0013)	(0.0010)	(0.0015)
Financial wealth	0.00631***	0.00754***	0.00410***	0.00878***	0.00164*
	(0.0006)	(0.0008)	(0.0009)	(0.0007)	(0.0009)
Health	-0.04666***	-0.04687***	-0.04616***	-0.04311***	-0.05489***
	(0.0011)	(0.0016)	(0.0017)	(0.0014)	(0.0020)
Unemployment rate	0.00176**	0.00196**	0.00134	0.00300***	-0.00183
	(0.0007)	(0.0010)	(0.0010)	(0.0008)	(0.0013)
Local tax burden	0.00046	-0.00348**	0.00496***	0.00012	0.00316*
	(0.0010)	(0.0014)	(0.0015)	(0.0013)	(0.0018)
Female	-0.00088			0.00814***	-0.02960***
	(0.0024)			(0.0029)	(0.00461)
Number of children	-0.00074	-0.00069	-0.00038	-0.00322***	-0.00322***
	(0.0006)	(0.0008)	(0.0009)	(0.0010)	(0.0010)
Married	0.02003***	0.03610***	-0.00759		
	(0.0029)	(0.0038)	(0.0048)		
Hispanic	-0.05465***	-0.05813***	-0.05054***	-0.03085***	-0.03085***
	(0.0054)	(0.0075)	(0.0080)	(0.0098)	(0.0098)
Black	0.00775*	0.00454	0.01315**	0.00985*	0.00985*
	(0.0040)	(0.0053)	(0.0063)	(0.0060)	(0.0060)
Otherrace	-0.04292***	-0.04284***	-0.04152***	-0.00565	-0.00565
	(0.0082)	(0.0113)	(0.0121)	(0.0141)	(0.0141)
Education dummies	controlled	controlled	controlled	controlled	controlled
Age group dummies	controlled	controlled	controlled	controlled	controlled
Wave dummies	controlled	controlled	controlled	controlled	controlled
N	95,656	51,776	43,880	67,892	27,764

*Significance at the 10% level. **Significance at the 5% level. ***Significance at the 1% level.

Note: Property taxes, and financial assets are naturally logged.

5. Unretirement Decisions

A. Multinomial Logit Estimation to the Transition of Unretirement

Here we focus on unretirement, investigating whether or not it is influenced by housing wealth and property taxes. Roughly 2,000 individuals (six percent of the sample) reversed an initial retirement decision. Two possibilities may explain this phenomenon. First, if the decision to reverse ones retirement is planned, that means an individual viewed initial retirement as one stage in a multistage process, from an earlier point in time. In this case, events occurring during the postretirement period should not correlate with the likelihood of retirement reversals. However, if at least some retirement reversals are not planned, then retirement reversals may in fact be influenced by unexpected shocks occurring after the initial retirement occurs.

Drawing upon Maestas (2010) we use a multinomial logit specification. This carries the typical advantages associated with predicting categorical probabilities without assuming normality or linearity, but also sheds the timing dynamic of the retirement models (e.g., where hazard models allow variables to hasten/delay outcomes, rather than make them more/less likely). Beyond ones potential inability to perfectly foresee future levels of health or local economic conditions, it may also be difficult to fully predict the evolution of one's future housing wealth, future property tax burdens, and future financial wealth. The last two decades witnessed a volatile housing market and frequent macroeconomic fluctuations, causing a tremendous amount of uncertainty over asset values. As such, we follow prior work on the topic and assume unretirement outcomes are primarily unanticipated, such that postretirement changes in respondents' health, wealth, and local economic conditions may influence the likelihood of making this transition.

The multinomial logit model used in our analysis describes as

$$(6)\ P(y_{i,r+t} = k \mid X_{i,r+t}) = \frac{\exp^{X_i{'}_{r+t}\beta_{k,r+t}}}{\sum_{j=1}^{k} \exp^{X_i{'}_{r+t}\cdot\beta_{k,r+t}}}$$

where *r* denotes the individual i's retirement date, and *r+t* denote the survey wave after retirement. The multinomial logit model over the choices after the initial retirement decision is defined by complete retirement, partial retirement and unretirement. In our data, *k* ranges from 1 to 3. The benchmark specification is:

$$(7)\ \beta X = \beta_0 + \beta_1 \text{housing wealth}_{it} + \beta_2 \text{property taxes}_{it} + \beta_3 \text{financial wealth}_{it} +$$

$$\beta_4 \text{health}_{it} + \beta_5 \text{demographics}_{it} + \beta_6 \text{unemployment rate}_{mt} + \beta_7 \text{local tax burden}_{st} + \beta_4 \text{wave}_t + \varepsilon_{it}$$

The results from estimating equation (7) using self-reported housing wealth are reported in Table 6a and 6b. [Insert Table 6a and 6b about here] The probabilities of choosing partial retirement, or of working full time (not retired), are each measured relative to the baseline full retirement probability. We note that if retirement reversals are unplanned, and potentially associated with adverse wealth shocks, then greater retirement resources should have a buffering effect, thus reducing the probability of unretirement. Table 6a provides the results when including both genders, as well as separated for women and men. The estimated coefficients for housing wealth are -0.026 and -0.054 for partial retirement and unretirement, respectively, suggesting higher housing wealth is associated with lower likelihood of experiencing both outcomes relative to staying retired. Additionally, the negative effect appears stronger on unretirement than that on partial retirement. We also find a significant negative effect of financial assets, with coefficient estimates of -0.056 and -0.10 on partial retirement and unretirement, implying a somewhat stronger effect than the effect of housing wealth. Touching briefly on the other explanatory variables, we find evidence that:

- Better health increases the likelihood of reversing their previous retirement.
- Facing higher local tax burdens increases the odds of reversing retirement.
- Poor employment conditions increase the odds of reversing retirement.
- Aging brings decreasing likelihoods of transiting from retired to unretired.
- Married individuals are less likely to transition to being unretired, whereas having children correlates positively to retirement reversals.
- Black workers are more likely to transit from full retirement to partial retirement or being unretired than white workers. Hispanic workers carry increased likelihoods of being partly retired.
- More highly educated individuals are more likely to become only partially retired, but education does not to influence the chances of full unretirement.

