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The Housing Wealth Effect: The Crucial Roles of Demographics, Wealth Distribution and Wealth Shares

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ABSTRACT

Current estimates of housing wealth effects vary widely. We consider the role of omitted variables suggested by economic theory that have been absent in a number of prior studies. Our estimates take into account age composition and wealth distribution (using poverty rates as a proxy), as well as wealth shares (how much of total wealth is comprised of housing vs. stock wealth). We exploit cross-state variation in housing, stock wealth and other variables in a newly assembled panel data set and find that the impact of housing on consumer spending depends crucially on age composition, poverty rates, and the housing wealth share. In particular, states with more young people who are more likely to be credit-constrained, and older homeowners, likely to be "trading down" on their housing stock, experience the largest housing wealth effects, as suggested by theory. Also, as suggested by theory, housing wealth effects are higher in state-years with higher housing wealth shares, and in state-years with higher poverty rates (likely reflecting the greater importance of credit constraints for those observations). Overall, we estimate the *average* housing wealth effect to be approximately 8.1 cents per dollar. However, consistent with theory, demographic and wealth characteristics of the population cause this effect to vary widely across states and over time.

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1 Introduction

If the value of your house rose $10,000 this year, by how much would your consumption rise this year? This is a straightforward question, but economists have failed to agree on an answer that is consistent with the theoretical modeling of consumption wealth effects, as evidenced by the wide-ranging empirical estimates of their magnitude.

In theory, the estimation of wealth effects should take into account variation related to age and the composition of wealth. Consumers with different age and wealth characteristics should have different housing wealth effects. Households that face binding constraints that limit their borrowing against future income (for example, young people just starting a family) or older people who plan to downsize their housing consumption in the future should exhibit relatively large housing wealth effects, while those who neither face binding borrowing constraints nor are planning to downsize their housing consumption in the near future should exhibit smaller housing wealth effects.

Empirical evidence on aggregate housing wealth effects has produced widely varying estimates. There are a number of problems that have made it difficult to interpret the sources of empirical disagreements across studies. First is the challenge of finding reliable data on housing wealth, securities wealth, consumption and other variables of interest. Although good measures of these variables exist for the United States as a whole, aggregation over regions with different economic cycles and limited degrees of freedom from time series aggregates make it difficult to obtain reliable estimates of consumers' responses to variation in wealth and income. In principle, the cross-sectional variation in state-level panel data could provide more precise estimates. In practice however, finding reliable state-level data is a challenge. For example, state-level consumption is typically proxied using retail sales while data on securities wealth are constructed by allocating aggregate figures across states using household surveys on mutual fund holdings. This is particularly problematic because these surveys are only available for a handful of years, forcing researchers to interpolate across many intervening quarters.

Second, wealth effect estimates are acutely prone to bias due to omitted variables. For example, in a regression that omits unobservable permanent income, housing wealth changes (which are likely correlated with omitted expected future income) may proxy for the omitted variable; thus, observed housing wealth effects may overstate true wealth effects. Calomiris *et al.* (2009), following Campbell and Mankiw (1990), employ instrumental variables to address that problem, and find that taking this bias into account substantially reduces estimated housing wealth effects (see also Case *et al.*, 2013, who adopt that same approach).

Third, the functional forms for estimating wealth effects in prior work are generally not consistent with some of the basic implications of the permanent-income/life-cycle model of consumption. As Carroll and Zhou (2011) have noted, coefficient estimates from the standard empirical functional form that regresses the log of consumption (or its difference) on the logs of income, housing wealth, and securities wealth (or their differences) cannot be interpreted as measuring a standard wealth effect; instead those estimates simply measure partial correlations between housing (or equities) and consumption.

A particular problem with regressions using the standard functional form is that they posit a constant elasticity of consumption with respect to housing wealth. The reasonableness of this assumption, however, depends on the constancy of the ratio of housing wealth to securities wealth. As we show below, there have been remarkable swings in the ratio of housing wealth relative to stock wealth. Indeed, the housing boom and bust between 1999 and 2010 saw dramatic changes in that ratio. If the housing wealth ratio is not constant, then assuming constant elasticities in estimation can result in severe bias. To see why, consider two individuals, A and B, who each earn \$50,000 per year and consume \$55,000. Individual A possesses \$1,000 in securities wealth and \$500,000 in housing wealth while individual B possesses \$500,000 in securities wealth and \$1,000 in housing wealth. Suppose that actual individual behavior follows the following pattern: consumption equals 80 percent of current income plus 3 percent of total wealth, irrespective of whether wealth is in housing or securities.

Suppose that one employs the standard functional form: $\ln c = \beta_0 + \beta_i \ln i + \beta_h \ln h + \beta_s \ln s$, where c is consumption, i is current income, h is housing wealth, s is stock wealth, and β_0, β_i, β_h, and β_s are parameters to be estimated. Suppose that one runs this specification on a sample that pools together a large population of many individuals, consisting of equal numbers of types A and B, and further suppose that the estimated elasticity of consumption with respect to housing wealth from that regression (parameter β_h) is 0.015. This estimate suggests that a 1 percent increase in housing wealth should give rise to a 1.5 percent increase in consumption.

This estimate, however, is not close to accurate for either type of individual in the population. For Type A individuals, consumption rises by roughly 3 percent when housing values rise by 1 percent, since almost all of type A's wealth is in housing. For type B individuals, consumption is virtually unaffected when housing values rise by 1 percent, since housing wealth is a trivial fraction of total wealth. One contribution of our paper is that we address this wealth-heterogeneity problem by allowing the elasticity of consumption with respect to different types of wealth changes to vary according to the ratios of each type of wealth to total wealth.

Finally, as the theoretical insights of Buiter (2007) and Sinai and Souleles (2005) emphasize, the demographic characteristics of the population should matter for housing wealth effects. If older people are more likely to downsize and younger people are more likely to face binding borrowing constraints against expected future income, then both young and old people should exhibit larger housing wealth effects than those who are middle-aged. Thus, in a panel analysis of U.S. states, heterogeneity across states or over time with respect to age distribution should have important implications for housing and securities wealth effects.

Along a similar line of reasoning, we posit that the distribution of wealth should matter to the extent that borrowing constraints bind (which should raise estimated wealth effects of consumption). Specifically, we allow wealth effects to depend on the extent of poverty in a state. Of course, poor people generally cannot afford to buy homes, but we expect that a higher incidence of poverty is correlated with the share of the population that has a low level of per capita wealth. Thus, higher poverty rates should be associated with higher wealth effects because a greater proportion of low-wealth individuals (including homeowners) should be associated with more binding constraints on borrowing against permanent income. Our assumption that the poverty rate is a proxy for the share of lower-wealth homeowners is supported by the fact that alternative regression specifications that substitute the unemployment rate for the poverty rate produce similar results.

In this paper, we deal with all of these considerations when we estimate consumption wealth effects for housing and securities. First, we construct a new annual dataset for the U.S. states for the period 1981–2009. By focusing on annual data, we are able to avoid several problems that may arise when using quarterly data. Second, we employ the same instrumental variables approach used in Calomiris *et al.* (2009). Unlike that study, we find that housing wealth effects are positive and significant after instrumenting. We attribute this change to improvements in the quality of the data employed in the present study.

Third, as suggested by the life-cycle consumption theory, we demonstrate that an empirical specification that takes into account the relative amount of housing and securities wealth in a given state-year improves the accuracy of the estimation. This reflects the fact that there is substantial variation across states and over time in the composition of total wealth.

Fourth, taking demographic variation (differences in age and poverty rates) into account also proves to be important, both across states and over time. As suggested by theory, housing wealth effects tend to be larger in state-years with high proportions of young and old people, and those with higher poverty rates. Given the substantial variation across states and over time in these population characteristics (reflecting, in part, the differential effects of the baby boom across states), it turns out to be important to take demographic differences into account when measuring wealth effects.

Overall, we find that consumption responds positively to innovations in both housing wealth and securities wealth, but housing wealth effects are significantly larger than stock wealth effects. On average, a 1 dollar increase in the value of housing wealth raises consumption by roughly 5 to 8 cents. In contrast, a 1 dollar increase in the value of securities wealth raises consumption by less than 2 cents on average. It is important to note that there is substantial variation across states and over time in both of these consumption responses to wealth changes, which are related to changes in the age, poverty and wealth characteristics of the population over time. The responsiveness of consumption to changes in different types of wealth should therefore be understood within the historical context of the importance of housing wealth as a fraction of total wealth, and the demographic and wealth composition characteristics of the population.

The magnitudes of these effects are considerable. For example, in our preferred specification, a one standard deviation increase in the share of young people in a state (a 4.1 percent change) would raise the estimated housing wealth effect by 0.9 cents; a similar increase in the elderly population (3.3 percent) or the poverty rate (3.8 percent) would raise the housing wealth effect by 1.8 cents and 1.3 cents, respectively. The composition of wealth has an even bigger impact on housing wealth effects: a one standard deviation increase in the housing wealth share (9.4 percent) within a state raises the housing wealth effect by 2.1 cents. Given the wide variation of age demographics, poverty rates, and wealth shares across states and over time, these estimates suggest it is important for policy makers to take these factors into account when considering policies that are affected by housing wealth effects.

Section 2 of this article briefly reviews the literature on estimating the consumption elasticity of housing and stock wealth. Section 3 describes our data set. Section 4 presents our empirical findings, and Section 5 concludes.

2 Previous Literature

Standard analysis of consumption decisions in a Permanent Income Hypothesis (PIH) framework indicates that an increase in the value of an agent's assets should cause the agent to increase consumption. Poterba (2000) summarizes the issues and findings relating to consumption effects of increases in stock values. He points out that, even in the absence of credit constraints or other imperfections, agents that are rational, forward-looking optimizers should increase consumption in response to the higher wealth that stock price increases create. It is therefore not surprising that a number of papers (Ludvigson and Steindel, 1999, is one of many examples) find a significant, positive consumption wealth effect from increases in stock wealth.

Housing shares some similarity to equity in that it is an asset, and thus there may also be a wealth effect on consumption from an increase in housing values. However, housing is also a consumption good, and a wealth effect from higher home prices is not as theoretically obvious as it is in the case of stocks. Buiter (2007) quotes Bank of England Governor Mervyn King, who stated that "housing wealth isn't wealth." The value of a house is simply the present value of the housing services it delivers in the future. Those who have more housing than they plan on consuming in the future (those who are net "long" housing) will be better off from an increase in house prices, and may, as a result, increase consumption. Those owning less housing than they plan to consume in the future will be made worse off, and may decrease consumption as a result. On average there should not be a large *net* housing wealth effect, since most residents own the houses in which they live. Buiter thus presents a model in which the only way that a net housing wealth effect is generated is through distributional considerations that result in small net wealth effects.

Sinai and Souleles (2005) also develop a theoretical model in which aggregate housing wealth effects should be relatively small for aggregate non-housing consumption. Their model however takes borrowing constraints into account, which makes it possible for housing wealth to exert a larger effect on consumption. Because future income cannot be credibly pledged to lenders, the possession of housing wealth can increase current consumption for borrowers with high expected future income growth. Indeed, housing wealth may be superior to stock wealth as collateral, since maximum permissible loan-to-value ratios on mortgages are much higher than margin limits on stocks, and because mortgage interest is tax-deductible while margin loan interest is not. As in Buiter (2007), an increase in house prices causes higher housing asset values but also causes an equivalent increase in housing liabilities (the cost of future housing consumption); any effect from increases in housing values on non-housing consumption therefore primarily reflects the impact of the relaxation of borrowing constraints on consumers, given housing's special value as collateral for consumer borrowing.

Thus, theoretically it is not at all clear that a substantial housing wealth effect on aggregate non-housing consumption should be observed; the size of the effect depends on the proportion of the population that is subject to binding borrowing constraints, and the distribution of this wealth among populations that are either net long or net short housing. The housing wealth effect may be greatest for younger homeowners who are most likely to suffer from credit constraints, or for older homeowners who are contemplating imminent downsizing.

Given the theoretical ambiguities of the housing wealth effect, a number of papers have attempted to empirically gauge the impact of rising home prices on consumption, and compare that housing wealth effect with the effect of stock wealth changes on consumption. Carroll *et al.* (2011) examine the housing wealth effect in the context of a habit formation model using aggregate time series data. The authors find that consumption rises more in response to housing than to stock wealth.

Carroll and Zhou (2011) use a panel data set of U.S. states to examine the housing wealth effect, and find a positive housing wealth effect but no significant stock wealth effect. They construct new semi-annual data on consumption and financial wealth at the state level that is likely more accurate than the data used in some previous papers. As in the present study, the authors employ data based on the Federal Housing Finance Agency (FHFA) home price index.[1] A major limitation of their data, however, is that it only runs from 2001 to 2005. This is a much shorter span than prior panel-based studies, which often have data covering three decades or more. As a result, the Carroll and Zhou (2011) data set misses out on most of the more volatile and infamous national and local housing cycles over the past 30 years.

Several studies employ micro data on households. Mian and Sufi (2011) analyze data on 75,000 existing homeowners over time and across Metropolitan Statistical Areas (MSAs), and conclude that the recent housing boom boosted consumption in the United States. Like us, Mian and Sufi analyze how age and financing constraints affect wealth effects, and find that younger homeowners and those with low credit scores and greater reliance on credit card borrowing (which may proxy for financing constraints) respond more to a rise in home values by borrowing against the value of their homes. Bostic *et al.* (2009) examine data from both the Survey of Consumer Finances and the Consumer Expenditure Survey, finding that housing wealth appears most highly associated with non-durable consumption, while financial wealth is most closely linked with expenditures on durables.

One of the most highly cited studies on housing wealth effects is Case, Quigley and Shiller (CQS, 2005). This study uses a panel of quarterly data for U.S. states running from 1982 to 1999, as well as a panel of fourteen OECD countries using annual data from the same period. The authors later updated this study (CQS, 2013); the new panel data set (for U.S. states only in this version) runs from 1978–2009.

