

# Commentary: Housing, Credit and Consumer Expenditure

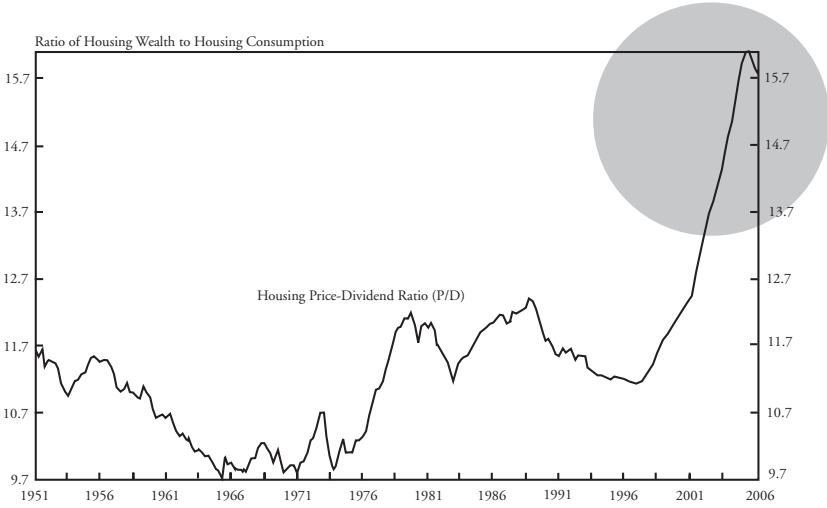
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*Sydney C. Ludvigson*

The subject of this working session is housing and consumer behavior. As emphasized by John Muellbauer, one possible way in which house prices could influence consumer spending is through a housing collateral effect that is especially important for credit constrained households. While we keep in mind Professor Muellbauer's work, I want to steer the conversation in a slightly different direction, to talk to you today about the role of risk premia in the U.S. housing market and the extent to which fluctuations in such premia might influence aggregate consumer spending. Indeed, the unprecedented surge in U.S. house prices that preceded the recent mortgage crisis appears, anecdotally, to have been driven by a decline in market participants' assessment of the riskiness of these assets. Consider Chart 1.

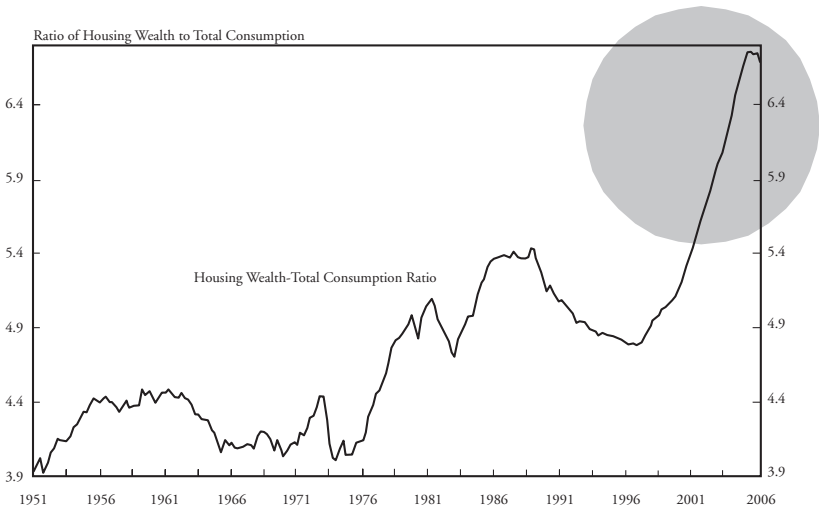
Chart 1 plots the ratio of aggregate housing wealth to housing consumption, for the post-war period up through the end of 2006. Houses effectively pay dividends equal to the service flow from the durable housing stock; hence, this is a housing price-dividend ratio, denoted P/D in the chart. I have spotlighted the dramatic run-up in this ratio that began in the late 1990s. The picture is similar if we look at housing wealth relative to total (housing and nonhousing) consumption (Chart 2) or if we look at the price-dividend ratio from real estate investment trusts (REITs) securities (Chart 3).