We also report the results for models run separately for females and males. The coefficients for housing wealth, financial wealth, and property taxes do not seem to be influenced by the gender of the respondent in the context of the multinomial logit framework. However, when shifting to the plausibly more exogenous housing wealth shocks reflected by the MSA level HPI changes and the renter-vs.-homeowner comparison, evidence surfaces in Table 7 that reactions from married workers are much stronger than for non-married workers. [Insert Table 7 about here] As was the case in the multinomial logit modles, unretirement is the outcome of interest, and the interaction term HPI_growth*homeowner pins identification of the effect essentially on the differential amplitudes of the housing price shocks across different MSA. In those estimations, the interaction term of interest suggests that when married persons experience positive (negative) housing wealth shocks, they become less likely (more likely) to reverse their retirement.

Table 6a: Multinomial logit model of Unretirement across genders.

Variable	Homeowners		Females		Males	
	partly retired	not retired	partly retired	not retired	partly retired	not retired
	(1)	(2)	(3)	(4)	(5)	(6)
Housing wealth	-0.02567***	-0.05392***	-0.02599*	-0.05259***	-0.02755**	-0.05540***
	(0.0091)	(0.0118)	(0.0134)	(0.0161)	(0.0124)	(0.0175)
Property tax	0.05772***	0.07237***	0.05004***	0.05402***	0.06532***	0.09586***
	(0.0086)	(0.0131)	(0.0124)	(0.0170)	(0.0120)	(0.0206)
Financial wealth	-0.05607***	-0.10066***	-0.06573***	-0.10095***	-0.04407***	-0.10025***
	(0.0063)	(0.0090)	(0.0090)	(0.0118)	(0.0087)	(0.0139)
Health	0.31185***	0.38189***	0.30960***	0.34906***	0.31897***	0.42265***
	(0.0120)	(0.0177)	(0.0180)	(0.0236)	(0.0162)	(0.0271)
Unemployment rate	-0.02133***	-0.01684	-0.03381***	-0.03292**	-0.01113	-0.00014
	(0.0072)	(0.0106)	(0.0110)	(0.0146)	(0.0095)	(0.0155)
Local tax burden	-0.06397***	-0.07553***	-0.01045	-0.01503	-0.10692***	-0.14602***
	(0.0111)	(0.0165)	(0.0164)	(0.0219)	(0.0151)	(0.0252)
N of children	0.02623***	0.03561***	0.03544***	0.02884**	-0.01569*	0.04216***
	(0.0061)	(0.0091)	(0.0089)	(0.0121)	(0.0084)	(0.0138)
Married	-0.05669**	-0.08637*	-0.15440***	-0.17511***	0.08106	0.04230
	(0.0315)	(0.0467)	(0.0414)	(0.0576)	(0.0513)	(0.0844)
Female	-0.35563***	0.09806***				
	(0.0252)	(0.0371)				
Hispanic	-0.20918***	0.03183	-0.09416	-0.02783	-0.29549***	0.11733
	(0.0688)	(0.0913)	(0.1023)	(0.1281)	(0.0931)	(0.1309)
Black	0.16217***	0.09274	0.30387***	-0.02004	0.00978	0.25951***
	(0.0436)	(0.0627)	(0.0598)	(0.0846)	(0.0645)	(0.0941)
Other race	0.13892	0.38067***	0.2522*	0.37398**	0.0277	0.35689*
	(0.0982)	(0.1305)	(0.1411)	(0.1741)	(0.1371)	(0.1988)
Education dummies	controlled	controlled	controlled	controlled	controlled	controlled
Age group dummies	controlled	controlled	controlled	controlled	controlled	controlled
Wave dummies	controlled	controlled	controlled	controlled	controlled	controlled
N of obs	48857		24366		24491	

*Significance at the 10% level. **Significance at the 5% level. ***Significance at the 1% level.

Note: Housing wealth, property taxes, and financial assets are naturally logged.

Table 6b: Multinomial logit model of unretirement across marital statuses.

Variable	Married		Non-married	
	partly retired	not retired	partly retired	not retired
	(1)	(2)	(3)	(4)
Housing wealth	-0.02839***	-0.04848***	-0.01550	-0.06562
	(0.0106)	(0.0141)	(0.0181)	(0.0217)
Property tax	0.06478***	0.09163***	0.03653**	0.02164
	(0.0101)	(0.0156)	(0.0165)	(0.0241)
Financial wealth	-0.05468***	-0.09652***	-0.05590***	-0.11232***
	(0.0073)	(0.0105)	(0.0124)	(0.0174)
Health	0.30298***	0.35269***	0.35020***	0.48866***
	(0.0136)	(0.0200)	(0.0262)	(0.0389)
Unemployment rate	-0.02636***	-0.01609	0.00260	-0.02031
	(0.0080)	(0.0116)	(0.0168)	(0.0263)
Local tax burden	-0.05525***	-0.06773***	-0.09613***	-0.10794***
	(0.0125)	(0.0186)	(0.0242)	(0.0362)
N of children	0.02648***	0.03226***	0.02345*	0.04796**
	(0.0069)	(0.0102)	(0.0137)	(0.0200)
Female	-0.39750***	0.05814	-0.17905***	0.21389**
	(0.0285)	(0.0415)	(0.0574)	(0.0898)
Hispanic	-0.16799***	0.03258	-0.37890**	-0.00628
	(0.0764)	(0.1034)	(0.1602)	(0.1973)
Black	0.14540***	0.09980	0.17512**	0.08879
	(0.0534)	(0.0771)	(0.0766)	(0.1090)
Other race	0.09249	0.32923**	0.23547	0.53305**
	(0.1178)	(0.1603)	(0.1790)	(0.2264)
Education dummies	controlled	controlled	controlled	controlled
Age group dummies	controlled	controlled	controlled	controlled
Wave dummies	controlled	controlled	controlled	controlled
N of obs	36,760		12,097	

*Significance at the 10% level. **Significance at the 5% level. ***Significance at the 1% level.

Note: Housing wealth, property taxes, and financial assets are naturally logged.

Table 7 : Difference-in-difference estimation of housing price index growth effect on unretirement.