The CQS (2005, 2013) studies estimate the effects of wealth on consumption in a variety of ways. First, they model the level of consumption as a function of the level of income, stock wealth, and housing wealth. Next, they model the difference in consumption as a function of differences in housing wealth, stock wealth, and income. CQS also estimate a version of an error correction model, in which the parameters of the cointegrating vector are imposed (income is constrained to affect consumption one-to-one). In all of these specifications, housing wealth is found to have a positive and significant effect on consumption, and in nearly all cases, the housing

[1] The FHFA was formerly known as the Office of Federal Housing Enterprise Oversight (OFHEO).

wealth coefficient is larger than the stock wealth coefficient. While the 2005 study only covers the years from 1982 to 1999, and therefore misses the latest dramatic rise and fall in house prices, the more recent study has been updated with quarterly data spanning 1978–2009.

In their 2005 paper, CQS regress the current change in consumption on the *current* change in income, housing and stock wealth (without instrumenting). This causes a potentially severe endogeneity problem. Aron and Muellbauer (2006) point out that studies of the housing wealth effect tend to be plagued by "poor controls for common drivers" of both housing wealth and consumption. One key common driver is permanent income. An increase in expected permanent income will increase both consumption and demand for homes, and therefore house prices. Because CQS (2005) do not control for shocks that are related to permanent income, it is possible that their results are driven by correlations between permanent income shocks (which should be the dominant source of housing price changes across time and across states) and housing price changes. In other words, in states where housing prices are rising, that rise reflects not just past income growth, but also expectations of future income growth, which may produce improvements in many current market indicators, including rising home values.

In CQS (2013), the authors do include regressions that control for omitted variable/endogeneity bias by instrumenting wealth, following the methodology of Campbell and Mankiw (1990). The Campbell and Mankiw (1990) approach to instrumenting is intended to identify changes in endogenous variables that are uncorrelated with shocks to permanent income by using lagged endogenous variables as instruments for current wealth variables. This technique — which is also explained at length in Calomiris *et al.* (2009) — is valid so long as adjustment lags are not protracted (as we discuss in more detail below). The main results of the CQS (2013) paper are qualitatively similar to their earlier paper — an increase in housing wealth is associated with a statistically significant increase in consumption, and this effect is larger than that of an increase in stock wealth — although the authors now report a wider range of parameter estimates.

Using the CQS (2005) quarterly data but applying the Campbell and Mankiw (1990) instrumenting technique, Calomiris *et al.* (2009) show that the CQS (2005) wealth effect estimates are substantially reduced. Thus, the increased size and statistical significance of housing wealth effects reported in CQS (2013) — in contrast to Calomiris *et al.* (2009) — seem to result from the addition of new data.

While the attempt to measure housing wealth at the state level is a major contribution of CQS (2005, 2013), their use of quarterly data to measure wealth effects may be problematic. If consumption takes longer than one quarter to fully respond to a change in housing wealth then their estimates will be biased, since, in the CQS specification, consumption must respond to a change in home prices *within* the same quarter.[2] Even if the regressors were lagged, which they are not, it is unlikely that the full effect of housing wealth would exert itself upon consumption in just one quarter. Indeed, Carroll *et al.* (2011) estimate housing wealth effects within a habit formation framework and point out that it could take several years for a change in wealth to fully exert its effect on consumption. Along these lines, Carroll and Zhou (2011) allow for a two-year window to capture the impact of wealth changes on consumer spending. To address this issue, we employ annual data in our study. Annual data also allow us to avoid excessive interpolation of stock wealth data (see the Data Appendix for a detailed discussion of this issue), and to employ other data that are only available at annual frequency — i.e., demographic variables that are likely to matter for the size of housing wealth effects, as discussed above.[3]

Our study is not the first to examine the demographic aspects of housing wealth effects. Campbell and Cocco (2007) employ micro data, and find that older homeowners (those over forty) exhibit greater wealth effects than those under forty. This finding is consistent with older homeowners being net long housing due to anticipated downsizing; however, the authors only divide their age groups into “old” and “young”, making no allowance for middle age. Attanasio *et al.* (2009) divide age groups into three categories: young (under 35), middle-aged (35–60) and old (over 60). They find that their estimated housing wealth effect is larger for the young than the old.

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2 CQS (2013) do employ an error-correction specification, which does permit dynamic adjustment, but this is not an appropriate alternative specification if cointegration is rejected, as it is in our dataset. We discuss this further below.

3 We recognize that our own annual contemporaneous modeling of the response of consumption to changes in income and wealth may not fully capture the long-run response of consumption to these changes. Adding lagged consumption growth to our panel estimation in the presence of state fixed effects, however, would yield inconsistent estimates. While there are techniques that yield consistent estimates for dynamic panels with fixed effects, they are unreliable in small samples like ours. Given that we regard state fixed effects as warranted, we choose to model only contemporaneous annual responses.

Since the young are not likely to be looking to trade down, and are more likely to include non-homeowners, the authors believe that the estimated wealth effect likely reflects omitted factors. In particular, consistent with Sinai and Souleles (2005), we would note that young people are most likely to suffer from credit constraints, and thus the impact of house prices on their consumption may well represent an effect of home values on consumer spending, at least in part.

The results of Campbell and Cocco (2007) and Attanasio *et al.* (2009) are promising, and point to important potential demographic influences. However, both restrict themselves to data for the United Kingdom. Contreras and Nichols (2010) examine a micro panel data set for the U.S., and include controls for demographics (they include the age of the household head and its square). Dividing the country into nine regions, they find that those areas with the most cyclical house price changes also typically display the highest housing wealth, and often exhibit a high estimated *elasticity* of consumption with respect to home values, as well as smaller ratios of consumption to housing wealth.

The dependence of the wealth effect on the ratio of housing wealth to total wealth is an important insight that is unique to our analysis. As discussed in Section I, in a standard PIH model, the impact of housing on consumption should depend on the relative importance of housing wealth, and on the size of total wealth (relative to consumption). One of the contributions of our study is the development of a model that explicitly allows housing and stock wealth effects to vary based on the fraction of total wealth they comprise.

In summary, the existing literature on consumption responses to changes in housing and securities wealth has pointed in several promising directions, which we pursue below: (1) panel estimation of wealth effects, as in CQS, can add statistical power by taking advantage of variation across states and across time; (2) endogeneity/omitted variable bias is a concern that can be addressed by instrumenting wealth and income, as in Campbell and Mankiw (1990); (3) functional forms for estimating housing and securities wealth effects on consumption should take the basic logic of the PIH into account, which requires that elasticities be allowed to vary with differences in the relative proportions of housing and securities wealth; and (4) differences within populations in the proportions of different age groups, and in the distribution of wealth (the incidence of poverty), are likely to be important in influencing the magnitude of measured wealth effects.

3 Data

In what follows, we provide a brief description of the data that we use in our analysis; note that a more detailed description of our data sources is provided in the Data Appendix. Following CQS (2005, 2013), we use retail sales as a proxy for consumption, using state-level estimates from 1977Q1 through 2010Q1 provided by Moody's Economy.com. The underlying data for retail sales at the state level are nominal, seasonally-adjusted annual rates (SAAR) at a quarterly frequency; our annual figures are the average of the quarterly SAAR values within each year.

Housing wealth is measured as the average value of owner-occupied housing times the number of owner-occupants within each state. The average value of owner-occupied housing each quarter is taken from the "Land Prices by State Dataset" developed by Davis and Heathcote (2007) and provided by the Lincoln Institute of Land Policy; we use fourth quarter figures as the value for the year. The number of owner-occupied households in each state each year is derived from the Annual Social and Economic (ASEC) Supplement to the Current Population Survey (CPS). A detailed description of how we have calculated these estimates is provided in the Data Appendix. Total nominal housing wealth in each state year is calculated as the average value of owner-occupied housing times the number of owner households.

Total U.S. stock wealth is calculated as the sum of corporate equities, mutual fund shares and pension fund reserves for households and non-profit corporations from the Federal Reserve Z1 statistical release; we use year-end (fourth quarter) values. We allocate that measure of aggregate annual U.S. stock wealth among states based on the estimated share of mutual fund holdings across states. Mutual fund share estimates for each state are available only for 1986, 1987, 1989, 1991, 1993, 1995, 2000, 2008 and 2009. For years prior to 1986, we have used the 1986 values; values for the remaining missing years of each state's share in total mutual fund share percentages (1988, 1990, 1992, 1994, 1996–1999, and 2001–2007) were interpolated linearly. Estimated nominal stock wealth in each state is then calculated as the aggregate U.S. stock wealth times each state's share of aggregate mutual fund holdings.

Other variables that are used in the analysis include real per-capita personal income from the Bureau of Economic Analysis (BEA), and annual population estimates by age group and poverty rates from the U.S. Census. We transform our consumption, income and all three wealth variables (housing wealth, stock wealth, and total wealth) into real, per-capita values by dividing by population and deflating using the GDP implicit price deflator. Unless otherwise stated, all regressions below are run on log differences of these real, per-capita values.

Our measures of housing and stock wealth differ from those of CQS (2013) in several ways.[4] CQS measure housing wealth using the Fiserv Case Shiller Weiss indices to capture quarterly changes in house values at the state level. Davis and Heathcote's measure of housing wealth uses actual 1980, 1990, and 2000 census figures for the average value of owner-occupied homes in those years, and, as discussed in the Data Appendix, only relies on the FHFA index to fill in intervening years.[5] In contrast, CQS use only the 2000 census year to benchmark their housing value estimates. With respect to stock wealth, CQS use a similar approach to ours, although they lack data for 1995 and 2000 on state-level mutual fund shares, which requires that they interpolate over the entire period from 1993 to 2008.

Unlike CQS, we rely on annual rather than quarterly data. The sample period is long enough for annual data to provide reasonably precise estimation of wealth effects, and we regard annual data as more reliable for several reasons.

First, given the limited number of observations about equity holdings and the consequent need to interpolate states' shares of mutual funds, we are less comfortable with constructing estimated quarterly observations for stock wealth. Quarterly interpolation is particularly problematic since the spotty data on mutual fund shares at the state level are not associated with a particular quarter within the year. Furthermore, forcing stock holdings to change smoothly over time while allowing housing wealth to vary quarterly may exaggerate the relative size of housing wealth effects, especially if the

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4 In the discussion that follows, we mainly compare our data with CQS (2013). Differences with Carroll and Zhou (2011) are more substantial and reflect the limited availability of state-level data on securities wealth and consumption.

5 Both the FHFA and the Fiserv Case Shiller Weiss indices are based on comparisons over time of transactions involving the same house, in contrast to hedonic pricing models that attempt to control for house characteristics. These same-sales indexes, however, can suffer from selectivity bias relating to the timing of particular types of house sales. For example, during the 2007–2009 period, housing sales include a large proportion of distressed home sales (foreclosures and the like), and observed values of the indexes may provide an exaggerated picture of housing price decline. Indeed, Leventis (2009) provides evidence that this is the case. One could make a similar argument that during the subprime housing boom of 2004–2006, transactions gave an unrepresentative and exaggerated picture of housing price increases.

two kinds of wealth are positively correlated. While this problem remains with our annual data, it should be less pronounced than it is with quarterly interpolation.

Second, the use of annual data avoids having to take a position on the appropriate means of adjusting for seasonality in personal income and house prices; adjusting for seasonality is especially challenging given the potential for differences in seasonal patterns across states with very different weather patterns and ages of structures.

Finally, our population, age composition, and poverty estimates are only available at an annual frequency.

4 Empirical Analysis

4.1 Variation Across States and Over Time in Wealth and Population Composition

Table 1 reports summary statistics for the variables used in this study, pooling data across states and over time. As discussed above, our study emphasizes how variation in age groups, poverty incidence, and the proportion of wealth in housing can affect the estimation of consumption wealth effects for housing and stock. Table 2 shows how our demographic variables vary across states. The states with the smallest and largest average proportions of young adults are West Virginia (27.6 percent) and Utah (39.4 percent). Alaska has the largest percentage of middle-aged people (45.2 percent), while Florida has the lowest percentage (35.1 percent). Alaska is home to the smallest proportion of old (18.7 percent), while the state with the highest proportion of old, Florida, had twice as many (37.3 percent). Mississippi has the largest average poverty rate (21.2 percent), while New Hampshire's poverty rate is the lowest (6.7 percent).

Figure 1 shows how the age distribution has changed over time for a sample of eleven states, and for the U.S. as a whole. The percent of the adult population that we define as *young* (ages 20–34) is plotted on the x axis, while the percent of the adult population that is *old* (ages 55+) is plotted on the y axis. Clearly, despite the differences in average population composition across states, states followed a similar within-state pattern over time. The proportion of young people declined steadily from 1985 to about 2000 while the proportion of old remained roughly constant. After 2000, the proportion of young people was roughly constant while the proportion

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Consumption	1,275	11,997	2,186	6,887	20,973
Income	1,275	29,550	6,544	15,877	63,053
Housing Wealth	1,275	45,348	21,778	17,173	170,507
Stock Wealth	1,275	56,169	24,989	7,496	120,102
Total Wealth	1,275	101,517	41,570	28,317	260,588
Housing Wealth Percent	1,275	0.457	0.103	0.242	0.735
Stock Wealth Percent	1,275	0.543	0.103	0.265	0.758
Percent Young (Ages 20–34)	1,275	0.312	0.041	0.229	0.478
Percent Middle Age (Ages 35–54)	1,275	0.384	0.034	0.292	0.499
Percent Old (Ages 55+)	1,275	0.304	0.033	0.135	0.386
Poverty Rate	1,275	0.127	0.038	0.029	0.272
Log Difference of					
Consumption	1,275	0.012	0.033	−0.122	0.156
Income	1,275	0.019	0.022	−0.108	0.096
Housing Wealth	1,275	0.029	0.061	−0.372	0.259
Stock Wealth	1,275	0.056	0.152	−0.423	0.429
Total Wealth	1,275	0.041	0.094	−0.364	0.265

Table 1. Summary statistics.

Notes: Consumption, income and wealth variables are expressed in real, per-capita terms. Data are presented for the years 1985–2009 for all U.S. states and the District of Columbia; the years 1981–1984 are excluded from the analysis because of lags used for instrumenting.

of old people rose steadily. This pattern reflects the effects of the post-World War II baby boom on population composition.