### Chart 1 House Prices are High Relative to Housing Consumption



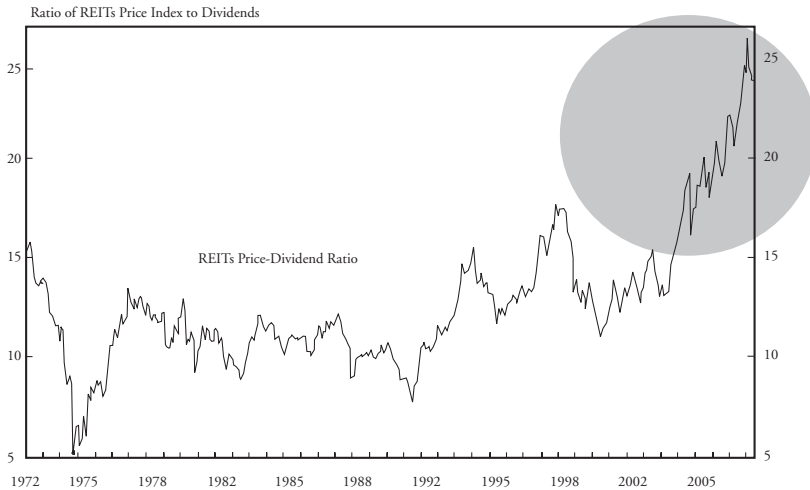
Source: NIPA and U.S. Flow of Funds

### Chart 2 House Prices are High Relative to Total Consumption



Source: NIPA, U.S. Flow of Funds and author's calculations

**Chart 3**  
**Price-Dividend Ratio from REITs**



Source: National Association of Real Estate Investment Trusts

These charts convey two important empirical facts. One is that the big movements in housing wealth are really movements relative to measures of fundamental value. A second is that aggregate house prices—relative to virtually any indicator of fundamental value—have reached unprecedented levels in the last ten years. As we’ll see, statistical evidence suggests that a substantial fraction of the variability in these valuation ratios is attributable, historically, to movements in risk premia.

Why concern ourselves with risk premia in this session on consumer behavior? I’ll make two points. First, movements in risk premia in the U.S. housing market are large and account for a substantial fraction of variability in house prices relative to measures of fundamental value. Second, unlike the direct effects of credit market conditions, movements in risk premia in the aggregate housing market potentially affect all homeowners, including unconstrained households who comprise a large fraction of aggregate consumption. By saying this, I do not mean to suggest that access to credit is a negligible factor in the consumption decisions of some households. I do suggest, however, that the direct effect of credit market conditions on broader aggregate

consumption may be smaller than the indirect effect that these conditions could have on aggregate consumption via risk premia.

Before elaborating on these points, let's briefly review some other economic channels through which housing wealth might influence consumer spending. First, there might be a direct wealth effect: The more wealth people have (of any form) the more they consume. But as John Muellbauer points out, classic life-cycle consumption theory suggests that such a wealth effect should be small or even negative. Moreover, houses may merely provide a hedge against rent risk, and therefore fluctuations in house prices could have no real wealth effect (Sinai and Souleles, 2005). Second, house prices may also be correlated with consumer spending because of a common macroeconomic factor (for example, income expectations) that affects both housing and consumption at the same time. Professor Muellbauer's analysis explicitly controls for this possibility by including a measure of expected income growth in the empirical model for consumption. Third, housing wealth may be related to consumer spending because of a direct housing collateral effect. For households that are borrowing constrained, an increase in house prices means a higher collateral value of their home, enabling them to borrow and spend more.