Variable	All	Females	Males	Married	Non-married
	(1)	(2)	(3)	(4)	(5)
Hpi_growth	0.00154***	-0.00132*	-0.00159*	0.00280***	0.00099
	(0.0006)	(0.0007)	(0.0009)	(0.0010)	(0.0007)
Homeowner	-0.02767***	-0.00960	-0.04940***	-0.05717***	0.01117
	(0.0092)	(0.0121)	(0.0143)	(0.0131)	(0.0135)
Hpi_growth*homeowner	-0.00109*	-0.00117	-0.00090	-0.00235**	-0.00047
	(0.0006)	(0.0007)	(0.0009)	(0.0010)	(0.0007)
Mortgage	0.00665***	0.00662***	0.00677***	0.00623	0.00785***
	(0.0004)	(0.0006)	(0.0006)	(0.0005)	(0.0008)
Property tax	0.00513***	0.00315**	0.00730***	0.00807***	-0.00044
	(0.0012)	(0.0016)	(0.0017)	(0.0015)	(0.0019)
Financial wealth	-0.00743***	-0.00804***	-0.00609***	-0.00851***	-0.00544***
	(0.0008)	(0.0011)	(0.0013)	(0.0011)	(0.0012)
Health	0.05646***	0.05545***	0.05784***	0.05515***	0.05937***
	(0.0016)	(0.0022)	(0.0024)	(0.0021)	(0.0026)
Unemployment rate	-0.00305***	-0.00602***	-0.00008	-0.00389***	-0.00090
	(0.0010)	(0.0014)	(0.0015)	(0.0013)	(0.0018)
Local tax burden	-0.00874***	-0.00193	-0.01587***	-0.00831***	-0.01015***
	(0.0015)	(0.0020)	(0.0022)	(0.0019)	(0.0023)
Female	-0.03691***			-0.04548***	-0.01596***
	(0.0036)			(0.0044)	(0.0060)
Number of children	0.00480***	0.00588***	0.00343***	0.00448***	0.00508***
	(0.0008)	(0.0011)	(0.0012)	(0.0011)	(0.0013)
Married	-0.00970**	-0.02062***	0.00600		
	(0.0042)	(0.0053)	(0.0068)		
Hispanic	-0.01389	-0.00010	-0.02717**	-0.01960*	-0.00316
	(0.0086)	(0.0119)	(0.0125)	(0.0109)	(0.0136)
Black	0.02201***	0.03410***	0.00783	0.02023**	0.02355***
	(0.0059)	(0.0077)	(0.0093)	(0.0084)	(0.0081)
Otherrace	0.03249**	0.04405**	0.02119	0.02998*	0.03634*
	(0.0133)	(0.0182)	(0.0194)	(0.0178)	(0.0192)
Education dummies	controlled	controlled	controlled	controlled	controlled
Age group dummies	controlled	controlled	controlled	controlled	controlled
Wave dummies	controlled	controlled	controlled	controlled	controlled
N	58,704	30,647	28,057	39,435	19,269

*Significance at the 10% level. **Significance at the 5% level. ***Significance at the 1% level.

Note: Property taxes, and financial assets are naturally logged.

All estimations pass the parallel trends assumption test using a dummy variable for HPI_growth positive (pre-treatment) and HPI_growth negative (post-treatment). The interaction term remains significant when controlling for potential differences in renter vs. homeowner trends. Still, we provide a further robustness check using an instrumental variables (2SLS) approach as a solution to the suspected endogeneity problem. In Table 8, we follow a common technique (e.g., Chetty, Sandor, and Szeidl, 2017; Aladangady, 2017) by instrumenting for housing wealth with an interaction term taking the movement of a national house price index multipled by a state level measure of housing supply elasticity as estimated/published by Saiz (2010). [Insert Table 8 about here] All of our main results hold up in this environment as well.

Table 8 : Estimation of housing price index growth effect on unretirement, 2SLS

Variable	All	Females	Males	Married	Non-married
	(1)	(2)	(3)	(4)	(5)
Hpi_growth	-0.02043***	-0.01707***	-0.02209***	-0.02577***	-0.00663
	(0.0039)	(0.0058)	(0.0052)	(0.0049)	(0.0063)
Mortgage	0.00799***	0.00736***	0.0087***	0.00771***	0.00833***
	(0.0006)	(0.0009)	(0.0008)	(0.0007)	(0.0012)
Property tax	0.00730***	0.00831***	0.00562**	0.01027***	0.00163
	(0.0019)	(0.0025)	(0.0028)	(0.0024)	(0.0029)
Financial wealth	-0.00936***	-0.01121***	-0.00650***	-0.00817***	-0.01066***
	(0.0014)	(0.0019)	(0.0021)	(0.0018)	(0.0022)
Health	0.05493***	0.05474***	0.05514***	0.05475***	0.05595***
	(0.0026)	(0.0036)	(0.0037)	(0.0031)	(0.0046)
Unemployment rate	-0.01746***	-0.02185***	-0.01268***	-0.01941***	-0.01153*
	(0.0031)	(0.0049)	(0.0040)	(0.0037)	(0.0063)
Local tax burden	-0.00696***	0.00537	-0.01877***	-0.00770**	-0.00624
	(0.0026)	(0.0038)	(0.0037)	(0.0031)	(0.0062)
Female	-0.04130***			-0.05296***	-0.00654
	(0.0055)			(0.0066)	(0.0104)
Number of children	0.000493***	0.00672***	0.00269	0.00515***	0.00439*
	(0.0014)	(0.0019)	(0.0020)	(0.0017)	(0.0025)
Married	-0.00804	-0.02541***	0.02191**		
	(0.0066)	(0.0082)	(0.0109)		
Hispanic	0.02539*	0.02308	0.02366	0.03704**	0.00125
	(0.0151)	(0.0209)	(0.0216)	(0.0183)	(0.0274)
Black	0.03476***	0.04902***	0.02119	0.04096***	0.02579*
	(0.0089)	(0.0118)	(0.0135)	(0.0116)	(0.0135)
Otherrace	0.07597***	0.11706***	0.03734	0.07236***	0.10130***
	(0.0218)	(0.0327)	(0.0294)	(0.0267)	(0.0379)
Education dummies	controlled	controlled	controlled	controlled	controlled
Age group dummies	controlled	controlled	controlled	controlled	controlled
Wave dummies	controlled	controlled	controlled	controlled	controlled
N	26,212	13,109	13,103	19,287	6,925

*Significance at the 10% level. **Significance at the 5% level. ***Significance at the 1% level.

Note: Property taxes, and financial assets are naturally logged.

6. Controlling for Early Retirement Expectations

As mentioned above, there is no theoretical prior governing whether or not adjustments to retirement status are anticipated or unanticipated. An important aspect of the HRS as it pertains to this unresolved issue is that we can identify households whose early retirement expectations and subsequent actual retirement realizations differ. In this section, we present another robustness check that explores what happens when retirement outcomes are modeled using all the previous variables, but also adding early retirement expectations (essentially developing a model of their deviations). Specifically, we estimate equation (8) below that explains retirement status after age 62 using the individuals' previous expectation over this outcome, along with the other variables. Specifically, we have:

$$(8)\ work\_62_i = \beta_0 + \beta_1 ret\_ex_i + \beta_2 X_i + \varepsilon_i$$

where X_i represents various combinations of the control variables used in the previously presented estimations.