Figure 2 shows the variation in the poverty rate over time for each state. States are arrayed on the x axis, with each dot representing one year's value for the poverty rate for that state. From this figure it is clear that there is as much or more variation in the poverty rate over time within states as there is across states.

Similarly, Figure 3 plots the ratio of housing wealth to total wealth for each state over time. As with the poverty rate, this figure shows variation in the average ratio of housing wealth across states as well as over time

State	Percent Ages 20–34	Percent Ages 35–54	Percent Ages 55+	Poverty Rate	State	Percent Ages 20–34	Percent Ages 35–54	Percent Ages 55+	Poverty Rate	State	Percent Ages 20–34	Percent Ages 35–54	Percent Ages 55+	Poverty Rate
AK	36.1	45.2	18.7	9.5	KY	31.0	38.2	30.8	16.4	NY	30.8	38.2	31.0	14.9
AL	30.8	37.5	31.7	17.0	LA	32.6	38.0	29.3	19.8	OH	30.1	38.3	31.6	12.0
AR	29.6	36.5	33.9	17.7	MA	31.3	37.9	30.8	10.2	OK	30.7	37.1	32.2	15.2
AZ	32.4	36.6	31.0	15.0	MD	31.4	40.5	28.1	9.0	OR	29.4	39.2	31.4	11.9
CA	34.7	38.6	26.7	14.4	ME	28.0	39.4	32.6	11.6	PA	28.2	37.3	34.5	10.9
CO	32.8	41.2	26.0	10.5	MI	30.8	39.0	30.2	12.4	RI	30.5	36.9	32.5	10.5
CT	29.0	39.3	31.7	8.1	MN	31.3	39.1	29.6	9.9	SC	32.0	38.1	30.0	14.9
DC	36.2	35.5	28.3	18.8	MO	30.1	37.6	32.3	12.5	SD	30.0	36.4	33.6	13.0
DE	31.5	37.9	30.6	9.2	MS	32.2	36.9	30.9	21.2	TN	30.8	38.4	30.7	15.7
FL	27.6	35.1	37.3	13.4	MT	28.0	39.4	32.7	14.8	TX	34.7	38.8	26.5	16.7
GA	34.1	39.9	26.0	14.3	NC	32.0	38.1	29.9	13.8	UT	39.4	35.9	24.6	9.3
HI	32.0	38.0	30.0	10.2	ND	31.3	35.9	32.8	12.0	VA	32.7	39.5	27.7	9.9
IA	29.1	36.8	34.1	10.4	NE	30.5	37.3	32.2	10.6	VT	29.5	40.3	30.2	9.5
ID	31.4	38.6	30.0	12.9	NH	30.1	41.0	28.9	6.7	WA	31.5	40.0	28.5	10.6
IL	31.9	38.3	29.8	12.4	NJ	29.3	39.4	31.3	8.6	WI	30.4	38.4	31.2	9.7
IN	31.2	38.3	30.5	11.2	NM	31.7	38.8	29.6	19.8	WV	27.6	37.4	35.0	17.6
KS	31.0	37.5	31.5	11.3	NV	32.2	39.1	28.6	10.5	WY	30.3	40.3	29.4	10.8

Table 2. Average demographic characteristics by state.

Notes: Data are averaged over the years 1985–2009 for all U.S. states and the District of Columbia; the years 1981–1984 are excluded from the analysis because of lags used for instrumenting.

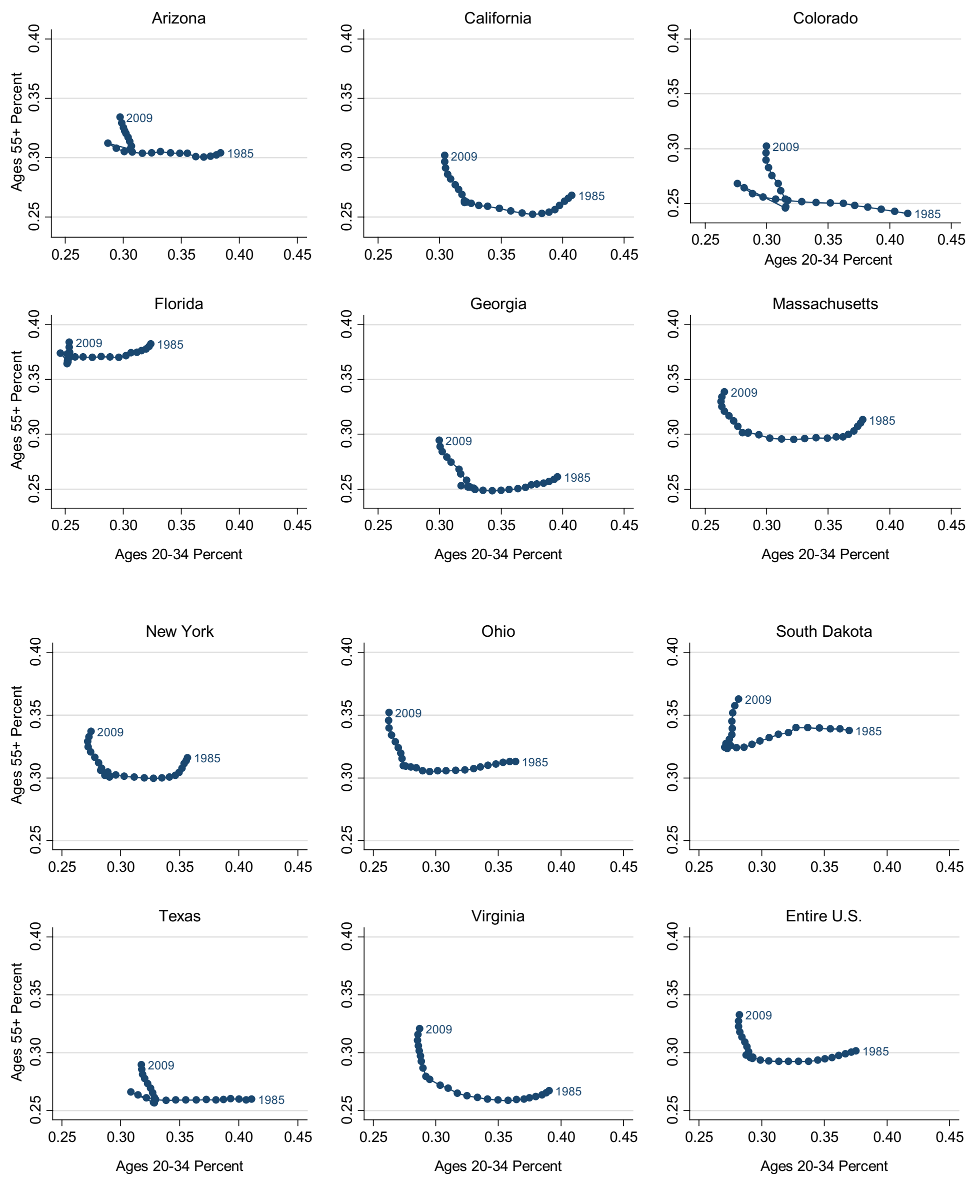

Figure 1. Changes in old and young population ratios in selected states.

Notes: Figure shows the percent of the adult population ages 25–34 and ages 55+ in each year for selected states and the United States. Observations for 1985 and 2009 are labeled and consecutive years are connected.

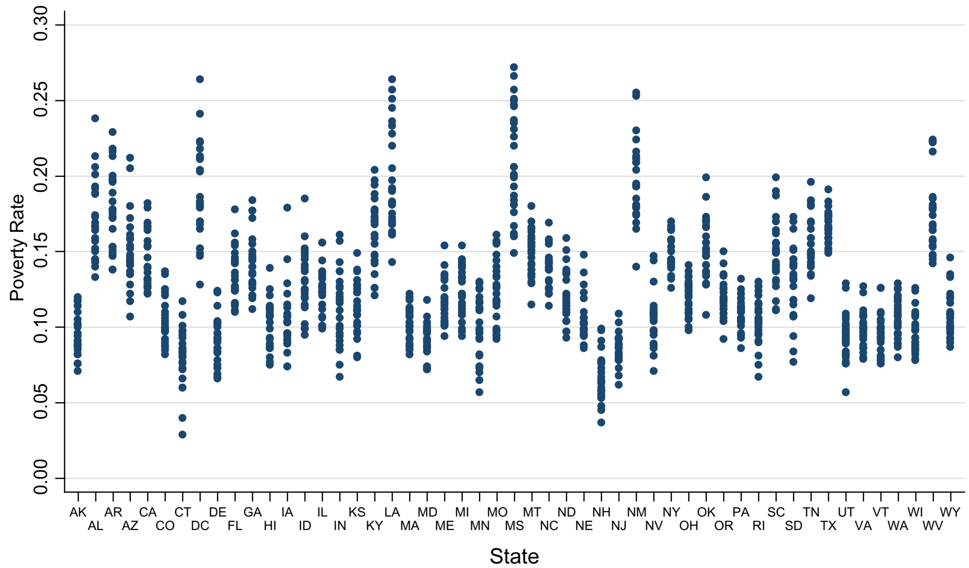

Figure 2. Poverty rates across time by state.

Notes: Figure shows the poverty rate in each year of the analysis for each state. Data are presented for the years 1985–2009; the years 1981–1984 are excluded from the analysis because of lags used for instrumenting.

within states. For example, Nebraska displays a low average proportion of housing wealth, and a relatively small amount of variation over time in the housing wealth ratio. Hawaii displays a high average proportion of housing wealth, and a relatively small amount of variation around that mean. The average ratios of other states — Louisiana, Mississippi, and West Virginia, for example — are closer to the national mean and show much greater variation over time.

Figure 4 shows that this variation over time in the proportion of housing wealth follows a similar pattern across the various states, although some states display more pronounced variation over time than others. The housing wealth ratio declined from 1985 to 2000, then rose during the early 2000s, and fell again during the post-2006 subprime crisis.

4.2 Calculating Wealth Effects

Our full regression model allows the estimated consumption elasticities of housing and stock wealth to vary as a function of the relative size of housing

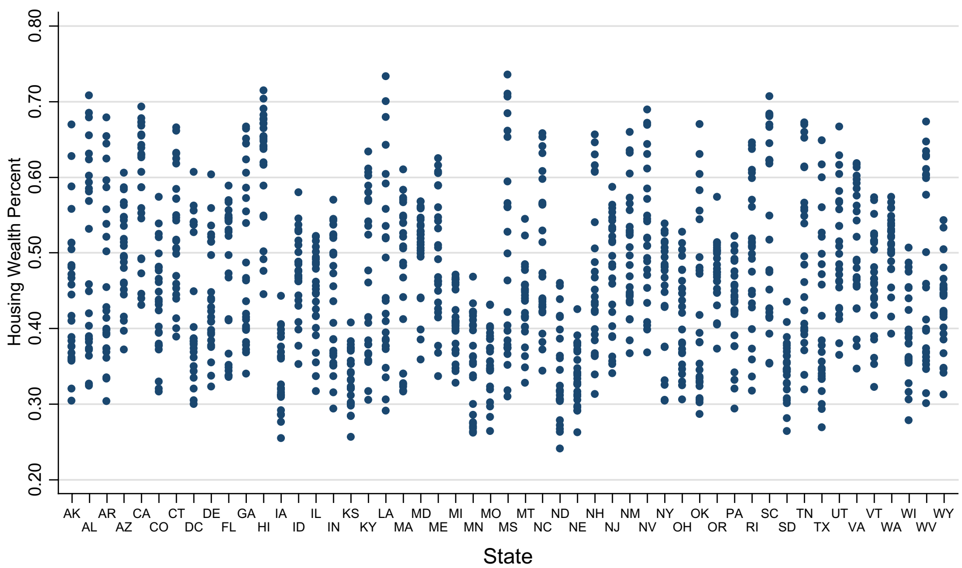

Figure 3. Housing wealth/total wealth across time by state.

Notes: Figure shows fraction of total wealth comprised by housing wealth in each year of the analysis for each state. Data are presented for the years 1985–2009; the years 1981–1984 are excluded from the analysis because of lags used for instrumenting.

and stock wealth. We do this by including the log difference of total wealth in the model. As we show below, this specification allows the housing and stock wealth elasticities to vary based on their shares of total wealth. In addition, our model includes interaction effects between the wealth variables and the demographic variables. Our full regression specification can be written as:

$$
\begin{aligned}
\Delta \ln c_{st} = {} & \beta_0 + \beta_h \Delta \ln h_{st} + \beta_s \Delta \ln s_{st} + \beta_w \Delta \ln w_{st} + \beta_i \Delta \ln i_{st} + \beta_y Y_{st} \\
& + \beta_o O_{st} + \beta_p P_{st} + \beta_{yh} Y_{st} \times \Delta \ln h_{st} + \beta_{ys} Y_{st} \times \Delta \ln s_{st} \\
& + \beta_{yw} Y_{st} \times \Delta \ln w_{st} + \beta_{oh} O_{st} \times \Delta \ln h_{st} + \beta_{os} O_{st} \times \Delta \ln s_{st} \\
& + \beta_{ow} O_{st} \times \Delta \ln w_{st} + \beta_{ph} P_{st} \times \Delta \ln h_{st} + \beta_{ps} P_{st} \times \Delta \ln s_{st} \\
& + \beta_{pw} P_{st} \times \Delta \ln w_{st} + \epsilon_{st},
\end{aligned}
$$

where c_{st} is real, per-capita consumption in state s at time t; h_{st} is real, per-capita housing wealth in state s at time t; s_{st} is real, per-capita stock wealth in state s at time t; w_{st} is real, per capital total wealth in state s at time t; i_{st}