This latter channel—what I'll call the housing collateral channel—forms the focus of John Muellbauer's work. To my knowledge, this work provides one of the few empirical investigations of credit market conditions and aggregate consumer spending, and is therefore most welcome. Prior to considering this evidence, however, I find it useful to pause for a moment in order to muse on the theoretical basis for linking credit market conditions to broader aggregate consumption, keeping in mind the general equilibrium complications. For example, in some economic frameworks, such as in work by Hanno Lustig of the University of California, Los Angeles, and Stijn Van Nieuwerburgh of New York University (Lustig and Van Nieuwerburgh, 2006, 2007), fluctuations in housing collateral affect households' ability to share risks with one another, and therefore affect the cross-sectional distribution of consumption, but do not affect the size of the overall consumption pie, or aggregate consumption. Movements in housing

collateral do affect risk premia in the housing market, however, which are linked to aggregate consumption.

My reading of Professor Muellbauer's results is in fact quite consistent with a relatively modest role for credit market conditions in directly affecting broader aggregate consumption. Looking at his Tables 2 and 3, we see that although the credit conditions index is found to be statistically significant in some regressions, measures of consumer credit conditions add little to the explained variation in quarterly aggregate consumption growth. For example, in estimations on U.S. data, including measures of credit market conditions in the regression enables the empirical model to explain just 3.8% more of the quarterly variation in consumption growth than what can be explained by a benchmark specification that excludes these indicators.

Both the benchmark specification in Professor Muellbauer's study as well as the specifications including the credit conditions index contain a freely estimated stochastic trend to account for factors such as "demographic trends, evolutionary changes in the inequality of income and wealth and changes in social security and pensions systems, cohort-specific evolutionary shifts in attitudes in time preferences and risk, as well as long-term shifts in credit conditions." While it is possible that changes in credit conditions have had an important effect on consumption through their effect on this trend term, we cannot ascertain whether this is the case because it is impossible to identify the relative importance of the many potential economic forces that may have contributed to movements in a freely estimated stochastic trend. What we do observe from these results is that the direct effect of credit market conditions on broader aggregate consumption appears to be small relative to the effect of other fundamental determinants, for example, the change in the unemployment rate and the log income-consumption ratio.

With that, I want to return to the question of housing risk premia, and bring your attention to several more charts. From Chart 1 we saw that up until about 1998, the housing price-dividend ratio fluctuated within certain ranges. If we accept for the moment that housing valuation ratios will continue to fluctuate within their old historical ranges, then a high housing price-dividend ratio must

foretell some combination of unusual increases in housing consumption or decreases in housing prices in order to bring the ratio back in line with historical norms.

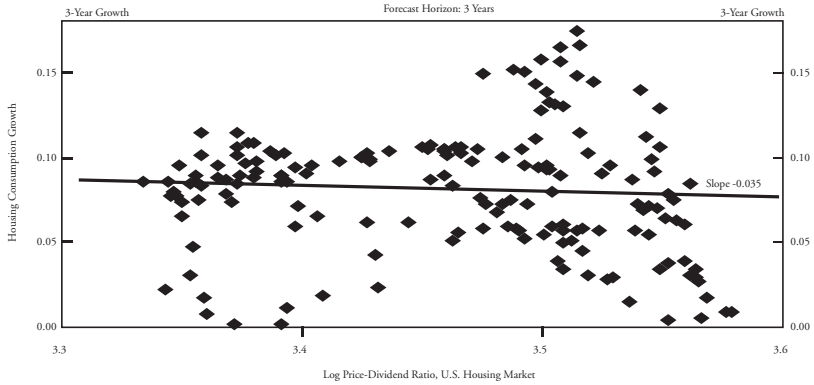
So let's look at those historical relationships. Chart 4 shows the relationship, in data up to 1998, between the log housing price-dividend ratio, on the horizontal axis, and the growth in housing consumption three years ahead, on the vertical axis. It is clear from the cloud of data points in Chart 4 that there is no evidence of a statistically reliable relationship between these two variables. In fact, the slope of a fitted line is almost zero, but if anything is slightly negative, suggesting that high price-dividend ratios forecast *lower* future housing consumption, not higher. High house prices do not forecast higher housing consumption.

