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ABSTRACT

Models used to guide policy, as well as some empirical studies, suggest that the effect of housing wealth on consumption is large and greater than the wealth effect on consumption from stock holdings. Recent theoretical work, in contrast, argues that changes in housing wealth are offset by changes in housing consumption, meaning that unexpected shocks in housing wealth should have little effect on non-housing consumption. We reexamine the impact of housing wealth on non-housing consumption, employing the Case-Quigley-Shiller data on U.S. housing wealth that have been used in prior studies to estimate a large housing wealth effect. Existing empirical work fails to control for the fact that changes in housing wealth may be correlated with changes in expected permanent income, biasing the resulting estimates. Once we control for the endogeneity bias resulting from the correlation between housing wealth and permanent income, we find that housing wealth has a small and insignificant effect on consumption. Additional analysis of time-series results provides further support for that view.

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I. Introduction

In the United States, the housing cycle has been found to be a uniquely reliable indicator of the business cycle, and housing investment appears to have a disproportionately large impact on the rest of the economy (Green, 1997, Gauger and Snyder, 2003, Miles, 2009). Indeed, Leamer (2007) drives this point home by titling his study "Housing IS the Business Cycle," and concludes with the suggestion that a new "Taylor Rule" be employed by the Federal Reserve that replaces output with residential investment as a better means to guide monetary policy. Doing so, Leamer argues, would decrease the intensity of business cycles. The current business cycle seems to offer further evidence that swings in housing are central to business cycles.

The relative importance of the various channels through which the housing cycle are related to other sectors in the economy, however, remains controversial. Housing investment, and the forward and backward linkages between construction demand and other sectors of the economy, is one potentially important channel. Another is consumption; macroeconomic models typically posit a wealth effect through which increases in the value of the housing stock raise consumption demand: when housing prices rise, consumers experience an increase in wealth, and increase spending accordingly.

Prominent economists and policy makers, such as former Fed governor Frederic Mishkin (2007), have found evidence of a large wealth effect from housing (much larger than the estimated wealth effect from stocks), although Mishkin (2007) recognizes that estimates of housing wealth effects are imprecisely measured. Other Fed officials, such as Ben Bernanke, Donald Kohn, and Charles Plosser have made public statements indicating their belief in the housing wealth effect. Indeed the Federal Reserve employs a model, which presumably guides its policies, that assumes the housing wealth effect is large and significant.

A number of recent empirical papers have examined the housing wealth effect. Two of the most widely cited recent studies, (Case, et al., 2005, and Carroll, et al., 2006) not only find significant housing wealth effects, but both also find that the wealth effect from housing is larger than the wealth effect from stocks. If these results are correct, the housing wealth effect is indeed an important channel through which housing affects the economy, and policymakers at the Fed are correct to take this channel into account when formulating policy.

The theoretical underpinnings of the housing wealth effect, however, remain controversial. Buiter (2008) shows that housing wealth effects should be of second-order importance for non-housing consumption. In his model, housing value increases result in higher housing consumption costs that offset the housing wealth effect on non-housing consumption; any effect from increases in housing values on non-housing consumption, therefore, should be small and primarily reflect the impact of the relaxation of borrowing constraints on consumers (given housing's special value as collateral for consumer borrowing). Sinai and Souleles (2005) present a somewhat different model of the housing wealth effect, but one with similar implications to Buiter (2008): increases in housing wealth should affect the distribution of consumption much more than its aggregate amount. The theoretical concerns raised by these articles are echoed by some policy makers: Buiter (2008) quotes Bank of England Governor Mervyn King who states that "housing wealth isn't wealth."

In addition to the lack of a clear theoretical rationale for a housing wealth effect, there are empirical and data problems that raise questions about past estimates. Carroll, et al. (2006) employ as their measure of housing wealth a residual metric from total wealth from the Flow of Funds accounts. Close inspection reveals that much of this measure – sometimes more than fifty percent – consists of non-housing wealth. Case. et al. (2005) employ a more reliable and

disaggregated measure of the stock of housing wealth for the fifty U.S. states, which is based on actual house price indices and the number of existing houses. These authors employ a panel estimation approach, rather than an aggregate time series, which increases the number of observations and the power of their tests. Their estimation strategy, however, suffers from possible endogeniety bias; as we will show, when one corrects for this endogeneity bias, their finding of a large wealth effect disappears.

In this paper, we investigate the housing wealth effect using the state-level Case-Quigley-Shiller (hereinafter CQS) data set (Case, et al., 2005). We employ lags for the housing and stock wealth variables, following a method to address simultaneity issues suggested by Hall (1978), Auerbach and Hassett (1989) and Campbell and Mankiw (1990). After investigating the robustness of our results across many different specifications, we find that, in all but four of the thirty-two specifications we investigate, housing wealth has no significant effect on consumption. In the four cases in which home prices appear to have some impact, the effect is significant only at the ten percent level, and its magnitude is less than the stock wealth effect, contrary to CQS, Carroll et al. (2006), and the U.S. Federal Reserve.

We conclude that the housing wealth effect has been overstated. While we do not deny that housing is perhaps the most important sector of the economy with respect to business cycle fluctuations, that link likely reflects channels other than the housing wealth effect (Leamer 2007 concurs with that view).

This paper proceeds as follows. The following section describes the existing literature on the housing wealth effect. The next section describes our methodology. The fourth section discusses our results, and the fifth section concludes.

II. Previous Empirical Literature

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 unsurprising that a number of papers (Ludvigson and Steindel, 1999, is one of many examples) find a significant, positive consumption wealth effect from stock price increases.