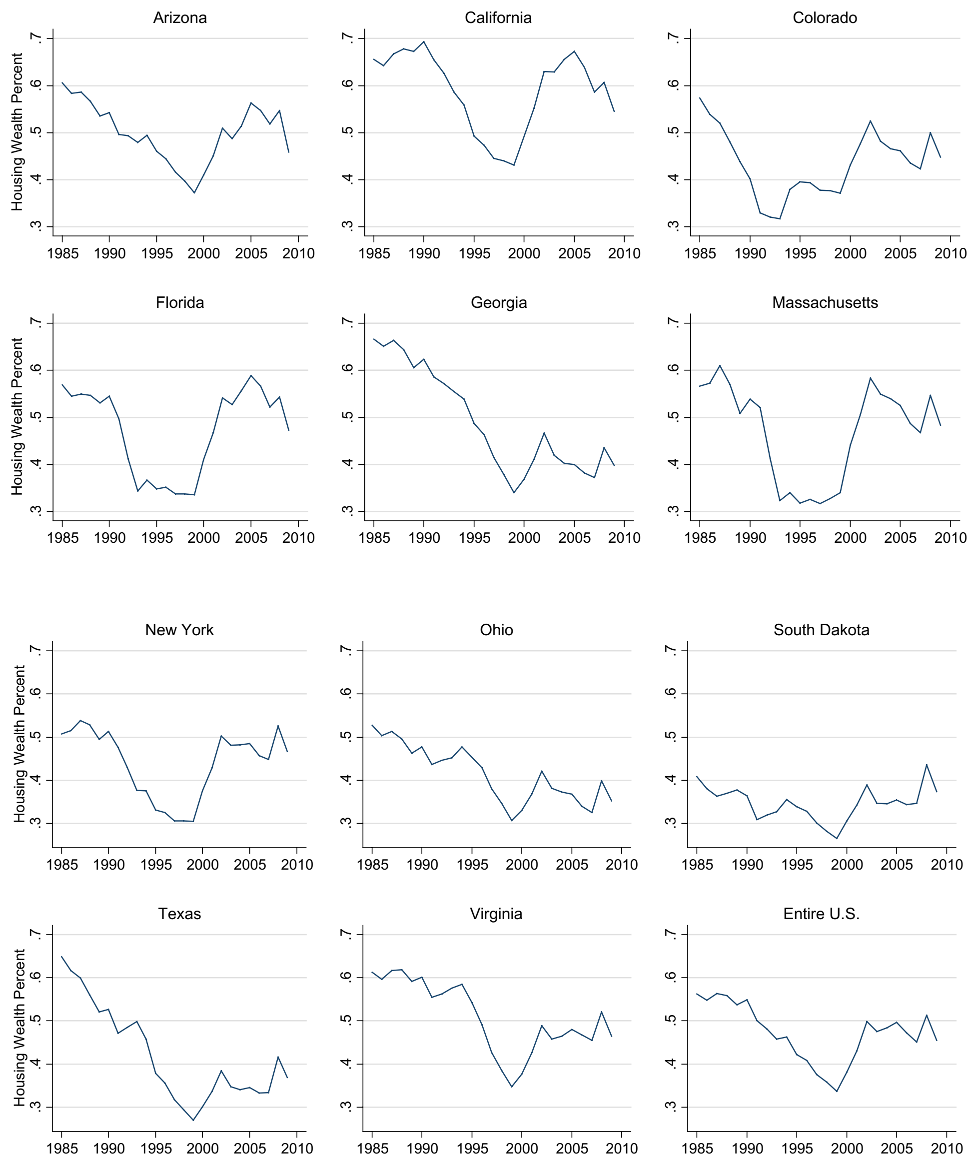

Figure 4. Housing wealth/total wealth in selected states.

Notes: Figure shows fraction of total wealth comprised by housing wealth over time for selected states and the United States as a whole.

is real, per-capita personal income in state s at time t; Y_{st} is the percent of the adult population aged 20–34 in state s at time t; and O_{st} is the percent of the adult population aged 55+ in state s at time t; and P_{st} is the poverty rate in state s at time t.

Noting that $w_{st} = h_{st} + s_{st}$ and $\Delta \ln(x_{st}) = \ln(x_{st}) - \ln(x_{st-1})$, the impact of a one dollar change in housing wealth is calculated as:

$$\begin{aligned}
dc\frac{1}{c} &= dh\left[\beta_h \frac{1}{h} + \beta_w \frac{1}{w} + \beta_{yh} Y \frac{1}{h} + \beta_{yw} Y \frac{1}{w} + \beta_{oh} O \frac{1}{h} + \beta_{ph} P \frac{1}{h}\right. \\
&\quad \left. + \beta_{ow} O \frac{1}{w} + \beta_{pw} P \frac{1}{w}\right] \\
&\Rightarrow \mathrm{HWE} \equiv \frac{dc}{dh} = \frac{\bar{c}}{\bar{h}}\left[\beta_h + \beta_{yh}\bar{Y} + \beta_{oh}\bar{O} + \beta_{ph}\bar{P}\right. \\
&\quad \left. + (\beta_w + \beta_{yw}\bar{Y} + \beta_{ow}\bar{O} + \beta_{pw}\bar{P})\frac{\bar{h}}{\bar{w}}\right], \qquad (1)
\end{aligned}$$

where bars denote sample mean values of the variable or ratio in question. We will sometimes refer to $\frac{dc}{dh}$ as the housing wealth effect (HWE), and to the analogous derivative of consumption with respect to stock wealth ($\frac{dc}{ds}$) as the stock wealth effect (SWE). The consumption elasticity of housing wealth is therefore simply

$$\begin{aligned}
\varepsilon_h &= \frac{dc/\bar{c}}{dh/\bar{h}} = \beta_h + \beta_{yh}\bar{Y} + \beta_{oh}\bar{O} + \beta_{ph}\bar{P} \\
&\quad + (\beta_w + \beta_{yw}\bar{Y} + \beta_{ow}\bar{O} + \beta_{pw}\bar{P})\frac{\bar{h}}{\bar{w}}; \qquad (2)
\end{aligned}$$

stock wealth effects and elasticities are calculated analogously.

Notice that in this specification, the consumption elasticities of housing and stock wealth explicitly depend on the shares of total wealth. To see this, consider a simplified version of the model that does not include demographic variables. In this case, the consumption elasticity of housing wealth simplifies to $\varepsilon_h = \beta_h + \beta_w \frac{\bar{h}}{\bar{w}}$. In other words, the consumption elasticity of housing wealth is not constant in this model but rather depends directly on the ratio of housing wealth to total wealth.

In addition to average (sample mean) housing wealth effects and elasticities, we can also calculate predicted values for each state-year observation:

$$\begin{aligned}\mathrm{HWE}_{st} \equiv \frac{dc_{st}}{dh_{st}} = \frac{c_{st}}{h_{st}}\Bigg[&\beta_h + \beta_{yh}Y_{st} + \beta_{oh}O_{st} + \beta_{ph}P_{st}\\ &+ (\beta_w + \beta_{yw}Y_{st} + \beta_{ow}O_{st} + \beta_{pw}P_{st})\frac{h_{st}}{w_{st}}\Bigg]\end{aligned} \tag{3}$$

and

$$\begin{aligned}\varepsilon_{h_{st}} = &\beta_h + \beta_{yh}Y_{st} + \beta_{oh}O_{st} + \beta_{ph}P_{st}\\ &+ (\beta_w + \beta_{yw}Y_{st} + \beta_{ow}O_{st} + \beta_{pw}P_{st})\frac{h_{st}}{w_{st}}.\end{aligned} \tag{4}$$

Calculating predicted housing and stock wealth effects allows us to map how these effects have changed over time due to changes in demographics and wealth ratios.

The derivatives of the housing wealth effect with respect to Y, O, and P, are simply

$$\frac{d\mathrm{HWE}}{dY} = \frac{dc^2}{dhdY} = \frac{\bar{c}}{\bar{h}}\left[\beta_{yh} + \beta_{yw}\frac{\bar{h}}{\bar{w}}\right], \tag{5}$$

$$\frac{d\mathrm{HWE}}{dO} = \frac{dc^2}{dhdO} = \frac{\bar{c}}{\bar{h}}\left[\beta_{oh} + \beta_{ow}\frac{\bar{h}}{\bar{w}}\right], \tag{6}$$

and

$$\frac{d\mathrm{HWE}}{dP} = \frac{dc^2}{dhdP} = \frac{\bar{c}}{\bar{h}}\left[\beta_{ph} + \beta_{pw}\frac{\bar{h}}{\bar{w}}\right]. \tag{7}$$

We hypothesize that all three of these derivatives should be positive. A higher proportion of young people or people with low wealth should be associated with more binding borrowing constraints, which should raise the wealth effect. Similarly, a larger proportion of older people (for whom downsizing of housing consumption is more likely) should also produce a larger wealth effect. Note that our model specification also implies that $\frac{dc_{st}}{dh_{st}}$ is higher when housing wealth (h_{st}) is lower, *ceteris paribus*, because h_{st} only appears in the denominator of expression (3) above.

For comparison purposes, we present four additional specifications that do not include all the effects modeled above. All estimations are specified as log differences to satisfy stationarity requirements, and follow the Campbell and Mankiw (1990) instrumenting procedure, as in Calomiris, *et al.* (2009). In addition, all of our regressions control for state fixed effects.[6] Presumably, these fixed effects capture average differences across states in expected future income growth, human capital, and other omitted factors that influence consumption growth rates. We do not include time effects since much of the annual variation in wealth (especially in stock wealth) reflects common factors that affect all the states (e.g., the stock market). Standard errors are clustered by state. Despite some minor differences, results are quite similar across all these specifications, as we discuss further below.

In a supplementary appendix, we also report results from OLS log difference regressions, for comparison purposes.[7] We do not report error-correction model results since the variables in our model do not appear to be cointegrated, as discussed in the following brief digression.

4.3 Is an Error-Correction Model Warranted?

Some authors (e.g., CQS, 2005, 2013) estimate error-correction models of housing wealth effects. This approach, however, has drawn criticism. Carroll *et al.* (2011) argue that changes in interest or growth rates should change the relationships among other variables (e.g., consumption, income, and wealth), thus eliminating a stable cointegrating vector among those variables. If the cointegrating vector is not stable, according to the well-known Granger representation theorem, an error correction model would not make sense. Carroll *et al.* go on to point out that even if a cointegrating vector does exist, changes in any other variables that are relevant for consumption decisions might have such long-lived dynamics that "hundreds or thousands of years of data" might be necessary for good estimates (Carroll *et al.*, p. 55).

We have tested for the possibility of cointegration among all four variables in our system (consumption, income, housing wealth, and stock wealth) by utilizing the panel cointegration test of Westerlund (2007). A traditional challenge in testing for cointegration is the lack of power in traditional methods such as the Johansen-Juselius technique, which posits the null hypothesis as a lack of cointegration; a lack of power means that one will

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6 The state fixed effects coefficients for our full specification (Model 5) are reported in Appendix Table A1.

7 Supplemental appendices can be found at http://realestate.wichita.edu/data-research/academic-research/.

often conclude that the variables in question are not cointegrated when, in fact, there could be a stationary long-run relationship among them.

Fortunately, however, we are utilizing a panel dataset. The larger panel dataset increases the power of the test, just as panel unit root tests increase the power of testing for nonstationarity in a single series. Some early panel cointegration tests suffered from low power, which arose from imposing restrictions, such as requiring the long-run parameters to be equal to the short run responses in differences (see Westerlund, 2007) or not allowing for cross-sectional dependence. Note that allowing for cross-sectional dependence is vital in our study, as there are clearly common shocks to income, stock, and housing wealth across states.

Westerlund (2007) has developed a test for panel cointegration which does not impose such restrictions and has been demonstrated in simulations to have greater power than existing panel cointegration tests. By applying this test, we are choosing a technique with a high probability of finding a cointegrating relationship if one exists.

In particular, the Westerlund technique tests for the significance of the error-correction, or speed-of-adjustment term. Consider a simple model where y is a variable and x is a vector of variables:

$$\Delta y_{it} = \alpha_i (y_{i,t-1} - \beta_i' x_{i,t-1}) + \sum \alpha_{ij} \Delta y_{i,t-j} + \sum \delta_{ij} \Delta x_{i,t-j} + e_{it}$$

Here α_i is the error correction term, and $y_{i,t-1} - \beta_i' x_{i,t-1}$ is the cointegrating vector. Again, according to the Granger representation theorem, if the variables are cointegrated, the model has an error correction representation as shown in the above expression. The Westerlund technique thus tests for the significance of α_i; if it is significant, then the variables are cointegrated.

When allowing for a trend, cross-sectional dependence, and differing speed of adjustment coefficients across the four variables, we were unable to reject the null hypothesis of no cointegration. Specifically, the Westerlund test statistic was −2.792, implying a p-value of 0.235. This suggests that it would not be appropriate to model wealth effects using an error-correction model.

4.4 Estimation Results

Our regression results are presented in Table 3. Model 1 is a traditional specification including only income, housing wealth, and stock wealth. Model 2 includes total wealth, allowing housing and stock wealth elasticities to

	Model 1	Model 2	Model 3	Model 4	Model 5
Income	0.878***	0.954***	0.636***	0.548***	0.562***
	(0.077)	(0.074)	(0.080)	(0.068)	(0.070)
Housing Wealth	0.183***	−0.019	−0.345	−6.456***	−8.194***
	(0.026)	(0.087)	(0.495)	(1.635)	(2.157)
Stock Wealth	0.058***	−0.150	0.949***	−7.381***	−8.556***
	(0.017)	(0.095)	(0.276)	(1.513)	(2.110)
Total Wealth		0.398**		13.872***	16.501***
		(0.175)		(3.001)	(4.007)
Young Percent			0.017	−0.016	0.016
			(0.078)	(0.073)	(0.080)
Old Percent			−0.271***	−0.516***	−0.454***
			(0.084)	(0.073)	(0.092)
Poverty Rate			0.096		0.128*
			(0.089)		(0.075)
Young Percent ×			0.634	8.457***	12.820***
Housing Wealth			(0.766)	(2.984)	(3.961)
Old Percent ×			1.044	11.962***	15.260***
Housing Wealth			(1.050)	(2.653)	(3.660)
Poverty Rate ×			0.631		−4.000
Housing Wealth			(1.128)		(2.682)
Young Percent ×			−1.039*	10.217***	13.511***
Stock Wealth			(0.607)	(2.512)	(3.606)
Old Percent ×			−2.279***	12.224***	15.215***
Stock Wealth			(0.632)	(2.559)	(3.743)
Poverty Rate ×			0.766		−5.507**
Stock Wealth			(0.864)		(2.593)
Young Percent ×				−18.790***	−26.431***
Total Wealth				(5.489)	(7.432)
Old Percent ×				−23.442***	−29.430***
Total Wealth				(4.800)	(6.682)
Poverty Rate ×					11.189**
Total Wealth					(5.559)
Constant	−0.011***	−0.010***	0.015	0.102***	0.069
	(0.001)	(0.001)	(0.044)	(0.038)	(0.044)
Observations	1,275	1,275	1,275	1,275	1,275
Wald Chi-square	388.74***	345.77***	752.62***	808.27***	1, 052.50***
Degrees of freedom	53	54	62	62	66

Notes: The dependent variable is log difference of real, per capita consumption (where consumption is proxied by state-level retail sales). Wealth variables are expressed in log differences of real, per capita values. Young Percent is the percent of the adult population ages 20–34; Old Percent is the percentage of the adult population ages 55 and up. Standard errors (clustered by state) are shown in parentheses below the estimates. The Wald Chi-square statistic tests for the joint significance of all of the coefficients except the constant term. All wealth and interaction variables are instrumented using the 2nd–4th lags of these variables.
***Coefficient significant at the 1% level.
**Coefficient significant at the 5% level.
*Coefficient significant at the 10% level.