Unsurprisingly, a preliminary comparison of estimation results that use retirement expectations at various previous waves reveals the superiority of selecting the expectations from the immediately preceding wave, so we use those as our expectations control variable. Before moving to these results, we would note that a weakness of this robustness check is that our sample size drops dramatically, since the early retirement expectations variable is missing for many HRS respondents. Table 9 presents these estimations. [Insert Table 9 about here] The columns provide the results for all workers, females, males, and married females, respectively. In all four cases, the most recent expectation does a good job predicting retirement – with large coefficients and comparably small standard errors as one would expect. However, they do not perfectly explain the variation; deviations from expectations are still meaningful. We find that workers' retirement decisions are still impacted by changes in housing wealth in this environment, also in the same direction as all of our main models. Here we see perhaps the strongest evidence that these unexpected shocks heavily influence female retirement choices, particularly those of married females, more than is the case for other groups of workers. The housing wealth coefficient for unmarried females (not presented) and for male workers both register as insignificant, whereas the effect nearly doubles in size for married women.

The coefficients on property taxes are positive and generally significant, as expected, and display the same pattern of more dramatically influencing the decisions of married females. Overall, these results indicate expectations are a strong, but not perfect predictor of subsequent retirement decisions, and that changes in housing wealth and property taxes are correlated with the deviations from expectations in the expected manner.

Table 9: Estimating Full-time Employment after age 62 Using Recent Expectations.

Variable	(1) Both Genders	(2) Females	(3) Males	(4) Married Females
The most recent expectation	0.56924***	0.53125***	0.60496***	0.55133***
	(0.0164)	(0.0237)	(0.0228)	(0.0279)
Housing wealth	-0.00942**	-0.01543**	-0.00415	-0.02422***
	(0.0071)	(0.0063)	(0.0058)	(0.0079)
Property tax	0.01565***	0.02243***	0.00798	0.02789***
	(0.0050)	(0.0067)	(0.0074)	(0.0080)
Financial wealth	-0.00041	-0.00227	0.00123	-0.00242
	(0.0037)	(0.0052)	(0.0054)	(0.0063)
Health	0.02572***	0.04312***	0.00826	0.04033***
	(0.0071)	(0.0102)	(0.0099)	(0.0120)
Unemployment rate	-0.00534	-0.00536	-0.00569	-0.01582**
	(0.0040)	(0.0059)	(0.0054)	(0.0068)
Local tax burden	0.00338	-0.00124	0.00806	0.00237
	(0.0062)	(0.0089)	(0.0085)	(0.0105)
Age	-6.3871***	-5.8229***	-6.68274***	-7.22856***
	(1.4108)	(2.0001)	(2.0008)	(2.3586)
Age^2	0.05062***	0.04602***	0.05307***	0.05726***
	(0.0113)	(0.0160)	(0.01603)	(0.0188)
Female	0.07361***			
	(0.0139)			
Married	-0.02725	-0.03950*	-0.01141	
	(0.0178)	0.0223	(0.0309)	
N of children	-0.00540	-0.01123**	-0.00030	-0.01250**
	(0.0036)	0.0053	(0.0050)	(0.0063)
Hispanic	0.03136	-0.02129	0.09608**	-0.04705
	(0.0335)	0.0473	(0.0477)	(0.0550)
Black	-0.00129	-0.02900	0.04153	-0.07641*
	(0.0244)	0.0336	(0.0358)	(0.0439)
Otherrace	0.33111	-0.06529	0.13656*	-0.10453
	(0.2174)	(0.0750)	(0.0700)	(0.0981)
High school	-0.04023*	-0.09540***	0.00888	-0.11644***
	(0.0222)	(0.0324)	(0.0305)	(0.0389)
College	-0.00939	-0.11237***	0.08026**	-0.14835***
	(0.0259)	(0.0383)	(0.0353)	(0.0463)
Otherdegree	-0.33111	-0.42299***	-0.30196	-0.45344
	(0.2174)	(0.3126)	(0.3043)	(0.3110)
R^2	0.2600	0.2475	0.2871	0.2605
N of obs.	4226	2137	2089	1518

*Significance at the 10% level. **Significance at the 5% level. ***Significance at the 1% level.
Note: Housing wealth, property taxes, and financial assets are naturally logged.
Covariates not shown in this table also include wave dummies.

7. Conclusion

Recent decades witnessed unprecedented volatility in the housing market. Many households experienced dramatic fluctuations in housing wealth as they approached retirement. This paper uses restricted access HRS data spanning the boom/bust cycle to explore how housing wealth and property tax liabilities affect employment paths near typical retirement ages. We explore both retirement and unretirement. In addition, two measures of housing wealth - self-reported values and MSA level housing price indexes – are used, since each carries advantages and disadvantages.

The results are intuitive and robust across multiple identification strategies. We show that housing wealth and property taxes play a role in explaining the employment patterns of older workers in the closing stages of their careers. Individuals retire earlier (later) when experiencing positive (negative) shocks in housing wealth and financial wealth, whereas they postpone (hasten) retirement due to higher (lower) property taxes. We also find unretirement occurs more frequently when previously retired households experience unexpected housing wealth losses. Finally, we find that other control variables including financial wealth, health, marital and parental status, macroeconomic labor market conditions, and factors like age, race, and level of education all help explain longitudinal variation in retirement and unretirement outcomes.

While we follow traditional approaches, using respondents' self-classification of "retired", "partly retired", or "not retired". Future work could strive to examine more nuanced definitions of retirement status, and comparisons with our results could test whether there are disparities between self-reported retirement measures and more objective classifications. In addition, prior work (Ruhm 1990) shows that partial retirement frequently involves a change of employment sector, and that women maintain attachment to their prior job/industry more commonly than men do. Given tht our underlying housing wealth effect seems to interact with marital status and gender, it may be interesting to pursue these potential nuances even more deeply. For example, might housing wealth shocks actually influence these outcomes independently or are the effects of the wealth shocks simply magnified and/or muted based on ones initial status regarding these two dimensions. These and other questions relating to labor supply among older workers remain a topic of interest as again populations in the United States and abroad continue to play larger roles in the overall economy.

References

Aladangady, Aditya. 2017. Housing wealth and consumption: Evidence from geographically-linked microdata. *American Economic Review.* 107, no. 11: 3415-3446.

Begley, Jaclene, and Sewin Chan. 2018. The effect of housing wealth shocks on work and retirement decisions. *Regional Science and Urban Economics.* 73, 180-195.