Housing, like stock, is an asset, and there have been a number of studies investigating whether housing wealth affects consumption. The model employed by the Federal Reserve assumes that the value of housing has the same effect on consumption as the value of financial assets. It may not be the case, however, that the wealth effects from housing and stocks are the same. CQS point out that, among other differences, households may not be able to easily measure wealth from an increase in home prices. Furthermore, an increase in home prices may be perceived as being more or less temporary than a rise in shares. Furthermore, as already noted, Buiter (2008) points out that houses are not simply assets, but rather are both assets and consumption goods. In theory, that fact should reduce the size of the wealth effect of housing wealth on non-housing consumption, and make wealth effects on consumption from housing wealth smaller than wealth effects from other wealth components.

Given the potential differences in housing and stock wealth for consumption, a number of studies have investigated the housing wealth effect empirically. Several studies employ micro data on households, with conflicting results. Campbell and Cocco (2007) find that, for British households, an increase in home values raises consumption.

Attanasio, Blow, Hamilton and Leicester (2009), on the other hand, employ micro-level British survey data, and find no wealth effect from house prices. Their results are particularly damaging to the notion of a large housing wealth effect. The authors posit that the correlation between housing wealth and consumption may arise from three possibilities: a straightforward wealth effect, the relaxation of credit constraints, or the response of both housing wealth and consumption to common third factors, such as expected future income. The authors find that the estimated "wealth" effect is the same for both homeowners and renters, which casts the credit constraint channel in doubt. Moreover, the authors find the purported impact on consumption from house prices is greater for young than for older households. This makes a straightforward wealth effect very questionable, as older households are most likely to own houses, while younger households are more likely to be saving for a home purchase, and if anything may theoretically experience a negative wealth effect. Levin (1998), similarly, finds virtually no housing wealth effect on consumption in the United States.

Several studies using aggregate data find a significant impact of housing on consumption. A number of such studies, until the last several years, relied on cointegration analysis, as permanent income/life cycle models of consumption implied a stable long-run relationship between consumption and forms of wealth such as housing or stock holdings. These studies, however, often failed to find stable cointegrating vectors, which does not surprise Carroll, et al. (2006). These authors point out that

> …cointegration models implicitly require the existence of a stable long-run relationship between consumption, labor income, and wealth. Theory implies no such stability, unless every major facet of the economy is perpetually unchanging. Even for the U. S., the 50 year span of available data has seen major changes in taxes, productivity growth, financial structure, social insurance , and every other aspect of reality incorporated in the theory (and embodied in the cointegrating vector). It is unsurprising, therefore, that empirical tests strongly suggest instability in the cointegrating vectors. (p. 1)

Thus while still desiring to imbed the estimation of wealth effects in a permanent income consumption framework, Carroll, et al. eschew cointegration and follow an estimation approach that builds on Hall (1978).

In Hall's canonical specification, if agents are Permanent Income Hypothesis (PIH) consumers, and there are no market imperfections such as credit constraints, consumption should follow a random walk. In particular, if consumption is a random walk, then:

$$C_t = C_{t-1} + \varepsilon_t \tag{1}$$

or equivalently,

$$\Delta C_t = \varepsilon_t \tag{2}$$

where ε_t is random error – in this case, changes in expected permanent income.

To test the random walk hypothesis, (2) is augmented by adding variables such as changes in stock or housing wealth or past changes in income or consumption. Since changes in expected permanent income (ε_t) are likely correlated with changes in current transitory income or wealth, these variables cannot be entered directly into (2). Instead, instruments (any lagged value of changes in wealth, consumption or income are valid) must be employed.

Hall (1978) initially found only lagged stock prices to be significant as regressors in (2), giving some support to the random walk hypothesis. In contrast, other authors, such as Campbell and Mankiw (1990) have found later lags of consumption to be significant, suggesting substantial deviations from the PIH, perhaps in the form of credit constraints.

Carroll, et al. (2006) build a model that explicitly takes account of consumption persistence, and thus deviates from Hall's assumption of a random walk for consumption. Theoretically, Carroll, et al. (2006) develop a model of habit formation to allow for the possibility that changes in wealth can exert effects on consumption. They also introduce other

innovations in the measurement of consumption change over time, which take into account of the implications of persistence in consumption. The authors find that, when lags of measured housing and stock wealth are entered into an empirical consumption model, both have significant effects. Indeed, the impact of housing wealth is larger than that of stock wealth, and that effect is much larger in the long run than in the short run.

Carroll, et al. (2006) present results that are motivated by a clear theoretical framework. There are, however, two potential problems with their results. First, consumption persistence may reflect adjustment costs in consumption behavior rather than habit formation. This is an important distinction because it implies that lagged consumption is not a sufficient statistic for forecasting future permanent income. The lagging responsiveness of consumption to permanent income shocks complicates the problem of endogeneity bias when measuring the effects of wealth change on consumption. If housing and stock prices anticipate and are endogenous to changes in permanent income, and if consumption adjusts to these changes in permanent income with a lag, then a large housing wealth effect on current consumption (large positive coefficients on lagged housing wealth in a regression that includes lagged consumption and lagged housing wealth) may simply indicate a speedier adjustment of housing wealth to permanent income shocks, rather than any causal relationship between housing wealth increases and subsequent consumption.

Carroll, et al. (2006) recognize that their specification for the housing wealth effect, which does not even include lagged consumption in the equation measuring the impact of lagged housing wealth on current consumption, can be criticized for potential endogeneity bias. They argue, however, that the fact that housing wealth effects appear both in aggregate studies and in microeconometric studies of individual behavior suggests that endogeneity bias is small.

We do not agree with that conclusion. Endogeneity bias can be important in studies of household-level consumption. For example, if consumers who bid up prices of houses do so because they anticipate a rises in income that are location-specific, then that expected future rise in income and consumption may be reflected first in rising house prices.