Table 3. Panel data wealth effect regressions.

vary based on their proportions of total wealth. Model 3 adds age and poverty demographics to the model but does not allow elasticities to vary with wealth shares. Model 4 includes age demographics and wealth shares effects while Model 5 is the full specification including age demographics, the poverty rate, and wealth shares effects.

Based on the regression results reported in Table 3, Table 4 shows the implied average housing wealth effects (HWE), average stock wealth effects

	Model 1	Model 2	Model 3	Model 4	Model 5
Housing Wealth Effect (HWE)	0.055***	0.049***	0.075***	0.067***	0.081***
Stock Wealth Effect (SWE)	0.016***	0.018***	0.008*	0.000	−0.005
Difference	0.039***	0.031***	0.067***	0.066***	0.086***
Housing Wealth Elasticity	0.183***	0.163***	0.250***	0.222***	0.270***
Stock Wealth Elasticity	0.058***	0.066***	0.030*	0.002	−0.019
Difference	0.124***	0.097***	0.221***	0.220***	0.288***
Wealth Effect Derivatives					
d HWE/d Young Percent			0.191	−0.039	0.223
d HWE/d Old Percent			0.314	0.376	0.545
d HWE/d Poverty Rate			0.190		0.335
d SWE/d Young Percent			−0.277	0.003	−0.224
d SWE/d Old Percent			−0.607	−0.135	−0.204
d SWE/d Poverty Rate			0.204		0.152

Notes: Housing and stock wealth effects are expressed in dollar terms and calculated at the sample mean values for all variables. Housing and stock wealth elasticities and wealth effect derivatives are calculated at sample means for all variables as well. Standard errors (clustered by state) are shown in parentheses below the estimates.
***Estimated value significant at the 1% level.
**Estimated value significant at the 5% level.
*Estimated value significant at the 10% level.

Table 4. Estimated wealth effects, elasticities and derivatives.

(SWE), average elasticities of consumption with respect to housing and stock wealth, and the derivatives of HWE and SWE with respect to age composition and poverty rates, for each of the five models. Recall that HWE and SWE measure the effects on consumption of a $1 increase in either housing wealth or stock wealth. Using Model 5, a $1 increase in housing wealth raises contemporaneous consumption by roughly $0.08 on average. In contrast, the effect of a $1 increase in stock wealth on consumption is zero (although in the non-preferred specifications of Models 1 and 2, the average stock wealth effect is just less than $0.02).

As hypothesized above, in our preferred Model 5, the implied derivatives of HWE with respect to Y, O, and P are all positive. That is, higher proportions of young people and old people, and a higher poverty rate all act to raise the housing wealth effect for a state-year. In contrast, the estimated derivatives of SWE with respect to Y and O are negative. It is worth noting, however, that the overall stock wealth effect is insignificantly different from zero, making the implied derivatives less relevant. The insignificant estimated SWE reflects the offsetting influences of seven statistically significant coefficients from Model 5 in Table 3. In other words, the net effect of combining several statistically significant influences is an overall stock wealth effect that is not measurably different from zero.

Figure 5 plots the pattern of average estimated wealth effects over time (averaging across states within each year) for our various specifications, with confidence intervals estimated under the restrictive assumption that within-year covariances of HWEs and SWEs across states are zero.[8] In Models 4 and 5, which include both age demographics and wealth ratios, stock wealth effects are relatively high during the stock market boom of the 1990s when the proportion of stock wealth was relatively high; housing wealth effects fell sharply during this period. Over time, however, average housing wealth effects have generally been declining. The differences in the implied time variation of wealth effects for the different model specifications have interesting implications for understanding the factors that drive variation in housing and stock wealth effects across different time periods. Models 2 and 3, which take into account only age variation or

8 In principle, each of the state's HWE and SWE observations in a given year has an error component, but this can only be calculated for a given assumption of the covariances among the states' HWEs (or SWEs) within each year. By making a particular assumption — here, that covariance is zero — we are able to calculate the standard error in each year. If one assumed positive covariances among states, confidence bands would widen accordingly.

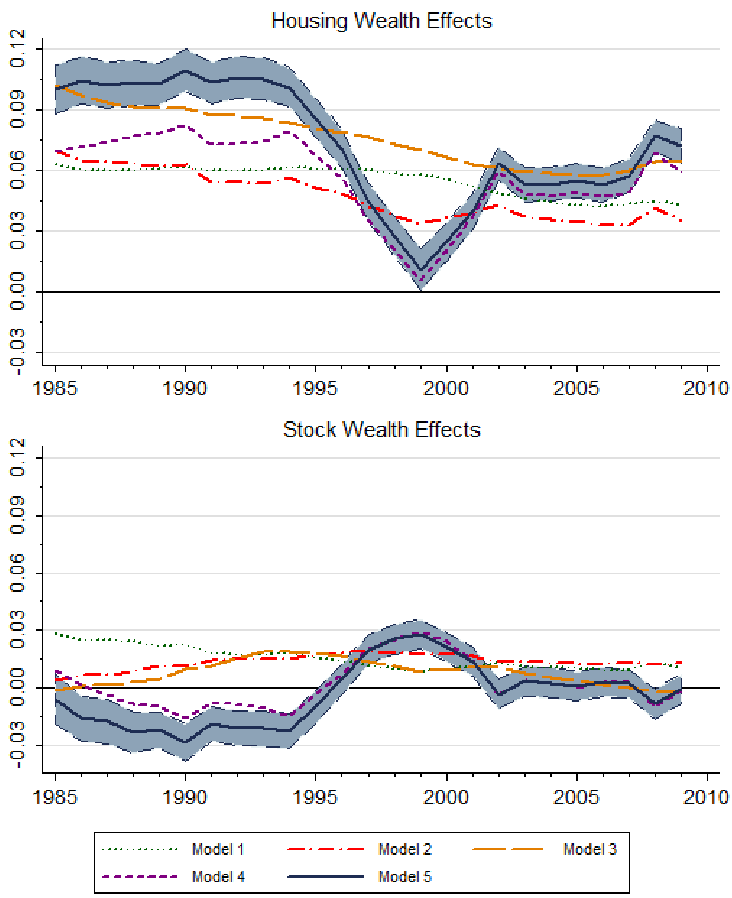

Figure 5. Average wealth effects over time ($\rho = 0$).

Notes: The time path of the average housing and stock wealth effects are shown for each of the five models presented in Table 3 (each year's value is the average across states). Model 1 is a traditional constant elasticity framework. Model 2 allows housing and stock wealth elasticities to vary based on the composition of total wealth. Model 3 includes demographic effects (age and poverty rates) but not wealth compositions. Model 4 includes both age demographics and wealth compositions but not poverty rates. Model 5 includes all demographic wealth composition effects; 95 percent error bands are calculated assuming zero cross-state correlation among wealth effects within a given year.

wealth composition (but not both simultaneously, as in Models 4 and 5), exhibit much smaller swings in wealth effects over time. Demographic and wealth compositional effects, therefore, obviously are correlated, since Model 5's time path is not a simple aggregation of the influences of Models 2 and 3 (wealth ratios and demographics). In addition to plotting Figure 5 based on simple averages across states, we also examined alternative versions of Figure 5 (available in a supplemental appendix) that weight states by consumption, total wealth, or population; all of these versions of Figure 5 appear to be virtually identical to the non-weighted version reported here.

As hypothesized, poverty rate interactions are statistically significant and the derivative of the housing wealth effect with respect to poverty is positive (Table 4). We interpret this as evidence that states with higher poverty also tend to experience more binding borrowing constraints on permanent income, which tends to strengthen the housing wealth effect. Figure 5 shows that the inclusion of poverty rates does not materially affect the patterns of time variation in the size of the two wealth effects once age effects are included, although it does increase the magnitude of the average estimated housing wealth effect. In other words, the time patterns of the wealth effects are qualitatively similar across Model 4 (without poverty rates) and Model 5 (with poverty rates).

The inclusion of poverty rates affects the correlations between wealth effects and total wealth. The top part of Figure 6 plots the relationship between total wealth and the housing and stock wealth effects under the Model 4 specification (which does not include poverty rates). As implied by our specifications, both of the estimated wealth effects decline as a function of wealth. When poverty is included in the model, however, (as shown in the bottom half of Figure 6) the association between estimated housing wealth elasticities and total wealth becomes more pronounced, while the association between estimated stock wealth elasticities and wealth becomes less pronounced. This reflects the fact that when poverty rates (which are strongly negatively correlated with real, per capita total wealth) are included in the specification, the housing wealth effect is larger for states with higher poverty rates.

In results that are not reported here, we explored whether the unemployment rate might serve as a better measure of wealth distribution than the poverty rate. That is, we re-ran the specifications reported in Table 3 using unemployment instead of poverty for the regressions in columns (3) and (5). Coefficients on unemployment interactions with wealth measures were less statistically significant. The housing and stock wealth effects, elasticities, and derivatives from these regressions were essentially unchanged from those reported in Table 4. Overall, we conclude from this analysis that

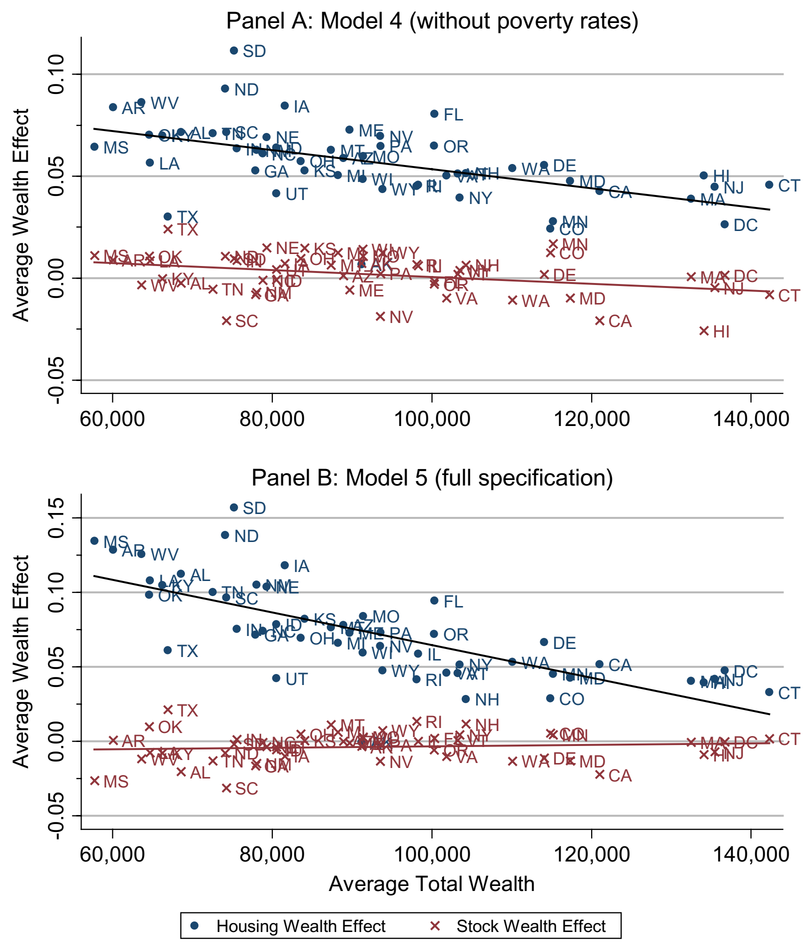

Figure 6. Relationship between wealth effects and total wealth.

Notes: Figure shows the relationship between each state's average housing and stock wealth effects and average total wealth within that state (averaged across over the years of the analysis, 1985–2009, within each state). Panel A calculates the average housing and stock wealth effects using the parameter estimates from Model 4, which does not include the poverty rate. Panel B calculates the wealth effects using the parameter estimates from Model 5 (the full specification).

unemployment is a somewhat noisier proxy than poverty rates for the distribution of wealth.