Benjamin, John, Peter Chinloy, and Donald Jud. 2004a. Real estate versus financial wealth in consumption. *Journal of Real Estate Finance and Economics.* 29, no. 3: 341-354.

Benjamin, John, Peter Chinloy, and Donald Jud. 2004b. Why do households concentrate their wealth in housing? *Journal of Real Estate Research.* 26, no. 4: 329-344.

Bhutta, Neil, and Benjamin Keys. 2016. Interest rates and equity extraction during the housing boom. *American Economic Review.* 106, no. 7: 1742-1774.

Chetty, Raj, Laszlo Sandor, and Adam Szeidl. 2017. The effect of housing on portfolio choice. *Journal of Finance.* 72, no. 3: 1171-1212.

Cox, David. 1972. Regression models and life tables. *Journal of the Royal Statistical Society Series B (Methodological).* 34, no. 2:187-220.

Disney, Richard, Anita Ratcliffe, and Sarah Smith. 2015. Booms, busts and retirement timing. *Economica.* 82, no. 327: 399-419.

Disney, Richard, and John Gathergood. 2018. House prices, wealth effects and labour supply. *Economica.* 85, no. 339: 449-478.

Farnham, Martin, and Purvi Sevak. 2016. Housing wealth and retirement timing. *CESifo Economic Studies.* 62, no. 1: 26-46.

Guo, Sheng, and William Hardin. 2014. Wealth, composition, housing, income, and consumption. *Journal of Real Estate Finance and Economics.* 48, no. 2: 221-243.

Hanoch, Giora, and Marjorie Honig. 1983. Retirement, wages, and labor supply of the elderly. *Journal of Labor Economics.* 1, no.2: 131-151.

Harding, John, and Stuart S. Rosenthal. 2017. Homeownership, housing capital gains and self-employment. *Journal of Urban Economics.* 99: 131-151.

Lovenheim, Michael F., and Kevin J. Mumford. 2013. Do family wealth shocks affect fertility choices? Evidence from the housing market. *The Review of Economics and Statistics.* 95, no. 2:464-475.

Lovenheim, Michael F., and Reynolds, C. Lockwood. 2013. The effect of housing wealth on college choice: Evidence from the housing boom. *Journal of Human Resources.* 48, no. 1:1-35.

Lusardi, Annamaria, and Olivia Mitchell. 2007. Baby Boomer retirement security: The roles of planning, financial literacy, and housing wealth. *Journal of Monetary Economics.* 54, no. 1:205-224.

Lutz, Byron F. 2008. The connection between house price appreciation and property tax revenues. *National Tax Journal.* 61, no. 3:555-572.

Maestas, Nicole. 2010. Back to work: expectations and realizations of work after retirement. *Journal of Human Resources.* 45, no. 3:718-748.

Ondrich, Jan, and Alexander Falevich. 2016. The Great Recession, housing wealth, and the retirement decisions of older workers. *Public Finance Review.* 44, no. 1:109-131.

Ruhm, Christopher J. 1990. Bridge jobs and partial retirement. *Journal of Labor Economics.* 8, no. 4: 482-501.

Saiz, Albert. 2010. The geographic determinants of housing supply. *Quarterly Journal of Economics.* 125, no. 3: 1253-1296.

Sevak, Purvi. 2002. Wealth shocks and retirement timing: Evidence from the nineties. Michigan Retirement Research Center Working Paper no. 27.

Shan, Hui. 2010. Property taxes and elderly mobility. *Journal of Urban Economics.* 67, no. 2: 194-205.

Zhao, Bo. 2018 Too poor to retire? Housing prices and retirement. *Review of Economic Dynamics.* 27, no. 1: 27-47.

Zhao, Lingxiao and Gregory Burge. 2017. Housing wealth, property taxes and labor among the elderly. *Journal of Labor Economics.* 35, no. 1: 227-263.

Appendix A: Estimates from retirement hazard model across age groups.

age group	A			
	obs.	housing	prop tax	financial
50+	81362	-0.03292***	0.03549***	-0.06019***
52+	77630	-0.03282***	0.03543***	-0.06038***
54+	72346	-0.03324***	0.03544***	-0.06025***
56+	66022	-0.03258***	0.03550***	-0.06069***
58+	59360	-0.03246***	0.03563***	-0.06112***
60+	52570	-0.03145***	0.03428***	-0.06149***
62+	46231	-0.02700***	0.03163***	-0.06243***
64+	40564	-0.02027***	0.02812***	-0.05993***
66+	35467	-0.01400**	0.02578***	-0.05772***
68+	30959	-0.01106	0.03090***	-0.05627***
70+	26538	0.00173	0.02940***	-0.05340***

age group	B				
	obs.	hpi_g	prop tax	financial	mortgage
50+	77398	-0.00393***	0.01624***	-0.04562***	0.04544***
52+	74034	-0.00389***	0.01631***	-0.04584***	0.04529***
54+	69216	-0.00383***	0.01640***	-0.04584***	0.04527***
56+	63354	-0.00384***	0.01703***	-0.04646***	0.04469***
58+	57105	-0.00381***	0.01765***	-0.04695***	0.04446***
60+	50668	-0.00354***	0.01709***	-0.04733***	0.04436***
62+	44580	-0.00277***	0.01646***	-0.04824***	0.04357***
64+	39038	-0.00211	0.01534***	-0.04557***	0.04277***
66+	34000	-0.00161	0.01543***	-0.04416***	0.04045***
68+	29548	-0.00094	0.02208***	-0.04362***	0.03852***
70+	25259	0.00095	0.02397***	-0.04149***	0.03469***

*Significance at the 10% level. **Significance at the 5% level. ***Significance at the 1% level.

Reply to Reviewer 2, Manuscript # REAL-D-18-00192: "Retirement, Unretirement, and Housing Wealth during the Great Recession". Revised and resubmitted to *Journal of Real Estate Finance and Economics.*

Dear Reviewer #2,

Thank you for your continued insights. Reacting to your remaining concerns led to significant improvements in the paper. For your convenience, we copied your original comments below and followed each with our reply.

Comments

1. *Your comment*: "Retirement is defined by self-reported status. But this is a definition of retirement based on conditions at the time, notably for the 1991 to 2010 period. To be retired means to be qualified to receive a pension, notably for a defined benefit.

   Over time, a smaller proportion of retirees are going to quality for a defined benefit. There needs to be a distinction between those who previously were in the public sector or other places where a defined benefit is paid. The definition of these retirees and their responsiveness to financial wealth is muted if not almost irrelevant. They will retire when they qualify for the defined benefit pension.