High house prices do forecast lower future house prices, however. Chart 5 shows that a high price-dividend ratio for the U.S. housing market is associated with slower and sometimes negative real housing wealth growth over a subsequent three-year horizon.

How do we link these findings back to movements in risk premia? We just saw that high house prices do not forecast higher housing consumption. But one reason house prices could be high relative to housing consumption is that the rate at which future housing consumption is discounted may have been driven down by unusually low risk premia. We can measure movements in risk premia in the housing market as forecastable movements in excess housing returns. The next two charts show that, historically, much of the variation in housing wealth relative to measures of fundamental value has been driven by movements in risk premia.

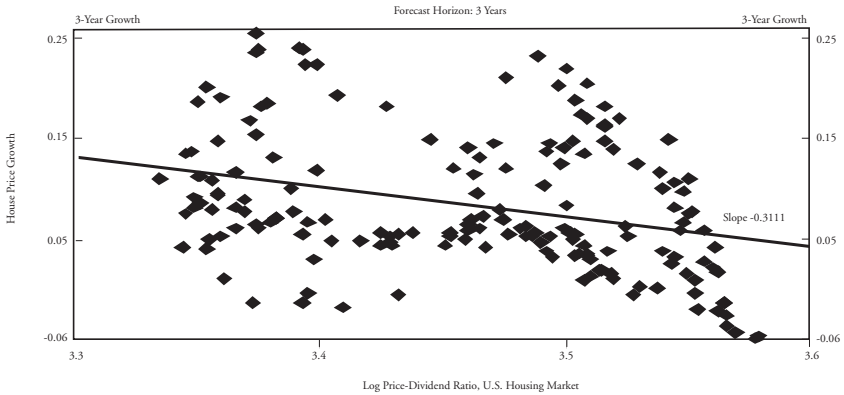
Chart 6 shows the historical relationship between the price-dividend ratio for the U.S. housing market, on the horizontal axis, and the three-year-ahead log housing return in excess of a short-term Treasury bill rate, on the vertical axis. The slope of a fitted line is negative and large in absolute value, indicating that a high price-dividend ratio forecasts sharply lower excess housing returns over the next three years. In fact, this simple relationship explains 56% of the three-year-ahead variation in the excess housing return. I should note that these findings are similar

### Chart 4 Do High Price-Dividend Ratios Forecast Higher Housing Consumption?



Note: Historically, high P/D does not forecast high future dividends, D. What about future house prices, P?  
Source: NIPA, U.S. Flow of Funds, 1952:Q1-1998:Q1, author's calculations

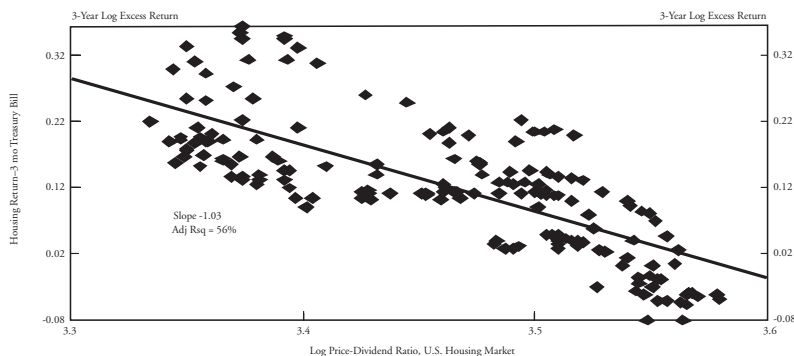
### Chart 5 Do Higher Price-Dividend Ratios Forecast House Price Growth?



Note: Historically, high P/D forecasts lower house prices, P.  
Source: NIPA, U.S. Flow of Funds, 1952:Q1-1998:Q1, author's calculations

## Chart 6

### High Price-Dividend Ratios Forecast Low Housing Returns



The housing P/D is high when *risk premia* are low.