Table 5 reports state-level averages (sorted by the size of the housing wealth effect) of the housing wealth effect, the stock wealth effect, and the

State	HWE	SWE	Cons./ HW	Cons./ SW	Young Percent	Old Percent	Poverty Rate	HW/ TW	SW/ TW	Total Wealth
SD	0.157	−0.002	0.548	0.288	0.300	0.336	0.130	0.347	0.653	82,818
ND	0.138	−0.008	0.505	0.266	0.313	0.328	0.120	0.339	0.661	81,647
MS	0.135	−0.027	0.340	0.404	0.322	0.309	0.212	0.495	0.505	64,275
AR	0.129	0.001	0.371	0.368	0.296	0.339	0.177	0.466	0.534	66,822
WV	0.126	−0.012	0.337	0.360	0.276	0.350	0.176	0.479	0.521	70,626
IA	0.118	−0.010	0.405	0.215	0.291	0.341	0.104	0.340	0.660	90,098
AL	0.112	−0.020	0.322	0.383	0.308	0.317	0.170	0.499	0.501	76,693
LA	0.108	−0.008	0.346	0.362	0.326	0.293	0.198	0.474	0.526	70,930
KY	0.105	−0.008	0.350	0.351	0.310	0.308	0.164	0.470	0.530	73,702
NM	0.105	−0.015	0.278	0.302	0.317	0.296	0.198	0.501	0.499	85,708
NE	0.104	−0.004	0.432	0.222	0.305	0.322	0.106	0.337	0.663	87,016
TN	0.100	−0.013	0.327	0.365	0.308	0.307	0.157	0.494	0.506	80,978
OK	0.098	0.010	0.375	0.310	0.307	0.322	0.152	0.426	0.574	70,442
SC	0.096	−0.031	0.292	0.402	0.320	0.300	0.149	0.532	0.468	83,130
FL	0.094	0.002	0.279	0.253	0.276	0.373	0.134	0.473	0.527	109,229
MO	0.084	0.003	0.348	0.196	0.301	0.323	0.125	0.358	0.642	100,264
KS	0.082	0.000	0.372	0.188	0.310	0.315	0.113	0.333	0.667	91,326
ID	0.079	−0.006	0.299	0.277	0.314	0.300	0.129	0.474	0.526	88,820
AZ	0.078	0.000	0.269	0.272	0.324	0.310	0.150	0.499	0.501	96,209
MT	0.076	0.011	0.308	0.237	0.280	0.327	0.148	0.433	0.567	96,315
IN	0.075	0.001	0.364	0.307	0.312	0.305	0.112	0.436	0.564	83,972
NC	0.074	−0.001	0.296	0.325	0.320	0.299	0.138	0.496	0.504	87,872
ME	0.073	−0.001	0.295	0.308	0.280	0.326	0.116	0.496	0.504	99,727
PA	0.073	−0.002	0.276	0.209	0.282	0.345	0.109	0.428	0.572	102,995
OR	0.072	−0.006	0.275	0.246	0.294	0.314	0.119	0.474	0.526	110,717
GA	0.071	−0.017	0.311	0.346	0.341	0.260	0.143	0.490	0.510	86,760
OH	0.069	0.005	0.320	0.238	0.301	0.316	0.120	0.415	0.585	92,230
DE	0.066	−0.011	0.290	0.221	0.315	0.306	0.092	0.441	0.559	124,287
MI	0.066	0.006	0.320	0.226	0.308	0.302	0.124	0.407	0.593	97,727
NV	0.064	−0.013	0.288	0.349	0.322	0.286	0.105	0.526	0.474	101,637
TX	0.061	0.021	0.421	0.336	0.347	0.265	0.167	0.420	0.580	73,066
WI	0.060	0.002	0.328	0.221	0.304	0.312	0.097	0.393	0.607	101,515
IL	0.059	0.000	0.257	0.214	0.319	0.298	0.124	0.445	0.555	108,790
WA	0.053	−0.013	0.220	0.236	0.315	0.285	0.106	0.509	0.491	121,400
CA	0.052	−0.022	0.161	0.240	0.347	0.267	0.144	0.589	0.411	132,668
NY	0.052	0.004	0.216	0.172	0.308	0.310	0.149	0.439	0.561	114,358
DC	0.048	0.000	0.153	0.105	0.362	0.283	0.188	0.425	0.575	150,173
WY	0.048	0.007	0.306	0.233	0.303	0.294	0.108	0.428	0.572	102,442
VA	0.046	−0.010	0.232	0.258	0.327	0.277	0.099	0.508	0.492	112,649
VT	0.046	0.001	0.268	0.238	0.295	0.302	0.095	0.464	0.536	114,618
MN	0.045	0.004	0.307	0.169	0.313	0.296	0.099	0.355	0.645	124,178
MD	0.043	−0.013	0.207	0.215	0.314	0.281	0.090	0.506	0.494	129,027
NJ	0.042	−0.007	0.193	0.173	0.293	0.313	0.086	0.472	0.528	148,834
RI	0.042	0.014	0.216	0.225	0.305	0.325	0.105	0.498	0.502	109,141

(*Continued*)

State	HWE	SWE	Cons./ HW	Cons./ SW	Young Percent	Old Percent	Poverty Rate	HW/ TW	SW/ TW	Total Wealth
UT	0.042	−0.004	0.275	0.303	0.394	0.246	0.093	0.505	0.495	88,639
MA	0.040	−0.001	0.197	0.180	0.313	0.308	0.102	0.469	0.531	146,918
HI	0.039	−0.009	0.165	0.278	0.320	0.300	0.102	0.620	0.380	149,082
CT	0.033	0.002	0.171	0.197	0.290	0.317	0.081	0.528	0.472	156,059
CO	0.029	0.005	0.253	0.198	0.328	0.260	0.105	0.435	0.565	125,780
NH	0.028	0.011	0.338	0.311	0.301	0.289	0.067	0.471	0.529	115,250
AK	−0.001	−0.004	0.329	0.288	0.361	0.187	0.095	0.448	0.552	97,818
Total	0.073	−0.004	0.301	0.266	0.312	0.304	0.127	0.457	0.543	101,517

Table 5. Factors affecting estimated housing and stock wealth effects.

Notes: Cell entries are averages of the variable over the years 1985–2009; the years 1981–1984 are excluded from the analysis because of lags used for instrumenting. Note that the average housing and stock wealth effects over the entire sample are not the same as the housing and stock wealth effects calculated at the sample means of the variables, and thus the totals presented in this table correctly differ from the values shown in Table 4.

Variables are defined as follows:

HWE = Average housing wealth effect
SWE = Average stock wealth effect
Cons./HW = Average consumption-to-housing wealth ratio
Cons./SW = Average consumption-to-stock wealth ratio
Young Percent = Average percent of the adult population ages 20-34
Old Percent = Average percent of the adult population ages 55 and up
Poverty Rate = Average poverty rate
HW/TW = Average housing wealth-to-total wealth ratio
SW/TW = Average stock wealth-to-total wealth ratio
Total Wealth = Average real, per capita total wealth

key variables that determine the size of these effects as calculated in expression (1) above. Table 6 uses the derivatives that were calculated for Model 5 in Table 4 to measure the impact of demographic and wealth changes within a state on housing and stock wealth effects. For example, a one standard deviation increase in the percentage of young adults in a state (4.1 percentage points) raises the housing wealth effect by 0.9 cents and lowers the stock wealth effect by the same amount. The impact of a one standard deviation increase in the fraction of the adult population that is over age 55 is even larger at 1.8 cents. Similarly, a one standard deviation increase in the poverty rate (3.8 percentage points) raises the housing wealth effect by 1.3 cents. Importantly, changes in the composition of wealth have an even bigger impact on housing wealth effects, with a one standard deviation increase in this ratio raising housing wealth effects by 2.3 cents.

	HWE Impact	SWE Impact	Mean	SD	Min.	Max.
Young Percent	0.009	−0.009	0.312	0.041	0.229	0.478
Old Percent	0.018	−0.007	0.304	0.033	0.135	0.386
Poverty Rate	0.013	0.006	0.127	0.038	0.029	0.272
Housing Wealth Ratio	0.023		0.457	0.103	0.242	0.735
Stock Wealth Ratio		0.020	0.543	0.103	0.265	0.758

Table 6. Impacts on housing and stock wealth effects.

Notes: Columns 1 and 2 show the dollar impact on the respective wealth effect from a one standard deviation change in the variable in question. Columns 3 through 6 provide summary statistics for these variables over the years 1985–2009; the years 1981–1984 are excluded from the analysis because of lags used for instrumenting.

Figure 7 shows the extent of variation within each state over time in the implied housing and stock wealth effects. Stock wealth effects vary less across states than do housing wealth effects, likely reflecting the fact that changes in stock wealth in all states are driven by national stock market movements. This figure makes it apparent, however, that demographic and wealth share differences lead to substantial variation in estimated wealth effects across both states and time.

4.5 Lagged Adjustment and Instrumenting

The regressions reported in Table 3 assume that all adjustments in endogenous variables (housing prices, stock prices, and consumption) take place within one year. This assumption is also reflected in the instrumenting technique employed. The Campbell and Mankiw (1990) approach makes it possible to estimate the effects of exogenous variation in wealth — in particular, wealth changes that are unrelated to current changes in permanent income — because the lagged endogenous variables are assumed to be uncorrelated with permanent income shocks. That approach depends upon assuming a contemporaneous adjustment of house-price growth and consumption growth to permanent income growth shocks. If house-price growth and consumption growth react with a lag to permanent income

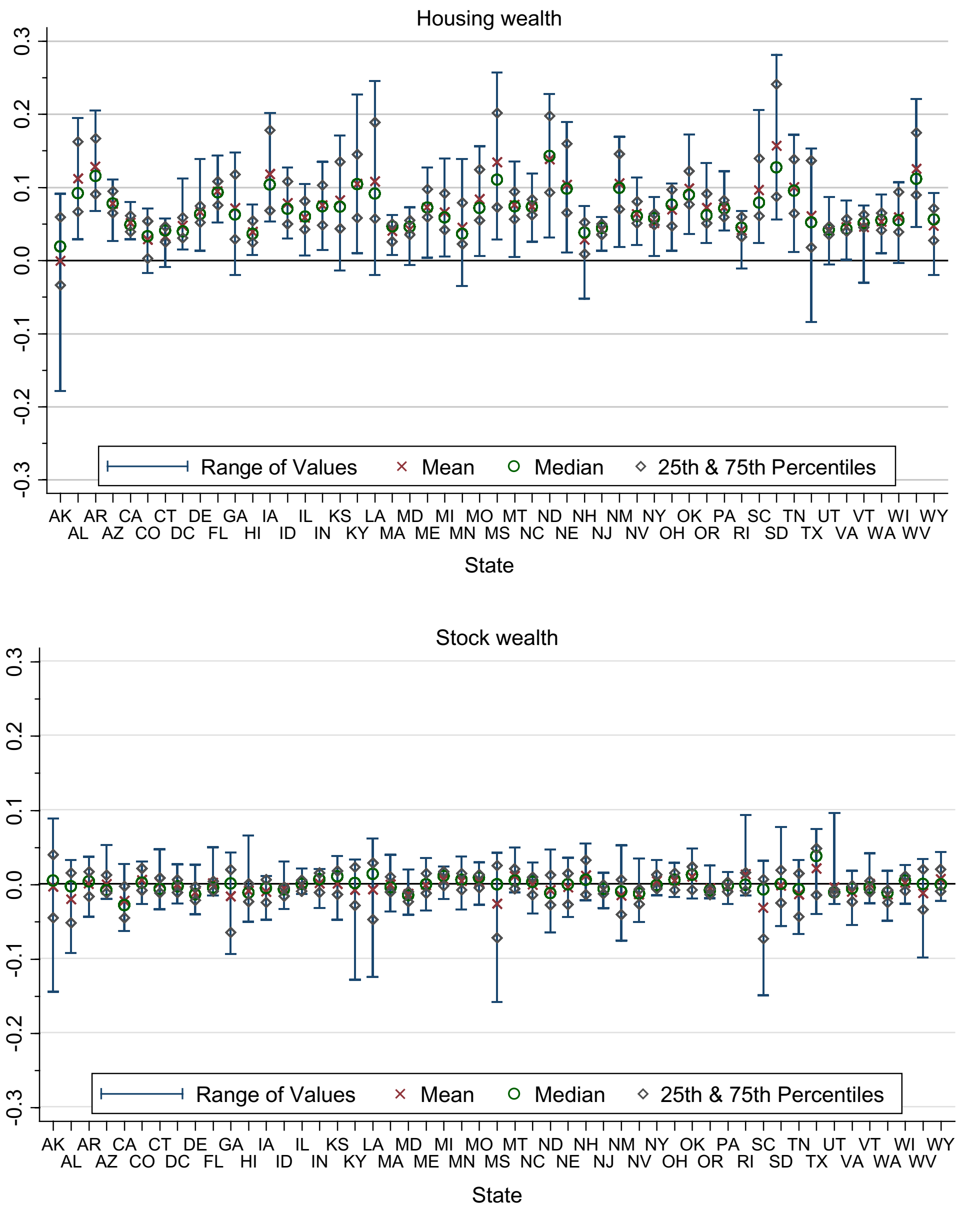

Figure 7. Range of wealth effects over time by state.

Notes: For each state, figure shows the range of calculated wealth effects over the years of the analysis (1985–2009), as well as the mean, median, 25th percentile and 75th percentile of these values.

growth shocks, then the instrumented values of housing wealth will contain information about permanent income and the instrumenting approach will be invalid (although it may still be superior to using contemporaneous endogenous variables as regressors).

If consumption and house prices both adjust with a lag to permanent income shocks, then our model should be altered to permit lagged wealth and income to affect consumption. Furthermore, we must use more distant lags of the endogenous variables as instruments. We explore the robustness of our model to the incorporation of these changes in Table 7. Our sample is slightly smaller due to the need to use more distant lags. Models 1A, 2A, and 5A are identical to models 1, 2, and 5 in Table 3. Models 1B, 2B, and 5B are identical to 1A, 2A, and 5A, except that they use lags 3–5 rather than 2–4 to instrument current values of income, housing wealth, and stock wealth. The coefficient estimates are very similar. Models 1C, 2C, and 5C add a lagged term to the consumption equation, and employ lags 3–5 of the endogenous variables as instruments.[9]

Table 8 computes HWE and SWE effects that are comparable to those reported in Table 4, summing across lagged values in the cases of Models 1C, 2C and 5C to report long-run effects. The implied values of HWE and SWE are very similar to those reported in Table 4. In other words, adding adjustment lags and lagging instruments does not change the conclusions reported in Tables 3 and 4. It is noteworthy that the lagged term for housing wealth is negative in Model 1C. This "overshooting" of consumption with respect to housing wealth is hard to interpret. We conclude that, overall, the results for HWE and SWE that take account of lagged adjustment are very similar to those from the models that include only instrumented contemporaneous values of regressors, using 2–4 lags of endogenous variables as instruments. In the interest of parsimony, and given that lagged values are frequently statistically insignificant, our subsequent computations and figures are based on the regression results reported in Table 3.

[9] Our approach incorporates lagged values of income and wealth but not lagged consumption. An alternative approach would include lagged consumption, but doing so would require an alternative estimation strategy and a different approach to instrumenting. Available approaches to estimation along these lines have very poor small sample properties. Consequently, we avoid those alternatives.