   Within these usually state and local public-sector work forces, the provisions can be generous but force someone to be officially retired. One condition is disability, another one requiring retirement and not planning to return to work. The issue is that someone is required to be retired to claim a benefit including a pension. For these people the causality is reversed.

   For other people who have a defined contribution or build-up the response will almost be the opposite. There is an income and substitution effect. Working longer allows added qualification for funds, especially with add-on provisions for people aged greater than 50. The income and wealth effect comes from the ability to tap into the built-up fund."

   Our reply: Thank you for these insightful comments. Taking them one at a time, you first mentioned that retirement is self-reported. Absolutely – you are 100% correct. This is one reason we are happy to be using the HRS, as it is the gold standard of household level research involving older American households. We also define the retirement decisions in ways that are very transparent and consistent with the previous literature.

   Next, regarding the pension eligibility status of the respondent its potential connections to retirement timing. You are correct that pension eligibility – particularly defined benefit pension plans where workers become "vested" or qualified by passing various thresholds – plays a big role in terms of influencing the timing of retirement. We see evidence to show you are correct. While the ways we address the role of pensions in the initial submission were minimal, we have enhanced that part of the analysis.

The HRS has some information related to pensions, but unfortunately not everything one might wish to observe. So for example, we know a person is pension eligible if they are drawing a current benefit. However, if they are not yet drawing, to observe they have a pension we need it to be one associated with their current job. This is likely to be exhaustive in most cases. However, in some rare cases, a worker could be fully vested in a pension from a previous job, and yet have no pension associated with their current job. [Retired Military workers for example may fall into this category.] Fortunately, we believe this is fairly rare. At the end of the day, over 60% of our sample reports having some direct connection to a pension plan that we can observe, split nearly perfectly even between defined benefit (DB) and defined contribution (DC) plans, and the "missed" pensions are not likely to be common.

That is the good news! The bad news is that very detailed information like the size of the assets (for a DC plan) or the size of the expected monthly payment (for a DB plan), along with the specific dates of eligibility for the plans, are not observed. Of course, please bear in mind that for many plans – particularly DB plans – they are complicated to a point where gathering easily quantifiable measures is hard. Often workers may accumulate better and better benefits the longer they work – a phenomenon we expand on below.

Importantly, while pension status is *one* determinant of retirement timing, it is not *the only* determinant. Moreover, it is not the determinant we are focusing on in this study. Our concern is not whether or not pension eligibility plays a role – as we know it surely must and have not chosen to focus on that effect in this study. Our goal is to estimate a clean/unbiased effect of the variables on housing wealth and property taxes, within an environment where many other factors play a role. Towards that end, we would offer a few initial reactions to your comment (and then expand in more detail below):

1. We see clear evidence that even for workers who carry pension eligibility, other factors (including housing wealth and property taxes) still play a role in influencing the timing of retirement. For example, in estimations where we recursively drop various "pension groups" (e.g., holders of DB pensions or holders of DC pension) from the sample as they relate to pension eligibility, we see little to no movement in the direction or significance of the key housing variable results. [This is explained in more detail below.]
2. We do not see any clear reasons why pension eligibility would introduce bias into the estimated coefficient on housing wealth in the first place. As you corrected stated, pensions are extremely common. Nearly two-thirds of our sample reports having a pension, and this is a lower-bound estimate given the discussion outlined above. The changes in housing wealth we observe in the panel are primarily driven by different house price experiences occurring in different parts of the US. However, pensions are common all over the US, and we do not observe any significant correlation between the presence of a

pension and the patterns of changes HRS respondents had in terms of their housing wealth and property taxes.

3. Very few pensions are attached to a single all-or-nothing type structure. Most, including pensions associated with government and military service, carry nuances that respect years of service and highest year(s) of earned wages/salaries in a continuous manner.

So to expand on these reactions, when you say "They (referring to DB pension eligible workers) will retire when they qualify for the defined benefit pension", yes, we agree that the core of this statement is clearly true. However, we must be careful before taking this idea too far. Other factors including housing wealth, property taxes, financial wealth, health status, and marital/parental status still influence retirement timing, even in the presence of a DB pension.

As your comment suggests, it is true that many workers are in situations where a particular date/time brings an important change in their pension status. So for example, a typical DB pension in the US military system has a service year threshold (e.g., 20 years) and then triggers when the worker hits ages like 60 or 65. [This is also true for other government based defined benefit pensions.] Importantly though, all of our estimations control for worker age – so this is covered and should not influence any of the estimated coefficients on the key housing wealth, property taxes, or financial wealth variables.

Also, in many cases, the defined benefit can still increase if the worker continues to work past the initial eligibility threshold. For example, a basic VA pension is calculated as 2.5% per year of service of the highest 36 months of base salary. So a DB pension eligible worker can still work longer in order to increase their eventual monthly defined benefit.

And yes, in some cases workers must retire from a current job without working elsewhere to draw benefits. However, we are confident this is fairly rare. Most defined benefit pensions – including the commonly drawn pension associated with service to the US armed forces – carry the option to work. Yes, there can be exceptions to this or even interactions with the pension benefits themselves, but many older individuals officially "retire" (i.e., take/start their pension), but then still work (sometimes even for the same company). This last situation for example – having triggered the pension but continuing to work for the same company – this is happening currently with the father of one of the co-authors on this paper.

Unfortunately, anyone working with HRS data does not have these specific relevant pension dates for most of the sample. We can observe when workers actually start to draw on a pension from one of the variables available – and one could assume that was the first date they were eligible to do so – but again, that would be an incorrect assumption in many cases.

So yes, we agree wholeheartedly that the particular date/year of the gained eligibility should matter – but the likelihood of retiring precisely at that point of eligibility is *still influenced* by other factors including housing wealth and property taxes. In fact, we now mention this directly in the paper now on page 19. We mention how all of our main results hold even when we sequentially drop each of the major pension related groups (i.e., DB pension, DC pension, and no pension) from the sample. We see the same qualitative results coming from each of these pair-wise grouping, suggesting no individual pension group is driving the results.

However, this does not mean that the pension status does not impact retirement timing, but rather just means that even for the large groups of workers who are DB pension or DC pension eligible (which again is most of the sample) the effect of housing wealth and property taxes holds.

And in fact, even within both of these distinct "pension eligible" environments (i.e., the DB and the DC environments), many other factors including the workers health, gender, parental/marital status, race, age, all still play an independent role in determining the timing of retirement. Again, we have to thank you for brining this important material into focus. It led to this direct improvement in the framing/discussion of the main results.