Source: NIPA, U.S. Flow of Funds, 1952:Q1-1998:Q1, author's calculations

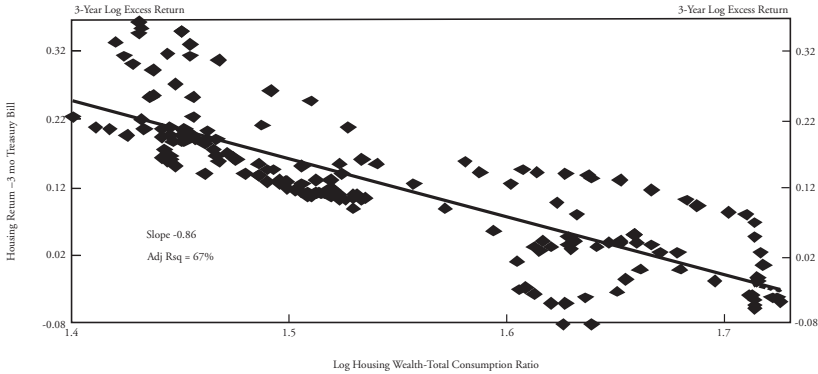
to those reported in a recent working paper by Federal Reserve Board economists Sean Campbell and coauthors (Campbell, Davis, Gallin and Martin, 2007). Similarly, Chart 7 shows that when the housing wealth-total consumption ratio is high (as recently, recall Chart 2), excess housing returns are again forecast to be sharply lower.

Given that movements in risk premia constitute a quantitatively important source of variability in the U.S. housing market, the question I would like to see addressed is whether such movements have any influence on aggregate consumer spending. This is an empirical question; economic theory provides little guidance as to the magnitude of any such relation. In an initial attempt to answer this question, Stijn Van Nieuwerburgh of the Stern School of Business at NYU and I recently estimated a dynamic model of optimal consumption choice that explicitly accounts for the role of risk-premia in asset markets. For your reference, the main features of this model and some empirical results are summarized in Exhibit A.

Let me direct you to the empirical results in the table. The results suggest that changes in aggregate consumption are affected contemporaneously by fluctuations in housing risk premia, but modestly so. Such movements comprise an estimated 9.5% of the quarterly variation in aggregate consumption growth. Fluctuations in current and expected future labor income growth (human wealth), and, to a lesser



### Chart 7 High Housing–Total Consumption Forecasts Low Housing Returns



Note: The housing wealth–consumption ratio is high when housing *risk premia* are low.  
Source: NIPA, U.S. Flow of Funds, 1952:Q1–1998:Q1, author’s calculations

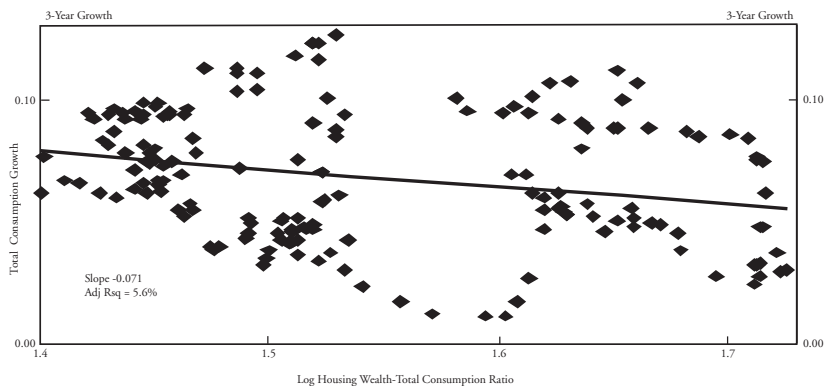
extent, current and expected future returns on financial assets, play a more important role in contemporaneous consumption decisions.