	Model 1A	Model 1B	Model 1C	Model 2A	Model 2B	Model 2C	Model 5A	Model 5B	Model 5C
Income	0.858***	0.669***	0.419***	0.973***	0.813***	1.013***	0.559***	0.520***	0.535***
	(0.090)	(0.084)	(0.104)	(0.075)	(0.094)	(0.185)	(0.075)	(0.100)	(0.118)
1st Lag			0.302***			−0.166			0.219
			(0.111)			(0.218)			(0.155)
Housing Wealth	0.200***	0.291***	0.427***	−0.176	−0.032	−0.170	−7.431***	−10.426***	−12.838***
	(0.028)	(0.037)	(0.058)	(0.122)	(0.107)	(0.221)	(2.416)	(2.987)	(4.408)
1st Lag			−0.202***			0.240			7.114**
			(0.037)			(0.285)			(2.791)
Stock Wealth	0.048**	0.050***	0.021*	−0.363***	−0.301**	−0.611**	−6.513***	−7.858***	−6.745
	(0.019)	(0.013)	(0.012)	(0.138)	(0.135)	(0.256)	(2.516)	(2.190)	(4.161)
1st Lag			0.087***			0.514*			4.700**
			(0.013)			(0.296)			(2.238)
Total Wealth				0.761***	0.647***	1.160***	13.678***	15.718***	14.529*
				(0.248)	(0.236)	(0.445)	(4.696)	(4.139)	(8.548)
1st Lag						−0.901			−8.659*
						(0.555)			(4.506)
Young Percent							−0.006	−0.161*	−0.028
							(0.091)	(0.097)	(0.117)
Old Percent							−0.457***	−0.683***	−0.455**
							(0.124)	(0.151)	(0.178)
Poverty Rate							0.133*	0.166**	0.158*
							(0.077)	(0.076)	(0.088)
Young Percent × Housing Wealth							16.347***	17.617***	23.419**
							(4.385)	(5.348)	(9.416)
1st Lag									−11.373*
									(6.220)
Old Percent × Housing Wealth							9.473**	17.600***	20.934***
							(4.256)	(5.841)	(7.826)
1st Lag									−10.970**
									(5.116)

(*Continued*)

	Model 1A	Model 1B	Model 1C	Model 2A	Model 2B	Model 2C	Model 5A	Model 5B	Model 5C
Poverty Rate × Housing Wealth							−3.661	−2.367	−5.421
							(2.813)	(3.594)	(5.087)
1st Lag									−1.654
									(3.705)
Young Percent × Stock Wealth							14.592***	12.256***	12.205
							(4.226)	(3.858)	(8.520)
1st Lag									−7.968*
									(4.154)
Old Percent × Stock Wealth							7.359	13.609***	11.649
							(4.513)	(4.342)	(7.222)
1st Lag									−7.301
									(4.574)
Poverty Rate × Stock Wealth							−4.443*	−2.802	−7.239*
							(2.653)	(3.093)	(4.050)
1st Lag									2.040
									(3.103)
Young Percent × Total Wealth							−30.787***	−25.862***	−27.197
							(8.609)	(7.995)	(18.708)
1st Lag									13.857
									(10.338)
Old Percent × Total Wealth							−15.852**	−26.496***	−24.166*
							(7.910)	(8.045)	(14.022)
1st Lag									13.636
									(8.365)

(*Continued*)

	Model 1A	Model 1B	Model 1C	Model 2A	Model 2B	Model 2C	Model 5A	Model 5B	Model 5C
Poverty Rate × Total Wealth							9.386*	6.407	14.228*
							(5.649)	(6.458)	(8.230)
1st Lag									−1.594
									(5.931)
Constant	−0.009***	−0.008***	−0.012***	−0.007***	−0.007***	−0.009***	0.069	0.182***	0.084
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.056)	(0.066)	(0.075)
Observations	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224
Wald Chi-square	283.88***	195.24***	280.38***	285.28***	248.91***	262.78***	1, 438.75***	954.19***	2,135.96***
Degrees of freedom	53	53	56	54	54	58	66	66	79

Notes: The dependent variable is log difference of real, per capita consumption (where consumption is proxied by state-level retail sales). Wealth variables are expressed in log differences of real, per capita values. Young Percent is the percent of the adult population ages 20–34; Old Percent is the percentage of the adult population ages 55 and up; Poverty is the poverty rate. Models 1A, 2A and 5A are identical to models 1, 2 and 5 in Table 3 (instrumenting all wealth and interaction variables using the 2nd–4th lags of these variables) but use the same sample as the other models that use additional lags as instruments. Models 1B, 2B and 5B use the 3rd–5th lags of the wealth and interaction variables as instruments. Models 1C, 2C and 5C include the 1st lag of the wealth and interaction variables as exogenous regressors, using the 3rd–5th lags of these variables as instruments. Standard errors (clustered by state) are shown in parentheses below the estimates. The Wald Chi-square statistic tests for the joint significance of all of the coefficients except the constant term.
***Coefficient significant at the 1% level.
**Coefficient significant at the 5% level.
*Coefficient significant at the 10% level.

Table 7. Panel data wealth effect with various lag structures.

	Model 1A	Model 1B	Model 1C	Model 2A	Model 2B	Model 2C	Model 5A	Model 5B	Model 5C
Housing Wealth Effect (HWE)	0.055***	0.049***	0.075***	0.067***	0.055***	0.049***	0.075***	0.067***	0.081***
Stock Wealth Effect (SWE)	0.016***	0.018***	0.008*	0.000	0.016***	0.018***	0.008*	0.000	−0.005
Difference	0.039***	0.031***	0.067***	0.066***	0.039***	0.031***	0.067***	0.066***	0.086***
Housing Wealth Elasticity	0.183***	0.163***	0.250***	0.222***	0.183***	0.163***	0.250***	0.222***	0.270***
Stock Wealth Elasticity	0.058***	0.066***	0.030*	0.002	0.058***	0.066***	0.030*	0.002	−0.019
Difference	0.124***	0.097***	0.221***	0.220***	0.124***	0.097***	0.221***	0.220***	0.288***
Wealth Effect Derivatives									
d HWE/d Young Percent			0.191	−0.039			0.191	−0.039	0.223
d HWE/d Old Percent			0.314	0.376			0.314	0.376	0.545
d HWE/d Poverty Rate			0.190				0.190		0.335
d SWE/d Young Percent			−0.277	0.003			−0.277	0.003	−0.224
d SWE/d Old Percent			−0.607	−0.135			−0.607	−0.135	−0.204
d SWE/d Poverty Rate			0.204				0.204		0.152

Notes: Housing and stock wealth effects are expressed in dollar terms and calculated at the sample mean values for all variables. Housing and stock wealth elasticities and wealth effect derivatives are calculated at sample means for all variables as well. Models 1A, 2A and 5A are identical to models 1, 2 and 5 in Table 4 (instrumenting all wealth and interaction variables using the 2nd–4th lags of these variables) but use the same sample as the other models that use additional lags as instruments. Models 1B, 2B and 5B use the 3rd–5th lags of the wealth and interaction variables as instruments. Models 1C, 2C and 5C include the 1st lag of the wealth and interaction variables as exogenous regressors, using the 3rd–5th lags of these variables as instruments. Standard errors (clustered by state) are shown in parentheses below the estimates.
***Estimated value significant at the 1% level.
**Estimated value significant at the 5% level.
*Estimated value significant at the 10% level.

Table 8. Estimated wealth effects, elasticities and derivatives with various lag structures.

4.6 Why Are Stock Wealth Effects Relatively Small?

We consistently find that stock wealth effects, elasticities, and wealth effect derivatives are small in relation to the comparable housing wealth measures.[10] This finding may appear somewhat puzzling, given that, in theory — as developed by Buiter (2007) and Sinai and Souleles (2005) — stock wealth effects should be larger than housing wealth effects. We can think of several possible explanations for our findings: tax policies that limit the use of securities retirement accounts, the relatively high volatility of stock wealth, and the relatively low proportion of the population that owns stock.[11]

First, stocks are often held in retirement accounts that cannot be liquidated without incurring special tax consequences. Those tax-related liquidation costs discourage consumers to increase current consumption in response to increases in stock prices.

Second, it may be that the higher volatility of stock wealth causes small short-run (one-year) responses of consumption to increases in stock wealth. If consumption decisions are costly to reverse (e.g., if there are costs of liquidating consumer durables, "habit formation" effects, etc.) then consumers will respond less to volatile changes in wealth. Indeed, several papers have found that consumers' short-run responses to stock wealth are much lower than their long-run responses (see the discussion in Parker, 2001).

As shown in Table 9, the coefficient of variation for housing wealth is generally lower than that of stock wealth on average. Furthermore, for the vast majority of states, stock wealth is much more volatile than housing wealth. There are eleven states for which the coefficient of variation is higher for housing wealth than for stock wealth, but in six of those eleven cases, the housing wealth coefficient of variation is no more than 11 percent higher

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[10] Note that our finding of a larger wealth effect for housing compared to equities is consistent with previous studies for the U.S. For instance, in nearly all specifications of CQS (2005), the housing wealth effect exceeds the stock wealth effect. CQS (2013) update their study, and similarly find small stock wealth effects compared to the impact of housing wealth. Carroll *et al.* (2011) find much larger housing than stock wealth effects, and Carroll and Zhou (2011) find a positive impact of housing wealth on consumption but no significant impact of stocks.

[11] It is also possible to argue that the relative size of housing wealth effects are inflated by behavioral factors that are less relevant for stocks, including the fact that home equity loans were actively marketed during the housing boom in ways that margin loans were not and the possibility that rises in housing prices led some unsophisticated investors (which are more prominent within the class of house investors than within the class of equity investors) to extrapolate growth rates, anticipating even greater increases in the future.

State	Housing Wealth Mean	Housing Wealth Standard Deviation	Housing Wealth Coefficient of Variation	Stock Wealth Mean	Stock Wealth Standard Deviation	Stock Wealth Coefficient of Variation
AK	41,295	8,768	0.21	57,802	27,055	0.47
AL	34,109	9,055	0.27	43,743	27,256	0.62
AR	28,541	7,174	0.25	38,998	20,480	0.53
AZ	47,693	17,363	0.36	49,355	17,412	0.35
CA	78,003	32,092	0.41	55,395	20,867	0.38
CO	54,135	17,228	0.32	72,435	22,571	0.31
CT	81,524	18,319	0.22	75,325	22,580	0.30
DC	67,885	39,643	0.58	83,348	20,341	0.24
DE	56,321	23,865	0.42	68,283	15,104	0.22
FL	51,554	20,584	0.40	58,066	18,230	0.31
GA	39,086	8,885	0.23	48,587	26,109	0.54
HI	92,622	39,362	0.42	57,310	21,994	0.38
IA	29,462	7,961	0.27	61,677	24,487	0.40
ID	41,098	15,785	0.38	48,624	21,496	0.44
IL	47,111	13,045	0.28	62,439	23,564	0.38
IN	33,855	8,886	0.26	51,010	26,480	0.52
KS	29,809	6,617	0.22	62,152	19,061	0.31
KY	31,486	8,143	0.26	43,044	24,383	0.57
LA	30,545	7,007	0.23	41,101	22,351	0.54
MA	67,688	16,885	0.25	79,744	24,474	0.31
MD	64,649	24,275	0.38	65,346	25,679	0.39
ME	47,254	12,687	0.27	53,610	25,019	0.47
MI	38,870	11,434	0.29	59,656	22,247	0.37
MN	43,849	13,310	0.30	80,936	21,859	0.27
MO	35,325	9,310	0.26	65,389	20,434	0.31
MS	28,154	6,991	0.25	37,059	23,007	0.62
MT	41,731	16,148	0.39	55,510	19,166	0.35
NC	40,933	10,682	0.26	47,779	23,246	0.49
ND	26,545	8,150	0.31	56,133	22,544	0.40
NE	28,678	6,752	0.24	59,200	18,955	0.32

(*Continued*)

	Housing Wealth			Stock Wealth		
State	Mean	Standard Deviation	Coefficient of Variation	Mean	Standard Deviation	Coefficient of Variation
NH	52,028	11,409	0.22	64,550	27,369	0.42
NJ	70,637	20,246	0.29	78,525	16,485	0.21
NM	40,918	11,123	0.27	45,414	21,210	0.47
NV	51,698	20,089	0.39	50,756	23,337	0.46
NY	49,607	12,348	0.25	65,296	17,931	0.27
OH	36,375	7,628	0.21	56,758	23,858	0.42
OK	27,199	4,527	0.17	44,370	22,918	0.52
OR	52,857	23,719	0.45	58,849	23,191	0.39
PA	43,278	11,641	0.27	60,388	20,190	0.33
RI	52,738	15,420	0.29	57,505	23,592	0.41
SC	39,898	12,161	0.30	44,109	27,964	0.63
SD	28,467	9,972	0.35	55,394	19,743	0.36
TN	36,731	8,904	0.24	44,895	24,705	0.55
TX	28,223	5,018	0.18	45,571	21,832	0.48
UT	42,550	14,740	0.35	47,040	23,752	0.50
VA	54,753	15,840	0.29	59,035	28,125	0.48
VT	52,165	13,640	0.26	63,436	21,284	0.34
WA	60,788	21,816	0.36	61,798	25,995	0.42
WI	38,421	11,554	0.30	63,882	25,674	0.40
WV	30,221	7,442	0.25	41,342	24,449	0.59
WY	43,413	15,681	0.36	59,910	21,275	0.36

Table 9. Wealth variability over time by state.

Notes: Cell entries show the mean, standard deviation, and coefficient of variation for housing and stock wealth across time for each state. In general, stock wealth is more variable than housing wealth.

than the stock wealth coefficient of variation. Among the five cases where housing wealth is substantially more volatile than stock wealth (Delaware, New Jersey, District of Columbia, Florida, and Oregon), two of those cases (DE and DC) exhibit housing volatility more than twice as high as stock wealth volatility. In 40 of 51 cases, stock wealth is more volatile than housing wealth. In four of those 40 cases stock wealth volatility is no more than 11 percent higher, but in 36 of the 40 cases, it is substantially more volatile, and in 15 cases, stock wealth is more than twice as volatile as housing wealth. In summary, in ten of 51 "states" (including DC), housing wealth and stock wealth are similarly volatile; in five states housing wealth is substantially more volatile than stock wealth; and in the remaining 36 states, stock wealth is substantially more volatile than housing wealth. Furthermore, in only two states is housing wealth more than twice as volatile as stock wealth; but in 15 states stock wealth is more than twice as volatile as housing wealth.