And finally, you are of course correct that these trends have been changing over time – something that we now do a better job of acknowledging right in the very start of the paper (see page 1).

2. *Your comment*: "The focus of the paper has been on property taxes. There are other cash flow aspects to owning a house, notably for a mortgage. The paper constructs housing wealth as the house value less mortgage debt. There is a difference in behavior between two households with similar net equity. A household with a house worth $100,000 and no debt has $100,000 in house equity. So does another with $200,000 in gross value and $100,000 in debt. The latter household faces a mortgage payment, while both deal with property taxes. The households with the same equity are not going to behave identically unless confirmed by the data."

   Our reply: Yes, we do see your point. We have a few comments in reply.

   We would see the main contribution of the paper as one that focuses on the role of housing wealth as primary, and then on the potentially offsetting effects of property taxes as a secondary emphasis.

   But yes, to focus on how we construct the variable measuring housing wealth – we follow the current standard in the literature and measure housing wealth as whatever a person could "walk away" from the current home obligations holding. So in your case, both persons walk away with $100,000 if they sold, so they have the same amount of wealth. And yes, they get the same estimated coefficient based on that similar holding from the wealth effect – but most of our models (see Table 4a, 4b, 5) do exactly what you are asking for by including a variable noting whether or not the person holds a current mortgage. The presence of that variable never causes the direction or significance of the housing wealth effect to change, a result we find reassuring.

To the extent the second person in our example has a more expensive home that is associated with greater property taxes (basically double in your example), then that also would be controlled for directly by the inclusion of each households reported property tax burden.

Moreover, we investigated the possibility you suggested that property taxes carry a differential effect on (outright) homeowner's relative to those in current mortgages, and found the estimated effects on retirement timing to be similar.

3. *Your comment*: "For older people, property taxes are frequently deferred or postponed altogether and often reduced. Any discussion and treatment of property taxes must take reductions and subsidies into account. Higher property taxes leading to increased retirement probabilities could be confounded by local public-sector requirements. Some jurisdictions keep raising property taxes to pay for retirement benefits. The property-tax effect will depend on whether that locality is obliged to fund current defined-contribution benefits for existing retirees."

   Our reply: Thank you for this fantastic point. In fact, we agree completely. We have a few points worth noting in reply – and we would highlight that this also gave us the chance to better clarify these points in the manuscript.

   You are right that many states have programs protecting the rate of year-over-year property tax increases for older homeowners. Many of these same protections extend to younger homeowners as well. In terms of the benefits funding issue – since we have individual respondent level data, all of this would be accounted for as we measure both the property tax liability and the nature of the pension/benefits package. Also, most workers in the HRS sample are not working in public sector government jobs, and therefore not directly influenced by how the local government funds their pension plan.

Finally, we again offer our sincere thanks for your careful review of out paper and for the excellent comments/suggestions you made. We hope at this point you are prepared to support the paper moving forward towards publication. Of course, if there are additional ways we can make the paper even better, we are happy to do so!

Reply to Reviewer 3, Manuscript # REAL-D-18-00192: "Retirement, Unretirement, and Housing Wealth during the Great Recession". Revised and resubmitted to *Journal of Real Estate Finance and Economics.*

Dear Reviewer #3,

It was very kind of you to thank us for the improvements to the paper in your report. Of course, we are the ones greatly in your debt – as we value your insights.

Your additional comments further motivated us to improve the way we present the contributions of the paper. For your convenience, we copied your second round review comments below, followed by our reply to each.

Comments

1. *Your comment*: "You are correct in stating that Zhao and Burge (2017) focus on current labor supply, and the present paper focuses on (un)retirement transition. It would be better if you could clarify this in Section 1 and 2 of the manuscript, which, as they currently stand, do not mention the differences with Zhao and Burge (2017)."

   Our reply: Absolutely! Thank you for noting the opportunity to improve the clarity of this point. The revised manuscript follows your exact suggestion. We kept the narrative much shorter than the one we presented in our initial reply, but key distinctions were highlighted. As suggested, this came in Section 1 (pages 2-3).

2. *Your comment*: "Related, I suspect that the estimation strategy leading to Table 7 and Table 8 is similar to that in the Zhao and Burge (2017); if so, it would be better to spell out what exactly is the 2SLS equation, especially, what the dependent variable is, for you need to clarify before- and after-, control versus treatment groups in diff-in-diff estimations."

   Our reply: This is a great suggestion! We followed it directly in the revision. The material clarifying the before- and after-, control/treatment pairing is now better explained. We also took the chance to further buttress our findings by mentioning the results of the "parallel trends" assumption that validates the results of diff-in-diff estimations. This material is mostly on pages 28-32.

3. *Your comment*: "I comprehend partially what you mean by saying that the identification strategy rests on "households did not sort themselves into the renter and homeowner categories in systematically different ways across different metropolitan areas based on what future housing price trends would be…" (pp.11 and pp.23). Still, this assumption may be difficult to grasp without mathematical notations. It may be a good idea to at least clarify this assumption mathematically in a footnote (if lack of space is a concern). You also write that "this assumption is made by many well published papers" (pp.11) and "has been used a number of times

successfully in previous research" (pp.23). Could you list a couple of (if not all of them) these papers?"

Our reply: Yes, thank you for pointing out the opportunity to make these improvements. We followed your request to provide a (partial) list of recent papers published in solid outlets including the *Journal of Labor Economics*, the *Review of Economics and Statistics*, the *Journal of Human Resources*, and *Regional Science and Urban Economics* – as some good examples. While this list is not exhaustive – as you mentioned – it is still helpful. Also, we took this chance to further clarify the assumption itself. Your request for mathematical clarity is also one that we honored – but we choose to do that with words (invoking math/stats!) – rather than adding a new formal equation. We stated that: "Statistically, this just means the probability of being a renter in the pre-bust period is uncorrelated to future housing price trends within the MSA."

This choice was in keeping with the style and flow of the rest of the paper. We also find it pretty intuitive for readers.

4. *Your comment*: "In Section 5, when analyzing unretirement decisions, why does the analysis suddenly change into the multinomial logit model? This causes difficulty when one wishes to discern the relative effects of housing wealth on retirement versus unretirement decisions. I understand that the previous literature probably has used it and you have separated the "completely retired" from "partly retired", but in analyzing the retirement decision, you lump the two together into one category. Why not do the same for unretirement analysis? Otherwise, we cannot compare the coefficients from different models conveniently without first converting them into odds ratio or probabilities."

Our reply: Thank you for this insightful comment. We will address each part separately.