The second potential problem with Carroll, et al.'s (2006) analysis involves possibile measurement error in their measure of housing wealth. The authors use Flow of Funds data from the Federal Reserve, and measure housing wealth as total net worth minus stock wealth. However, this residual net worth includes assets besides stocks and housing, such as consumer durables, equipment and software, non-housing real estate holdings, wealth owned by non-profit organizations, and non-equity financial assets. In the fourth quarter of 2004, for instance, (the last observation in Carroll, et al.'s sample), more than half of "housing wealth" consists of non-housing wealth: real estate holdings by households (over 16.5 trillion dollars) are less than half the value of net worth minus stock wealth (over 33.4 trillion).

Below we investigate the potential importance of endogeneity bias for the measurement of housing wealth effects. We also explore the implications of correcting the aggregate housing wealth measure by Carroll, et al. (2006) to exclude non-housing assets. Of course, even that corrected, aggregate measure is likely to be a poor one. The difficulty of getting good measures of housing wealth has plagued studies of the consumption wealth effect. Muellbauer (2007), for instance, similarly finds a positive wealth effect from housing using aggregate U.S. data. However, he notes that U.S. aggregate data are not very informative (compared with that of the UK) and regards his results as "preliminary" (p. 269, 307).

CQS adopt a somewhat different approach to measuring the housing wealth effect. They begin by noting that "[r]esearch designed to quantify an effect of changes in wealth on changes

in consumption, going back to Ando and Modigliani (1963), has largely used aggregate measures of wealth that emphasize the stock market and make no credible attempt to measure housing wealth with any accuracy." (p. 4).

CQS develop two alternative sets of data to investigate the housing wealth effect. The first is a panel of national-level data covering fourteen different countries. The authors concede that "it relies upon consumption measures derived from national income accounts, not our imputations, and there is reason to suspect that housing prices and housing wealth in this panel are measured less accurately" (p. 8).

CQS also develop a second panel dataset for the fifty U.S. states, at quarterly frequency, for the years 1982-1999. Their measure of consumption is retail sales, provided by Regional Financial Associates. Importantly, rather than relying on a residual (and poorly measured) metric for house wealth in the U.S., the authors construct a house wealth index for each state. The authors construct their measure based on the number of households in each state, as well as the homeownership rate. They assemble state-by state data on home prices for each quarter, employing the Case-Shiller index for the states in which that index was available, and the OFHEO home price index where the Case-Shiller index lacked coverage. For purposes of measuring housing wealth effects, the OFHEO index is less precise than the Case-Shiller index because it does not weight houses by their value, but rather, assigns equal weight to all houses.

The CQS study, then, employs the most detailed measure of housing wealth, in terms of the number of homes owned and their value, of any study on the wealth effect of housing of which we are aware.[1] The authors proceed to estimate the effects of wealth on consumption in a

[1] Both indexes 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

variety of ways, such as modeling the level of consumption as a function of the level of stock and housing wealth, as well as income. Alternatively, they model the difference in consumption as a function of differences in housing, stock wealth and income. Case et al. (2005) also estimate a version of an error correction model, in which the parameters of the cointegrating vector are imposed (income affects 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 impact of housing wealth is larger than that of stock wealth. Case et al. (2005) also perform an important robustness check. They test for asymmetric impacts of housing wealth, and find that, while an increase in house wealth raises consumption, a decrease in house wealth has no effect on consumption.

While the improved measurement of housing wealth is a major contribution of CQS, there are important empirical problems with their estimation method. First, two of their estimations simply regress levels of consumption on levels of income, without taking account of the stationarity properties of the variables. The fact that the authors estimate error-correction models later highlights the importance of accounting for what has typically been found to be the nonstationarity of consumption and income. The authors do not test for cointegration (which Carroll, et al. 2006, criticize), but rather, in their cointegration specifications they simply impose a cointegrating vector with a parameter of one for the effect of income on consumption.

There is one specification which does appear to address the nonstationarity issue without imposing cointegration. In Table 3, CQS regress the difference of consumption on the differences of housing and stock wealth and income, again, finding a positive, significant impact

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the indexes may provide an exaggerated picture of housing price decline. Indeed, Leventis (2008) 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. Nevertheless, during the sample period employed by Case et al. (2005), when neither a subprime boom or bust was occurring, selectivity bias should be much less.

of housing wealth. This specification also comes close to the standard studies of consumption such as Hall (1978) and Campbell and Mankiw (1990).

But there is one important difference between the empirical CQS models and these former studies. CQS regress the current change in consumption on the *current* change in income, housing and stock wealth. This causes a potentially severe problem of endogeneity. Because they do not control for shocks 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 expectations of future income growth, which may produce improvements in many current market indicators, including rising home values.

Referring back to the pure random walk theory, if the PIH holds, the ε_t term in (2) represents only unexpected changes in permanent income. If the PIH does not hold perfectly, due, for example, to the existence of credit constraints, habit formation, or adjustment costs in consumption, then ε_t represents a combination of unexpected changes in permanent income, expected and unexpected changes in current transitory income, expected changes in permanent income, and expected and unexpected changes in housing and stock wealth. However, changes in stock and housing wealth are likely correlated with other changes in expected and unexpected permanent and transitory income. Thus using current changes in housing or stock wealth as regressors in (2) will lead to inconsistent estimates. In particular, one may ascribe to housing and stock wealth a causal impact on consumption that really reflects anticipated or unanticipated changes in permanent or transitory income.

These potential problems are especially worrying given the theoretical perspectives that imply small housing wealth effects on consumption, noted above. According to Buiter (2008), houses are both assets and consumption goods that derive value from the shelter services they provide. In this scenario, a drop in housing prices hurts those "long" housing-those who own more housing than they plan to consume over their lifetime, and helps those "short" housing-those currently renting who may plan to buy in the future. On average, since a country's residents own their own housing, there should be no wealth effect, save for a distributional effect.