A third explanation for the low response of consumption to stock wealth could be aggregation bias. If there are fixed costs to holding stocks (e.g., the cost of becoming familiar with stock market investments and the process of establishing brokerage accounts), then many people may simply not participate at all in the stock market. In that case, the estimated stock wealth response for a state-year observation will be substantially downward biased, since the aggregate response reflects the behavior of only a portion of the population.

While virtually everyone lives in a home, and roughly two-thirds of Americans owned their primary residence during our sample period, as shown in Table 10, only 15–21 percent of Americans (depending on the year) owned stocks, and only 10–18 percent owned pooled investment funds.

	1992	1995	1998	2001	2004	2007	2009
Stocks	16.9	15.2	19.2	21.3	20.7	18.4	18.5
Pooled investment funds	10.4	12.3	16.5	17.7	15.0	11.5	10.8
Retirement accounts	37.9	45.2	48.8	52.2	49.7	55.6	56.2
Cash value life insurance	34.8	32.0	29.6	28.0	24.2	23.2	24.3
Other managed assets	4.0	3.9	5.9	6.6	7.3	5.6	5.7
Primary residence	63.9	64.7	66.2	67.7	69.1	68.9	70.3
Other residential property		11.8	12.8	11.3	12.5	13.9	13.0

Table 10. Household wealth holdings over time.

Notes: Cell entries show percent of households with some holdings of the specified asset in the given year. Households are much more likely to own their primary residence than they are to hold stock wealth.

Sources: Aizcorbe, *et al.* (2003), Bucks, *et al.* (2006), Bucks, *et al.* (2009), Kinnickell, *et al.* (1997), Kinnickell, *et al.* (2000).

Although it is beyond the scope of this paper, future empirical work using household-level data could distinguish between these competing hypotheses (volatility differences of wealth and aggregation bias) to estimate their relative importance in explaining the relatively low marginal propensity to consume from stock wealth. Nevertheless, for the purposes of our study, it is relevant to note that both views are plausible given the much greater volatility of stock wealth for most states and the much lower household participation rate in the stock market.

5 Conclusion

Economic theory has several important implications for the empirical modeling of consumption wealth effects: (1) The composition of wealth (that is, the relative proportions of housing and stock wealth) should matter for the estimation of wealth effects on consumption associated with changes in either type of wealth; (2) age characteristics of the population should matter for estimation of housing wealth effects, either because of anticipated downsizing of housing by older residents, or because younger residents tend to face more binding constraints on borrowing against permanent income; (3) the proportion of low-wealth individuals may matter for wealth effects through its effect on the extent to which residents are likely to face binding borrowing constraints against permanent income; and (4) permanent income and wealth variation are likely correlated, which means that estimates of wealth effects may suffer from endogeneity/omitted variable bias.

This paper assembles new annual data on state-level housing wealth, stock wealth, and other variables for the period 1981 to 2009 in order to address each of these theoretical ideas. In contrast to Calomiris *et al.* (2009) — which was based on less-reliable data — we find evidence of a large average housing wealth effect during our sample period. Consistent with theory, housing wealth effects vary dramatically over time and across states, reflecting variation in the proportion of housing wealth, variation in age composition associated with varying state-level experiences during the baby boom, and variation in the incidence of poverty. Stock wealth effects, on average, are much smaller than housing wealth effects, and they also vary over time and across states. These estimates show the importance of taking account of wealth composition, age composition, and wealth distribution when estimating housing and stock wealth effects. Wealth effects going forward, therefore, are likely to be very different from those of the past as they will be contingent on a variety of demographic and economic characteristics that will change over time.

One advantage of our state-level aggregate analysis is that our specification may be useful to macroeconomic forecasters to gauge the time variation in wealth effects. The most important inputs on which we rely for our estimation — annual state-level data on the age of the population, the poverty rate, and the amount of housing wealth — are generally available with short lags, and could therefore be used to update housing wealth effect forecasts annually. Given the amount of variation in wealth effects over time, this could be a useful forecasting tool.

Our finding that stock wealth effects are small and not highly statistically significant is at odds with some theoretical models. In the models developed by Buiter (2007) and Sinai and Souleles (2005), stock wealth effects should generally be larger than housing wealth effects, notwithstanding the greater usefulness of housing wealth as collateral for borrowing against permanent income. It is worth noting that Carroll and Zhou (2011) — who employ better quality data on stock wealth for a shorter time period — also find a negligible stock wealth effect as did Carroll *et al.* (2011), and Case *et al.* (2005, 2013). We conjecture that the presence of retirement accounts, the greater volatility of stock wealth, and the lower rate of participation by households in the stock market can explain the relatively muted response of consumption to changes in stock market wealth.

Data Appendix

Consumption: *Real, per-capita retail sales*

State-level retail sales data from 1977Q1 through 2010Q1 were provided by Moody's Economy.com. The underlying data are nominal, seasonally-adjusted annual rates at a quarterly frequency. Nominal annual retail sales are the average of the quarterly figures within each year.

Housing Wealth: *Real, per-capita value of owner-occupied housing*

Housing wealth is measured as the average value of owner-occupied housing times the number of owner-occupants within each state. The average value of owner-occupied housing each quarter is taken from the

Land Prices by State Dataset developed by Davis and Heathcote (2007), and provided by the Lincoln Institute of Land Policy; we use fourth quarter figures as the value for the year in our annual data.[12] We use the 2011Q1 release of these data.

The number of owner-occupied households in each state-year is derived from the Annual Social and Economic (ASEC) Supplement to the Current Population Survey (CPS) using the March micro data provided by the National Bureau for Economic Research.[13] Using the household data in each year, the H_TENURE variable is tabulated by state using MARSUPWT (the March Supplement, or household sampling, weight) to get an estimate of the number of owner-occupied, renter-occupied, and total households by state. These estimates are smoothed by taking the three-year moving average (forward and lagging), in order to minimize noise induced by changes in the sampling weights over time.[14]

Total nominal housing wealth for each state-year observation is simply the number of owner households times the average value of owner-occupied housing.

Stock Wealth: *Real, per-capita financial assets*

Total U.S. stock wealth is calculated as the sum of corporate equities, mutual fund shares and pension fund reserves for households and non-profit corporations from the Federal Reserve Flow of Funds (FoF) Z1 statistical release, Table L100, 2011Q1 release; annual data are year-end (fourth quarter) values.

Aggregate U.S. stock wealth is allocated across states based on the distribution of mutual fund holdings across states. CQS (2005) use data on mutual fund holdings by state obtained from the Investment Company Institute (ICI) as a proxy for the fraction of aggregate financial wealth

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12 These data are updated quarterly and can be found at "Land and Property Values in the U.S.", Lincoln Institute of Land Policy, http://www.lincolninst.edu/resources/. According to the Lincoln Institute website, this figure is estimated in two steps. "First, the average value for each state is estimated in 1980, 1990, and 2000 using micro data from the Decennial Census of Housing (DCH). Then the Federal Housing Finance Agency (FHFA) quarterly repeat-sales (constant quality) house price indexes for each state are used to scale the home value series by quarter between 1980 and 2000 and to extend the home value series back from 1980 to 1975 and forward from 2000 to the most recent quarter. The growth rates of the reported FHFA indexes are adjusted so that their growth between 1980–1990 and 1990–2000 match the decennial growth of average house values from the DCH data. The 1980–1990 growth-rate adjustments are applied to the pre-1980 FHFA data and the 1990–2000 growth-rate adjustments are applied to the post-2000 FHFA data."

13 http://www.nber.org/data/current-population-survey-data.html

14 The estimated coefficients in Table 3 are qualitatively similar using the raw estimates of the number of owner-occupied households instead of the three-year moving averages.

held in each state in the years 1986, 1987, 1989, 1991, and 1993. Since the publicly-available CQS (2005) data do not contain the underlying ICI mutual fund allocations, each state's implied percent of aggregate U.S. financial wealth was calculated using the CQS (2005) Nominal Stock Market Wealth variable in each quarter.[15] The percent of financial wealth held by each state in 1986, 1987, 1989, 1991, and 1993 was then assumed to be the first quarter values in these years.[16]

Additional years' estimates of the distribution of mutual fund assets by state were provided directly by ICI. For 1995, the figure is based on the same mutual fund company information that was used in CQS (2005); 2000, 2008, and 2009 figures are based on household survey results. For years prior to 1986, we used the 1986 value, while values for the remaining missing years were interpolated linearly.

Nominal stock wealth is then aggregate U.S. financial wealth times the mutual fund percent for each state-year.

Total Wealth: *Real, per-capita financial assets + real, per-capita housing wealth*

Total real, per-capita wealth is the sum of real, per-capita housing wealth and real, per-capita stock wealth.

Income: *Real, per-capita personal income by state*

Annual and quarterly data are from the Bureau of Economic Analysis (2011Q1 release).

Population

Mid-year population estimates of the Census Bureau, provided in the annual personal income summary by state from the Bureau of Labor Statistics. Intercensal population estimates for the 2000's were not yet available at the time of this draft, so population estimates for 2001 to 2009 are based on postcensal estimates that were obtained directly from the Bureau of the Census.[17] The 2010 population figures are from the 2010 census.

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[15] The publicly-available data used this study can be found at http://elsa.berkeley.edu/~quigley/papers.html.

[16] CQS (2005) interpolated quarterly values between these years, and analysis of the data revealed that first quarter values were the break points in the interpolation.

[17] http://www.census.gov/popest/states/NST-ann-est.html

Demographic (Age Range and Poverty) Data

Estimated population counts by age group for 1970–2009 were obtained from the Centers for Disease Control CDC WONDER on-line database.[18] The young adult population ratio is the percentage of the adult population aged 20–34; the middle adult population ratio is the percentage of the adult population aged 35–54; and the old adult population ratio is the percentge of the adult population aged 55 and up.

Poverty rates for each state-year were found in Historical Poverty Table 21, Number of Poor and Poverty Rate, by State, on the Bureau of the Census website.[19] According to notes in this table, the figures are estimated by the Bureau of the Census using the Annual Social and Economic (ASEC) Supplement to the Current Population Survey (CPS).

GDP Deflator

All real values are calculated using the Gross Domestic Product Implicit Price Deflator (Index 2005 = 100). Data were obtained from the Federal Reserve Bank of St. Louis Federal Reserve Economic Data (FRED) service (Series ID: GDPDEF; 2011Q1 release).[20] Fourth quarter values are used as the annual figure of the index.

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18 Actual data were obtained from two different pages on the CDC WONDER website: Data for 1970–1989 came from: United States Department of Commerce, U.S. Census Bureau, Population Division; Census Population 1970–2000 for Public Health Research, CDC WONDER On-line Database, March 2003. Centers for Disease Control and Prevention (CDC), National Center for Health Statistics (NCHS), Bridged-Race Population Estimates, United States, 1990–2003, July 1st resident population by state, county, age, sex, race, and Hispanic origin, on CDC WONDER On-line Database, June 2005. Accessed at http://wonder.cdc.gov/census.html on Jul 11, 2011 7:47:34 PM. Data for 1990–2009 came from: United States Department of Health and Human Services (US DHHS), Centers for Disease Control and Prevention (CDC), National Center for Health Statistics (NCHS), Bridged-Race Population Estimates, United States July 1st resident population by state, county, age, sex, bridged-race, and Hispanic origin, compiled from 1990–1999 bridged-race intercensal population estimates and 2000–2009 (Vintage, 2009) bridged-race postcensal population estimates, on CDC WONDER On-line Database. Accessed at http://wonder.cdc.gov/bridged-race-v2009.html on Jul 11, 2011 7:49:52 PM.

19 http://www.census.gov/hhes/www/poverty/data/historical/people.html.

20 http://research.stlouisfed.org/fred2/

Appendix Table

State		State		State	
AK	Omitted	KY	0.042***	NY	0.042***
			(0.010)		(0.009)
AL	0.048***	LA	0.029***	OH	0.054***
	(0.010)		(0.010)		(0.008)
AR	0.051***	MA	0.047***	OK	0.044***
	(0.012)		(0.007)		(0.009)
AZ	0.044***	MD	0.034***	OR	0.045***
	(0.009)		(0.005)		(0.008)
CA	0.028***	ME	0.061***	PA	0.062***
	(0.007)		(0.008)		(0.009)
CO	0.029***	MI	0.047***	RI	0.057***
	(0.005)		(0.007)		(0.008)
CT	0.053***	MN	0.051***	SC	0.041***
	(0.007)		(0.007)		(0.009)
DC	0.006	MO	0.057***	SD	0.056***
	(0.015)		(0.008)		(0.010)
DE	0.047***	MS	0.039***	TN	0.044***
	(0.007)		(0.011)		(0.009)
FL	0.071***	MT	0.048***	TX	0.015**
	(0.011)		(0.010)		(0.008)
GA	0.024***	NC	0.039***	UT	0.027***
	(0.006)		(0.008)		(0.006)
HI	0.043***	ND	0.056***	VA	0.037***
	(0.007)		(0.009)		(0.006)
IA	0.066***	NE	0.064***	VT	0.046***
	(0.009)		(0.008)		(0.006)
ID	0.041***	NH	0.056***	WA	0.034***
	(0.008)		(0.005)		(0.006)
IL	0.043***	NJ	0.053***	WI	0.054***
	(0.008)		(0.006)		(0.007)
IN	0.048***	NM	0.030***	WV	0.059***
	(0.007)		(0.011)		(0.012)
KS	0.051***	NV	0.049***	WY	0.035***
	(0.008)		(0.006)		(0.007)

Notes: Standard errors (clustered by state) are shown in parentheses below the estimates.
***Coefficient significant at the 1% level.
**Coefficient significant at the 5% level.
*Coefficient significant at the 10% level.

Table A1. State fixed effect coefficients for Table 3 — Model 5.

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