First, the modeling choice (i.e., the decision to use the multinomial logit models):

We already stated our original motivation – which is that the empirical models follow the 2010 *JHR* paper by Nicole Maestas – but we should have done more. In fairness, this influential in the unretirement/retirement-reversals literature has been cited over 400 times, and we did not want anyone to think we were cherry-picking our results, by estimating empirical models other than what the unretirement literature seems to have "settled" on.

However, we agree that we could do more to justify the choice of the multinomial logit – as it does carry several advantages over other models for this application. Those explanations now come at the appropriate point in the manuscript.

Regarding the part of the question asking about collapsing the three outcomes into two, we agree with you that some value would be gained by being able to directly compare the coefficients across the retirement and unretirement models. However, we must also point out that comparisons made across the two environments are intrinsically much harder than your comment acknowledges.

One process (retirement) is something nearly everyone in the HRS sample eventually gets to, and the relevant empirical issue is when they do it (i.e., how fast). As such, the primary models of interest for the retirement outcome are hazard models – a basic type of event-history models – where variables either speed up or slow down the (assumed to be) unavoidable outcome. The estimated difference-in-difference models are only meant to support the other main findings, lending additional credibility to our primary result.

Put another way, we are not particularly excited about interpreting the magnitudes of those coefficients in Table 5 in the first place. In fact, note that we do not talk much in the paper about the size of those coefficients – just their significance and direction. They technically represent the change in the linear probability of being in the retirement category at a given observation as the House Price Index for the MSA of the resident goes up by one unit (which is a one percentage point increase in this case). However, they are arbitrarily manipulated by things like the frequency of the HRS survey (e.g., every 2-3 years versus every year) and the pre-existing level of the HPI_growth variable in the respondents MSA, making the interpretation of the magnitude of their effects challenging.

On the other hand, once a worker has already initially retired, the most reasonable way to model the reversal is as a decision: specifically a decision where they may or may not come back into work. In that context, there is not much gained through a focus on the timing over how long it does or does not take. Also, certainly coming back into part-time work and coming back into full-time work would be different outcomes of interest.

More importantly, note that in Tables 6a and 6b we model both the part-time and full-time work re-entry outcomes separately, but the models are otherwise identical. Hence, by construction, if we combined the two outcomes into one, and then ran the same model, we would get something between the current -0.02567 (part time) and -0.05392 coefficients (full time). We feel like some readers may be interested in seeing both, so we left it alone. Also, when we estimate models that combine those two categories into one group, as you mentioned, the results remain very similar as one would expect – with the moderate sized coefficient between the other two presented results, so that is quite reassuring. If proving another set of results where the two outcomes are collapsed is very important to you, we can of course add that before publication.

5. *Your comment*: "In the analysis of the married subsample versus the unmarried (Table 4b, 5, 6b, 7, 8, 9), the differences in housing wealth effects strike me as large between these two groups. Do you control for spouses" labor supply? Intuitively, spouses' labor supply may affect respondents' retirement decisions, all else equal."

Our reply: While the coefficient coming from – for example Table 4b – shows the effect to be stronger for the unmarried group, there are other instances – for example Table 5 or Table 6b – where the coefficients estimated across the married and unmarried subsamples do not show a clear pattern of one being larger than the other.

We agree that spouse's labor supply can and should matter, but that is in fact one of the biggest reasons (if not the biggest single reason) why we decided to split the sample to see what happens with the baseline effect. Once the sample is split by marital status, only one of the estimations could control for spouses hours worked.

In general, the findings related to marital status are mixed – and thus we try not to overstate any claims regarding this topic. In some cases (Table 4b) it seems like non-married workers carry a larger response to changes in housing wealth, but in others (Table 5) the pattern reverses and married workers seem more intensely effected. At the end of the day, we have tried to keep our discussion of this issue specific to each particular set of results, and have not gone further. Also, it is a tricky issue, because marital status itself (i.e., independent of any interaction effects with housing wealth) plays a huge role in retirement timing. We can see from Table 4a that marital status itself influences retirement timing far more dramatically in magnitude than any of the wealth effects we are picking up from the housing wealth or financial wealth variables.

6. *Your comment*: "In Figure 1, when calculating the proportion of retirement transitions, is the denominator the total number of persons at each depicted age, or the number of unretired persons at each age? I find the declining proportion of retirement transitions after age 62 is a little intriguing."

Our reply: This is a fantastic question! Yes, we agree with you, there are two ways retirement rates/proportions could be constructed: overall (baseline) retirement rates and conditional retirement rates.

Method 1: Overall (baseline) retirement rates. So for example, taking all the respondents in the sample who report being age 67, what proportion report their initial retirement that wave? Figure 1 follows this approach. We can see from the Figure that the answer to this question is that about 1 out of every 10 respondents who is 67 reports an initial retirement. Of course, as your comment indicates, it is only this low since most respondents had reported a retirement in a previous wave.

Method 2: Conditional retirement rates. So for example, *given* that a 67 year old HRS respondent had not previously reported a retirement, what is the conditional likelihood they report retirement in the present wave? As you are thinking, this rate would be (much) higher. [In fact, this rate would be monotonically increasing. Perhaps this sort of a visual trend is what you were expecting to see.]

Between the two methods, we favored the first only because it directly shows the relevant story of the "retirement hump" peaking out around the relevant Social Security eligibility thresholds.

One tricky aspect of using Method 1 to construct Figure 1 is that – under other conditions – this choice would produce a set of proportions mathematically summing to 100%. However, our data does not produce this "clean" statistical outcome for several reasons including the bi-annual nature of the HRS survey waves, some unavoidable attrition seen in the HRS panel, and the fact that respondents can retire outside of the age 51-69 range. For example, some of our HRS respondents have still not retired at all – only future survey waves would see the completion of their careers. In the resubmitted paper, we do a better of clarifying this. Thank you for this chance to enhance the clarity of Figure 1.

7. *Your comment*: "The paper needs further proofreading. For example, on the bottom of pp. 18, "Additionally, we general find the magnitude of an additional dollar of…..carries a smaller impact than an additional dollar of….." is not correct grammatically.

   Our reply: Thank you for catching this grammar error and for correctly noting the paper could improve through better editing and enhanced prose. Our final review corrected a few other minor typos. Of course, it is possible others are still buried within the text. Hopefully the final copyediting process will uncover any remaining grammar errors.

Finally, we again offer our sincere thanks for your careful review of the work and for having providing us with an important perspective on the work. We appreciate the way it challenged us to improve the paper. We hope you will agree with us that the paper is now exceeding the standard needed to move forward with publication at *JREFE*. Of course, if there are other ways it can improve, please let us know.