Sinai and Souleles (2005) present a model with related implications. They assume that agents must live somewhere, and thus face "liabilities" in terms of future housing costs. Thus homeowners who experience an increase in home prices have experienced an increase in the price of an asset they own, but also an equivalent increase in their future rental liabilities. The logic of this model suggests little if any wealth effect from housing, as any increase in "wealth" is offset by an increase in housing liabilities. Given the conflicting beliefs and theories about the housing wealth effect, the actual impact of a change in house prices on consumption is an empirical question, which should be investigated in a PIH framework, using reliable data, and avoiding potential endogeneity bias from the correlations between the components of income and housing wealth.

III. Data and Methodology

Given the problems associated with previous attempts to measure housing wealth noted by CQS, we employ their dataset, which we consider the most carefully constructed measure of

housing wealth for the United States of which we are aware.[2] For comparison with time-series studies, we use Carroll, et al.'s (2006) U.S. data for our aggregate time series tests.[3]

CQS start with (2) and seek to augment the model and estimate the following:

$$\Delta C_t = \beta_0 + \beta_1 \Delta HW_t + \beta_2 \Delta SW_t + \beta_3 \Delta Y_t + \varepsilon_t \qquad (3)$$

where *HW* is housing wealth, *SW* is stock wealth, and *Y* is personal income. Case et al. (2005) test the significance of the β_1 parameter and conduct a test for whether β_1 is equal to β_2. They are able to reject these hypotheses in almost all cases, since the estimate of β_1 is usually much larger than that of β_2.

As noted above, however, estimating (3) directly leads to inconsistent results, since the residual will contain changes in expected permanent income, and changes in current income, and these will likely be highly correlated with changes in housing and stock wealth. This point is well-articulated in Campbell and Mankiw (1990), p. 266. In order to estimate whether a variable – current changes in housing wealth, stock wealth, income, etc. – affects current consumption, it is necessary to use instrumental variables to get consistent estimates. Since ε_t is a residual and reflects only new information, any variable that is known at $t-1$ and is correlated with ΔC_t is a valid instrument. It is thus standard practice when testing the PIH (going back to Hall, 1978), to use lags of consumption or income changes as instruments in (3). We will accordingly use lags of housing and stock wealth (as well as income or consumption changes) as instruments in (3).[4]

[2] In each of our models, we transform the raw variables as the log difference of their real, per capita values. For a complete description of how the data are constructed, see CQS. The complete dataset is available on John Quigley's personal website at http://urbanpolicy.berkeley.edu/data/CQSData072205.xls.

[3] Once again, we transform all variables into log differences of their real, per capita values. Carroll, et al.'s data is available on Christopher Carroll's personal website at http://www.econ.jhu.edu/people/ccarroll/housing.html.

[4] Muellbauer (2007) also re-examines the CQS data. However, in his re-examination, the author includes current income as a regressor (p. 283), which, as discussed, is endogenous, making his results subject to endogeneity bias.

IV. Panel Results

Table 1 shows the results of re-estimating CQS's model with a simple correction for the endogeniety problems discussed above. In these regressions, the dependent variable is the contemporaneous change in consumption (quarterly log difference of retail sales), while the independent variables are all lagged log differences. As in CQS, we present both OLS and Prais-Winsten estimates (to control for possible autocorrelation induced by the time series component of the panel), and we also run the model with and without state and quarter (seasonal) fixed effects.

In each of these specifications, stock wealth has a positive, significant effect on next quarter's consumption. In contrast, changes in housing wealth typically have no impact on future consumption; the estimated housing wealth effect is small and statistically insignificant in six of the eight specifications considered. The estimated housing wealth effect is significant only when state fixed effects are excluded from the model, and even then only at the 10 percent level and with a magnitude that is smaller than the estimated stock wealth effect.[5] We note that the coefficient on lagged income changes is negative, but as Romer (p. 341) notes, lagged changes in income may be of little value in forecasting future income.

The results in Table 1 show that lagged changes in housing wealth have no measurable impact on current changes in consumption. Contemporaneous housing wealth effects, however, may not be fully captured by this specification. In Table 2 we present results from a two-stage least squares (2SLS) regression of instrumented contemporaneous changes in housing wealth, stock wealth and income on changes in consumption. In the first four columns of this table, the

[5] The differences are not statistically significant, however.

second through fourth lags of the exogenous variables are used as instruments, while in the last four columns the second through sixth lags are used as instruments.[6]

As in Table 1, we find that the housing wealth effect is small and statistically-insignificant, regardless of the number of lags or fixed-effects dummies employed. Similarly, the stock wealth effect is always larger in magnitude than the estimated housing wealth coefficient and is always statistically significant. Finally, note our estimated income coefficients fall within the 0.3-0.7 range found by Campbell and Mankiw (1990).

In Table 2, we used the specified lags of the exogenous variables as instruments. As a robustness check, we ran one alternative specifications using lagged consumption in place of lagged income as instruments and another using both lagged income and consumption as instruments. The results appear in the appendix in Tables A1 and A2. When lags of consumption are used as instruments (Table A1), the estimated housing wealth effect is somewhat larger than that found in Table 2, but is only statistically significant (at the 10 percent level) in four of the eight specifications. In every case, however, the estimated wealth effect is smaller in magnitude and less significant than the stock wealth effect, in direct contrast to the findings of CQS. When both lagged income and consumption are used as instruments (Table A2), the housing wealth effect is somewhat larger and more significant when instrumenting with the 2^{nd} through 4^{th} lags. As before, however, the stock wealth effect is larger and more significant than the housing wealth effect. Interestingly, the larger the estimated income coefficient, the smaller and less significant the housing wealth effect (which occurs when the 2^{nd} through 6^{th} lags are used).

[6] Following Campbell and Mankiw (1990), we use twice-lagged values as instruments to avoid the measurement problem induced by the fact that the CQS consumption and income data are quarterly averages rather than values at a point in time.