But it is important to distinguish the question of how fluctuations in housing risk premia may be related to contemporaneous consumption, from the question of what, if anything, such fluctuations portend about the future path of consumer spending. The last chart in your handout, Chart 8, shows the historical relationship between the log housing wealth–total consumption ratio, on the horizontal axis, and the growth in total aggregate consumption, three years ahead, on the vertical axis. There is little relationship between the two variables; the slope of a fitted line is close to zero. Thus, even though fluctuations in housing risk premia may have some effect on contemporaneous consumption, high house prices relative to total consumption contain little information about future consumer spending.

Of course, these conclusions are based on historical relationships up to 1998. The important question going forward is whether the unusual behavior in the housing market since 1998 represents a break toward a fundamentally different long-run relationship between housing wealth and consumer spending, or whether it is simply a very large outlier, a more extreme version of a familiar historical pattern. It is too early to know, but at least the historical data give

### Chart 8

## Housing Wealth-Total Consumption Does Not Forecast Total Consumption Growth



Note: High house prices relative total consumption contain little information about future consumption.  
 Source: NIPA, U.S. Flow of Funds, 1952:Q1-1998:Q1, author's calculations

us some reason to expect that even a large decline in housing wealth may have only a modest impact on aggregate consumption, provided that such a decline does not have important spillover effects on other determinants of consumer spending.

I want to close my comments by making a general observation on the estimation of “wealth effects” in aggregate consumption data. Let lower case letters denote log variables, e.g.,  $\log C_t = c_t$ . To estimate wealth effects, a typical empirical specification is a regression of log consumption,  $c_t$ , on log housing wealth,  $h_t$ , log nonhousing wealth,  $s_t$  (including stock market wealth and other financial wealth), and log labor income,  $y_t$ :

$$c_t = \alpha + \omega_s s_t + \omega_h h_t + \omega_y y_t + u_t$$

For example, Case, Quigley and Shiller (2005) estimate equations of this form for several countries. It is commonplace to use such specifications to estimate the relative wealth effects out of different forms of wealth (e.g., housing versus nonhousing wealth); that is to estimate differential “marginal propensities to consume” out of housing and nonhousing wealth. A larger coefficient on  $h_t$  than on  $s_t$  is interpreted as a greater marginal propensity to consume out of housing wealth than nonhousing wealth.

In almost any representative agent model of consumer spending, however, a loglinear approximation of the household budget constraint implies an equation of the form above, in which the coefficients  $\omega_s$ ,  $\omega_h$  and  $\omega_y$  have the interpretation of *wealth shares*, with  $\omega_s$  the share of nonstockmarket wealth in aggregate (human and non-human) wealth,  $\omega_h$  the share of housing wealth in aggregate wealth and  $\omega_y$  the share of human wealth in aggregate wealth (Lettau and Ludvigson, 2001). In addition, a first-order approximation of the budget constraint implies that log consumption, log wealth (housing and nonhousing) and log labor income are cointegrated, with the wealth shares  $\omega_s$ ,  $\omega_h$  and  $\omega_y$  equal to the cointegrating coefficients, which may be estimated from data.

An advantage of this approach is that the estimated coefficients are superconsistent and therefore robust to regressor endogeneity in large samples. But because the estimated regression coefficients have the interpretation of wealth shares, they cannot be used to reveal the relative importance of housing versus nonhousing wealth in consumption fluctuations. To see this, note that the marginal propensity to consume out of nonhousing wealth is given by

$$\frac{dC_t}{dS_t} \approx \frac{d \log C_t / C_t}{d \log S_t / S_t} = \omega_s \frac{C_t}{S_t},$$

while the marginal propensity to consumption out of housing wealth is given by

$$\frac{dC_t}{dH_t} \approx \frac{d \log C_t / C_t}{d \log H_t / H_t} = \omega_h \frac{C_t}{H_t}.$$

In U.S. data, housing wealth,  $H_t$ , comprises a larger fraction of household net worth than nonhousing wealth,  $S_t$ , thus the housing wealth share is larger than the nonhousing wealth share,  $\omega_h > \omega_s$ . At the same time, the consumption-nonhousing wealth ratio is necessarily larger than the consumption-housing wealth ratio.  $C_t/S_t > C_t/H_t$ . It follows that marginal propensities to consume out of different forms of wealth may be similar even if wealth shares differ. Rather than giving an indication of the relative importance of different forms of wealth in aggregate consumption fluctuations, regressions

of the form above may merely tell us that housing wealth is a larger share of aggregate wealth than is stock market wealth.