We summarize all of these results in Table 3. In all, we find a positive and significant housing wealth effect in nine of the 24 specifications we ran, and only at the five percent level or higher when the 2nd through 4th lags of consumption and income are used as instruments. In every instance, however, the estimated wealth effect was smaller in magnitude and statistical significance than the stock wealth effect. These results suggest that the housing wealth effect found by CQS in fact reflects the fact that changes in housing wealth are correlated with changes in permanent income. Once we control for this endogeneity, we find a much smaller housing wealth effect, consistent with the theoretical results of Buiter (2008) and Sinai and Souleles (2005).

V. Time Series Analysis

In the previous section, we used CQS's state-level panel data to show that prior estimates of a large housing wealth effect likely reflect an endogeneity problem. Because changes in housing wealth are correlated with changes in permanent income, failing to control for this correlation will lead to a biased estimate of the impact of housing wealth on non-housing consumption.

In our opinion, these panel data provide two distinct advantages over national time series data. First, the CQS data provide a more accurate measure of housing wealth than the Federal Reserve Flow of Funds Accounts that are typically used in pure time series analyses. Furthermore, the more disaggregated structure of the database enhances the statistical power of our regressions. Thus, we believe that the panel data results presented in Section IV are more compelling than those coming from time series data.

Nevertheless, given that much of the housing wealth effect literature uses aggregate time series data, it is worthwhile to investigate the extent to which this endogeneity bias may be affecting time series estimates of the housing wealth effect. To do this, we re-run the analysis of Section IV using the aggregate U.S. time series data employed by Carroll, et al. (2006).

The results of OLS regressions using lagged regressors are shown in Table 4. The first four columns of this table report the results of regressions in which housing wealth is defined as non-stock wealth, following Carroll, et al. (2006). Although our methodology differs substantially from theirs, we similarly find a large and significant housing wealth effect. This is true whether we use one lag or four, and whether we include lagged income or consumption as instruments.

As discussed above, however, Carroll, et al. (2006) measure housing wealth as total net worth minus stock wealth using Flow of Funds data.[7] This residual net worth includes assets besides stocks and housing, such as consumer durables, equipment and software, non-housing real estate holdings, wealth owned by non-profit organizations, and non-equity financial assets. In the fourth quarter of 2004, for instance, (the last observation in Carroll, et al.'s sample), more than half of "housing wealth" consists of non-housing wealth.

Accordingly, we re-run our analysis using real estate owned by households as the measure of housing wealth, the results of which appear in the last four columns of Table 4.[8] With only one lag, the estimated housing wealth effect is much smaller than it was using Carroll, et al.'s non-stock wealth, and the effect is now statistically insignificant. When four lags are employed, the housing wealth effect is significant at the first three lags, but the sign changes

[7] Specifically, Carroll, et al. (2006) use Federal Reserve Flow of Funds data and define stock wealth as equity by households, corporate equity by private pension funds, government retirement funds, bank trusts and estates, closed end funds, mutual funds and life insurance companies. Non-stock wealth is then defined as total household and non-profit net worth minus stock wealth. All of these variables are expressed in real, per capita terms.
[8] Technically, we use the log difference of real, per capital real estate owned by households.

signs, with a positive effect at the first and third lags and a negative effect at the second and fourth lags.[9] As a result, the cumulative (four quarter) effect is very small and statistically insignificant. Taken as a whole, the results in Table 3 suggest that the large housing wealth effect estimated in prior studies may be biased by the choice of housing wealth variable employed.

As a further test of the potential for endogeneity bias, we break down durables and non-durables consumption to see how each are affected by changes in housing wealth. Because housing wealth should theoretically affect consumption primarily by relaxing borrowing constraints, we posit that housing wealth effects should be larger for durables than for non-durables (since lumpy durables should be more sensitive to the relaxation of borrowing constraints). On the other hand, if the housing wealth effect reflects the predictive role of housing wealth for permanent income, there should be little difference in the response of durables and non-durables consumption to housing wealth (since permanent income should affect durables and non-durables equally).

The results of this analysis appear in Table 5. Using either non-stock wealth or real estate owned by households as our proxy for housing wealth, we find a significant cumulative housing wealth effect only for non-durables. Durables do generally show significant effects at the first, third and fourth lags, but the signs of these effects at different lags offset one another. In contrast, changes in housing wealth affect non-durables only at the first lag, with later lags having only small and insignificant effects. We think these comparative results are best explained as reflecting simultaneity bias, which drives the short-term correlation between non-durables consumption and one-quarter lagged housing wealth.

[9] The results are similar regardless of whether consumption or income or both are used as instruments.

Finally, in results not reported here, we also ran two-stage least squares (2SLS) regressions of instrumented contemporaneous changes in housing wealth, stock wealth and income on changes in consumption using time series data. Regardless of the number of lags used, neither housing nor stock wealth is significant in these specifications. We believe this lack of significance for either variable is attributable to noisy instrumenting in the time series regressions; the variance of the 2SLS estimate is larger than that of OLS (i.e. 2SLS entails larger standard errors), reflecting the fact that time series aggregation reduces the precision of statistical inference compared to the CQS panel data set.

VI. Conclusion

The Federal Reserve Board employs a model for the U.S. economy in which the housing wealth effect is constrained to be the same as that of stocks. Former Fed governor Frederic Mishkin (2007) performs several simulations in which he allows for the possibility that the impact of housing wealth may be double that of equity wealth (see Buiter, 2007, p. 32).[10] Fears of falling house prices dampening consumer spending have been cited by both market participants and government officials, and have guided policymaking.

Our panel estimation results reported in Section IV indicate that past estimates of a large housing wealth effect on consumption likely reflect an endogeneity bias. When we control for this bias, we find that changes in housing wealth have perhaps no significant effect on consumption spending, or, at the very most, a positive impact that is much smaller than that of equity wealth. Consistent with Buiter's (2008) and Sinai and Souleles' (2005) theoretical models, we find evidence for the view that housing, unlike stock wealth, has a weak effect on

[10] Mishkin acknowledges, however, that the magnitude of the housing wealth effect is uncertain, and there are sensible arguments why they should be very small.

consumption, reflecting the fact that housing is not simply an asset such as equity; rather it is primarily a consumption good, and as such any wealth effects, if the exist at all, are highly muted.

Panel data on housing wealth at the state level are more accurate than flow of funds estimates, and the panel approach offers more powerful tests, due to greater degrees of freedom. Nevertheless, in Section V, we provide additional analysis of time series relationships, using flow of funds data to measure housing wealth. We find that the results are highly sensitive to the choice of housing wealth measure, and when we use a "pure" measure of housing wealth (real estate owned by households), we find no significant housing wealth effect.

If this interpretation is correct, it has profound policy implications. The current decline in housing prices, driven by a combination of the bursting of the housing bubble, the recession, and the credit crunch, is not likely to exert an independent negative effect on consumption. If that is true, than macroeconomic stabilization policies that focus on income and employment stabilization will have a greater impact on consumption than policies that attempt to support house prices.

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Table 1 – Panel data 2SLS wealth effect estimates (with and without state and time fixed-effect controls)

	OLS Estimates	PW Estimates	OLS Estimates	PW Estimates	OLS Estimates	PW Estimates	OLS Estimates	PW Estimates
Housing wealth	0.002	0.014	0.001	0.014	0.005	0.017	0.004	0.018
	(0.830)	(0.171)	(0.886)	(0.160)	(0.636)	(0.091)	(0.687)	(0.084)
Stock wealth	0.021	0.025	0.013	0.019	0.021	0.025	0.013	0.019
	(0.002)	(0.000)	(0.056)	(0.003)	(0.001)	(0.000)	(0.052)	(0.003)
Income	-0.080	-0.024	-0.135	-0.064	-0.069	-0.012	-0.126	-0.054
	(0.002)	(0.348)	(0.000)	(0.009)	(0.006)	(0.634)	(0.000)	(0.029)
R-square	0.0529	0.0503	0.0193	0.0191	0.0435	0.0373	0.0092	0.0052
State dummies	Yes		Yes		No		No	
Quarter dummies	Yes		No		Yes		No	

Notes: P-values shown in parentheses below the estimates. The dependent variable is the contemporaneous change in consumption (quarterly log difference of state-level retail sales), while independent variables are lagged log differences. OLS stands for ordinary least squares estimates, while PW stands for Prais-Winsten estimates.

In the first column, OLS estimates show a very small, statistically-insignificant housing wealth effect. In the second column, Prais-Winsten estimates (used to control for possible autocorrelation problems) also show a statistically-insignificant wealth effect. The third through eighth columns present robustness checks that test whether the estimated housing wealth effect is affected by the presence of state and quarter (seasonal) dummy variables. The estimated housing wealth effect is small in all cases, and significant at the 10 percent level only in the Prais-Winsten estimates without state fixed-effects.

Table 2 – Panel data 2SLS wealth effect estimates (with and without state and time fixed-effect controls)

	2^{nd}-4^{th} Lags Used as Instruments				2^{nd}-6^{th} Lags Used as Instruments			
Housing wealth	0.065	0.033	0.071	0.040	0.055	0.024	0.026	0.026
	(0.369)	(0.666)	(0.325)	(0.590)	(0.354)	(0.682)	(0.648)	(0.648)
Stock wealth	0.210	0.227	0.213	0.230	0.149	0.181	0.182	0.182
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Income	0.472	0.589	0.473	0.583	0.441	0.604	0.607	0.607
	(0.013)	(0.000)	(0.013)	(0.000)	(0.005)	(0.000)	(0.000)	(0.000)
F-test p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0090	0.0000	0.0000
State dummies	Yes	Yes	No	No	Yes	Yes	No	No
Quarter dummies	Yes	No	Yes	No	Yes	No	Yes	No

Notes: P-values shown in parentheses below the estimates. The dependent variable is the contemporaneous change in consumption (quarterly log difference of state-level retail sales), while independent variables are contemporaneous log differences of the listed variables, instrumented by the indicated lags of all exogenous variables.

Regardless of the number of lags used to instrument the dependent variables or whether state or quarter fixed effects are included, the estimated housing wealth effect is small and statistically insignificant. In contrast, we find a large and significant stock wealth effect.

Table 3 – Panel data robustness check summary

Instruments Used	Lags	State & Time Dummies	State Dummies	Time Dummies	No Dummies
Income	2-4	0.065 (0.369)	0.033 (0.666)	0.071 (0.325)	0.040 (0.590)
Consumption	2-4	0.112 (0.144)	0.102 (0.179)	0.127* (0.085)	0.118 (0.109)
Both	2-4	0.164** (0.027)	0.144* (0.060)	0.167** (0.021)	0.149** (0.047)
Income	2-6	0.055 (0.354)	0.024 (0.682)	0.026 (0.648)	0.026 (0.648)
Consumption	2-6	0.105* (0.091)	0.094 (0.111)	0.103* (0.071)	0.103* (0.071)
Both	2-6	0.102* (0.075)	0.077 (0.180)	0.079 (0.160)	0.079 (0.160)

Notes: P-values shown in parentheses below the estimates; * significant at the 10 percent level; ** significant at the 5 percent level.

This table summarizes the estimated housing wealth effect for the combinations of fixed-effect controls (state and time dummy variables), instrument lags (2-4 or 2-6), and the instruments used in addition to lagged housing wealth and stock wealth (income, consumption, or both). Cell entries are the estimated housing wealth effect (and p-value) from the regressions reported in Tables 2, A1 and A2.