An alternative approach to estimating wealth effects, one more akin to that taken in Professor Muellbauer's paper, is the Euler equation-inspired methodology in which estimation is carried out in growth rates and/or ratios of variables. For example, the empirical specifications in Professor Muellbauer's paper are regressions of consumption growth (log differences in consumption) on a variety of other variables, including measures of consumer credit conditions, which are plausibly covariance stationary. The regression coefficients in such specifications no longer have the interpretation of wealth shares, so we escape the difficulties with interpretation just discussed. But estimation in growth rates introduces a new set of difficulties: There is no longer reason to expect that the estimated coefficients will be robust to regressor endogeneity. The right-hand-side variables in these regressions are endogenous, and the estimated parameters may therefore fail to reveal the true empirical relationships that the researcher seeks to evaluate. Getting around these difficulties may require a more structural approach to modeling and estimating the wealth effects on consumer spending.

**Exhibit A: Model of Aggregate Consumption<sup>1</sup>:**

- Optimizing households derive utility from nonhousing and housing consumption.
- Allows for changing expected returns (risk premia and short rates) on both housing wealth and financial wealth.
- Allows for current and expected future labor income (human wealth).
- Innovations (unpredictable movements) in aggregate consumption determined by:
  - Revisions in expected future returns to housing wealth, financial wealth, current and expected future labor income (human wealth).
  - Revisions in expected changes to the share of expenditure on nonhousing consumption in total consumption.
- Estimates from U.S. aggregate data.

**Nonhousing Aggregate Consumption**

U.S. Data, 1952:Q1-2006:Q4

<i>Fraction of variance in nonhousing consumption attributable to:</i>	
Current and expected future housing wealth returns (incl. risk premia)	9.5%
Current and expected future labor earnings (human wealth)	54%
Current and expected future financial wealth returns (stocks, bonds, nonhousing durables)	26%
Covariance terms	10.5%

Source: Sydney C. Ludvigson and Stijn Van Nieuwerburgh, New York University.

<sup>1</sup>Under complete markets, the relationship between aggregate consumption and returns can be represented by the preferences of a fictitious representative agent who can trade the aggregate housing return. The per-period utility function is a constant-elasticity-of-consumption aggregator over housing and nonhousing consumption, while intertemporal preferences are modeled using the objective function proposed

by Epstein and Zin (1989, 1991) and Weil (1989). The model implies that the innovation in nonhousing consumption,  $c_{t+1}^{nh}$ , is given by

$$c_{t+1}^{nh} - E_t c_{t+1}^{nh} = r_{t+1} - E_t r_{t+1} + (1 - \sigma)(E_{t+1} - E_t) \sum_{j=1}^{\infty} \kappa^{-j} r_{t+1+j} + \left( \frac{\sigma - 1}{\varepsilon - 1} \right) (E_{t+1} - E_t) \sum_{j=0}^{\infty} \kappa^{-j} \Delta s_{t+1+j},$$

where  $E_t$  is the expectation operator, conditional on information at time  $t$ , the log return  $r_{t+1}$  is a portfolio weighted average of the returns on financial wealth, housing wealth, and current and expected future labor income (human wealth),  $s_{t+1}$  is the log expenditure share on nonhousing consumption in total consumption, and  $\sigma$  and  $\varepsilon$  are preference parameters. A vector autoregression is used to estimate conditional expectations, as in Campbell (1991).

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