The estimated housing wealth effect was insignificant in all but nine of the 24 specifications, and significant at the 5 percent level in only three instances. In each case, the magnitude of the estimated housing wealth effect is smaller and less significant than the estimated stock wealth effect.

Table 4 – Time Series Results for Aggregate Consumption

	Definition of "Housing Wealth"							
	NSW	*NSW*	*NSW*	*NSW*	*HHRE*	*HHRE*	*HHRE*	*HHRE*
HW (-1)	0.150	0.181	0.227	0.197	0.043	0.068	0.083	0.067
	(0.036)	(0.018)	(0.002)	(0.010)	(0.253)	(0.081)	(0.033)	(0.093)
HW (-2)		-0.128	-0.128	-0.141		-0.071	-0.066	-0.069
		(0.113)	(0.086)	(0.074)		(0.085)	(0.093)	(0.093)
HW (-3)		0.286	0.271	0.317		0.072	0.071	0.079
		(0.000)	(0.000)	(0.000)		(0.073)	(0.064)	(0.047)
HW (-4)		-0.145	-0.175	-0.185		-0.060	-0.068	-0.064
		(0.056)	(0.015)	(0.016)		(0.121)	(0.066)	(0.099)
SW (-1)	0.024	0.025	0.024	0.024	0.023	0.024	0.022	0.022
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
SW (-2)		0.011	0.008	0.006		0.013	0.010	0.010
		(0.042)	(0.128)	(0.222)		(0.013)	(0.051)	(0.072)
SW (-3)		0.002	-0.003	-0.001		0.000	-0.005	-0.004
		(0.699)	(0.621)	(0.789)		(0.984)	(0.366)	(0.481)
SW (-4)		0.010	0.007	0.007		0.011	0.009	0.008
		(0.053)	(0.152)	(0.200)		(0.034)	(0.103)	(0.126)
Inc. (-1)	0.184	0.157		0.102	0.208	0.164		0.104
	(0.001)	(0.003)		(0.084)	(0.000)	(0.002)		(0.086)
Inc. (-2)		0.057		-0.027		0.084		-0.003
		(0.284)		(0.668)		(0.118)		(0.958)
Inc. (-3)		-0.045		-0.080		0.008		-0.033
		(0.406)		(0.189)		(0.876)		(0.602)
Inc. (-4)		-0.020		0.016		-0.007		0.006
		(0.701)		(0.791)		(0.897)		(0.925)
Cons. (-1)			0.212	0.167			0.208	0.155
			(0.003)	(0.032)			(0.004)	(0.053)
Cons. (-2)			0.079	0.086			0.122	0.109
			(0.241)	(0.261)			(0.079)	(0.174)
Cons. (-3)			0.074	0.124			0.077	0.104
			(0.262)	(0.102)			(0.258)	(0.190)
Cons. (-4)			-0.164	-0.165			-0.111	-0.114
			(0.012)	(0.022)			(0.102)	(0.133)
R-Square	0.1849	0.3044	0.3323	0.3509	0.1725	0.2664	0.2886	0.3013
Cum. HW		0.194	0.195	0.188		0.009	0.020	0.013
		(0.057)	(0.050)	(0.067)		(0.875)	(0.729)	(0.813)
Cum. SW		0.048	0.036	0.036		0.048	0.036	0.036
		(0.000)	(0.000)	(0.001)		(0.000)	(0.001)	(0.001)

Notes: P-values shown in parentheses below the estimates. HW refers to the housing wealth coefficients, whereas SW refers to the stock wealth coefficients. In the first four columns, non-stock wealth (NSW) is used as the measure of housing wealth, while real estate owned by households (HHRE) is used in the final four columns. The dependent variable is the log change in real, per capital total personal consumption expenditures, while all dependent variables are log-differences of the real, per capita values of the respective variables.

Table 5 – Durables Consumption Wealth Effects

	Definition of "Housing Wealth" and Dependent Variable							
	NSW Dur.	*NSW Dur.*	*NSW Nondur.*	*NSW Nondur.*	*HHRE Dur.*	*HHRE Dur.*	*HHRE Nondur.*	*HHRE Nondur.*
HW (-1)	0.794	0.847	0.264	0.266	0.409	0.424	0.108	0.111
	(0.030)	(0.023)	(0.001)	(0.001)	(0.031)	(0.028)	(0.008)	(0.009)
HW (-2)	-0.504	-0.485	-0.031	-0.040	-0.322	-0.308	0.000	-0.008
	(0.176)	(0.207)	(0.707)	(0.633)	(0.093)	(0.115)	(0.994)	(0.859)
HW (-3)	1.177	1.188	0.079	0.092	0.425	0.416	0.002	0.011
	(0.002)	(0.002)	(0.334)	(0.267)	(0.025)	(0.031)	(0.963)	(0.803)
HW (-4)	-0.831	-0.780	0.064	0.059	-0.367	-0.354	0.025	0.027
	(0.021)	(0.036)	(0.418)	(0.463)	(0.044)	(0.056)	(0.527)	(0.513)
SW (-1)	0.092	0.088	0.021	0.021	0.081	0.079	0.020	0.020
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.002)	(0.001)	(0.000)
SW (-2)	0.092	0.087	-0.004	-0.005	0.100	0.094	-0.003	-0.004
	(0.000)	(0.002)	(0.529)	(0.428)	(0.000)	(0.001)	(0.566)	(0.459)
SW (-3)	0.005	0.002	-0.001	-0.004	-0.004	-0.008	-0.002	-0.005
	(0.846)	(0.937)	(0.848)	(0.540)	(0.884)	(0.775)	(0.704)	(0.390)
SW (-4)	0.017	0.021	0.010	0.009	0.025	0.029	0.010	0.008
	(0.518)	(0.442)	(0.061)	(0.142)	(0.349)	(0.289)	(0.073)	(0.171)
Dur. (-1)	-0.038	-0.046		0.018	-0.031	-0.036		0.021
	(0.588)	(0.520)		(0.249)	(0.665)	(0.620)		(0.187)
Dur. (-2)	0.047	0.045		0.015	0.058	0.050		0.017
	(0.483)	(0.517)		(0.318)	(0.390)	(0.473)		(0.276)
Dur. (-3)	-0.029	-0.026		-0.005	-0.032	-0.031		-0.005
	(0.653)	(0.698)		(0.730)	(0.623)	(0.649)		(0.738)
Dur. (-4)	-0.109	-0.098		-0.017	-0.089	-0.086		-0.016
	(0.091)	(0.140)		(0.237)	(0.176)	(0.208)		(0.281)
Nondur. (-1)		0.077	0.153	0.138		0.073	0.176	0.157
		(0.818)	(0.032)	(0.059)		(0.826)	(0.014)	(0.033)
Nondur. (-2)		0.030	0.170	0.170		0.165	0.203	0.201
		(0.930)	(0.018)	(0.021)		(0.623)	(0.005)	(0.007)
Nondur. (-3)		-0.169	0.059	0.071		-0.178	0.074	0.084
		(0.613)	(0.408)	(0.330)		(0.593)	(0.308)	(0.249)
Nondur. (-4)		-0.273	-0.137	-0.118		-0.190	-0.112	-0.093
		(0.400)	(0.048)	(0.095)		(0.559)	(0.111)	(0.193)
R-Square	0.2209	0.2255	0.2647	0.2792	0.2076	0.2111	0.2303	0.247
Cum. HW	0.636	0.770	0.376	0.377	0.145	0.178	0.135	0.141
	(0.163)	(0.124)	(0.000)	(0.001)	(0.594)	(0.531)	(0.025)	(0.026)
Cum. SW	0.206	0.198	0.026	0.021	0.202	0.194	0.025	0.019
	(0.000)	(0.000)	(0.016)	(0.060)	(0.000)	(0.000)	(0.032)	(0.112)

Notes: P-values shown in parentheses below the estimates. HW refers to the housing wealth coefficients, whereas SW refers to the stock wealth coefficients. In the first four columns, non-stock wealth (NSW) is used as the measure of housing wealth, while real estate owned by households (HHRE) is used in the final four columns. The dependent variable is the log change in real, per capital total personal consumption expenditures, while all dependent variables are log-differences of the real, per capita values of the respective variables.

Table A1 – IV wealth effect estimates using lagged consumption in place of lagged income as instruments

	2^{nd}-4^{th} Lags Used as Instruments				2^{nd}-6^{th} Lags Used as Instruments			
Housing wealth	0.112	0.102	0.127	0.118	0.105	0.094	0.103	0.103
	(0.144)	(0.179)	(0.085)	(0.109)	(0.091)	(0.111)	(0.071)	(0.071)
Stock wealth	0.182	0.200	0.194	0.213	0.118	0.097	0.107	0.107
	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.035)	(0.021)	(0.021)
Income	0.127	0.232	0.148	0.240	0.149	0.207	0.227	0.227
	(0.463)	(0.123)	(0.389)	(0.108)	(0.358)	(0.144)	(0.105)	(0.105)
F-test p-value	0.0001	0.7841	0.0000	0.0000	0.0001	0.9083	0.0003	0.0003
State dummies	Yes	Yes	No	No	Yes	Yes	No	No
Quarter dummies	Yes	No	Yes	No	Yes	No	Yes	No

Notes: P-values shown in parentheses below the estimates. The dependent variable is the contemporaneous change in consumption (quarterly log difference of state-level retail sales), while independent variables are contemporaneous log differences of the listed variables, instrumented by the indicated lags of housing wealth, stock wealth and consumption.

Using lagged consumption rather than lagged income as an instrument has little effect on the results. As in Table 2, the estimated stock wealth effect is large and statistically significant, although the magnitude is somewhat smaller when the 2^{nd} through 6^{th} lags are used as instruments. The estimated housing wealth effect is somewhat larger, and is statistically significant at the 10 percent level in four of the eight specifications, although its magnitude is never larger than the stock wealth effect. Interestingly, the income coefficient ceases to be significant in these specifications.

Table A2 – IV wealth effect estimates using lagged consumption and lagged income as instruments

	2^{nd}-4^{th} Lags Used as Instruments				2^{nd}-6^{th} Lags Used as Instruments			
Housing wealth	0.164	0.144	0.167	0.149	0.102	0.077	0.079	0.079
	(0.027)	(0.060)	(0.021)	(0.047)	(0.075)	(0.180)	(0.160)	(0.160)
Stock wealth	0.236	0.273	0.241	0.278	0.143	0.172	0.173	0.173
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Income	0.184	0.300	0.202	0.305	0.300	0.448	0.461	0.461
	(0.285)	(0.045)	(0.243)	(0.042)	(0.036)	(0.000)	(0.000)	(0.000)
F-test p-value	0.0000	0.0002	0.0000	0.0000	0.0000	0.0124	0.0000	0.0000
State dummies	Yes	Yes	No	No	Yes	Yes	No	No
Quarter dummies	Yes	No	Yes	No	Yes	No	Yes	No

Notes: P-values shown in parentheses below the estimates. The dependent variable is the contemporaneous change in consumption (quarterly log difference of state-level retail sales), while independent variables are contemporaneous log differences of the listed variables, instrumented by the indicated lags of housing wealth, stock wealth, income and consumption.

When both lagged consumption and lagged income are used as instruments, the housing wealth effect is larger and more statistically significant than in other specifications (Tables 2 and A1). As before, however, the stock wealth effect is larger and more significant than the housing wealth effect. Interestingly, the larger the estimated income coefficient, the smaller and less significant the housing wealth effect.