How Significant are Housing and Financial Wealth Effects in the US Context? An Identification-Robust Re-Estimation.

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Abstract

This paper presents an identification-robust method for estimating the separate effects of housing and financial wealth on per-capita consumption, using the Anderson-Rubin procedure. Using this procedure,we provide a re-estimation of the model proposed in Carroll, Otsuka and Slacalek (2011); a model that exploits consumption persistence to concurrently identify both short-run and medium run wealth effects. Using U.S. data for two separate periods; one terminating prior to 2007-Q4 and the second containing data for the post-financial crisis period, we find conflicting results. For the period preceding the financial crisis, our results tend to support prior studies that found evidence of a relatively dominant housing wealth effect; however, the respective roles of financial and housing wealth in terms of per-capita consumption growth are reversed with the use of our updated data set. Ultimately, we argue that the method employed in this study yields estimates that based on theory, are statistically superior to the results of previous studies.

Introduction

Application of the life-cycle hypothesis of saving and consumption, as developed in Ando and Modigliani (1963), elicits the view that equivalent increases in wealth, regardless of their respective underlying components (ie, financial wealth or housing wealth), have an equivalent and positive effect on household consumption. As may be expected, a number of recent empirical papers examining the 'wealth effect', and its respective components, have dismissed this view as an over-simplification[1]. The fundamental criticism levelled by these various authors is premised on the assertion that the theoretical underpinnings of inferred marginal propensities to consume out of housing wealth and financial wealth aren't synonymous.

Regarding the housing wealth effect, a simple transmission mechanism linking housing wealth and consumption is described as follows: a rise in current housing prices raises the value of the current housing stock and subsequently housing wealth; in the aggregate, this translates to an expanded budget set for consumers, who then increase consumption accordingly. Implicitly, this channel treats housing wealth as an asset that is not differentiated from forms of wealth such as financial equities. And yet, we note that this view is embedded in macro-econometric models of the Federal Reserve Board, and receives support amongst prominent policy-makers[2].

The potential importance of the housing wealth effect - in terms of policies directed at aggregate consumption - is typically buttressed by two attributes of housing wealth. First, where it exists, the affect of housing wealth on consumption promotes the efficacy of monetary policy; since an expansionary policy is expected to raise housing prices through an increase in demand. It follows then that the larger the housing wealth effect, the easier it is for the Federal Reserve to engineer consumption growth. Secondly, within the US context, the stock of housing wealth accounts for almost 70%

[1] (see for example, Mishkin (2007); Calomiris, Longhofer and Miles (2009); Carroll, Otsuka and Slacalek (2011).)

[2] Ben Bernanke is cited as stating that a \$100 reduction in housing value due to a drop in housing prices would lead consumers "to spend between \$3 and \$5 less per year" (Bernanke (2012)).

of the median household's total wealth (Iacoviello (2010)). This suggests that the aggregate effect of housing wealth on consumption would presumably be large and significant [3]. Furthermore, relative to financial wealth, housing wealth is observed to show a higher level of persistence, which may lead households to attach a higher psychological value to increases in housing wealth. As highlighted by Case, Quigley and Shiller (2005), the psychological value argument points to an augmented housing wealth effect. However, the transmission mechanism outlined above and the overall size of the housing wealth effect remains disputed.

Ignoring questions of causality for the moment[4], the simple housing wealth mechanism fails to account for the costs associated with a shift in housing prices. Unlike financial equities, housing is itself a consumption good. A number of prominent studies (see for example, Muellbauer (2007); Buiter (2008)) point to a related attenuating effect. These studies suggest that an increase in housing prices would simultaneously lead to increased consumption costs for home-buyers, thereby promoting an increase in saving and a reduction of non-housing consumption. In this respect, the relative composition (home-owners/home-buyers) of consumers is proposed to influence the potential size of the housing wealth effect. Naturally, contending dynamics makes it difficult to make ex ante conclusions regarding the magnitude of price-driven housing wealth effects.

Over the last decade, a large number of empirical papers have examined the housing wealth effect. Among these studies, many have found evidence supporting a large wealth effect from housing (that exceeds estimated financial wealth effects). Three of

[3] In the January 25, 2006 edition of the Wall St. Journal, Justin Lahart cited the importance of this presumed effect, stating that "Housing is becoming a front-burner issue for Wall St. First of all, investors fret that because prices ran up by so much over the past several years, the real estate market could be in for more than a garden-variety slowdown. Second, they worry that because housing's strength has provided a big boost to consumer spending, even a garden variety slowdown could prompt big -time belt tightening."

[4] See Leamer (2007) or Disney, Gathergood and Henley (2008) for a discussion of alternative links between consumption growth and housing wealth; for example, a common consideration in both of these studies is that future expected income links the two series; thereby biasing any observed 'putative effects'.

these recent studies are particularly prominent in the academic literature (see Case et al. (2005); Carroll et al. (2011); Duca, Muellbauer and Murphy (2012)), and could be seen to demonstrate that policy-makers would be correct to incorporate a larger housing wealth effect in the formulation of respective macroeconomic models. At the same time, a consensus remains absent. Using the Carroll et al. (2006) aggregate time series data set, Calomiris, Longhofer and Miles (2009) find that the housing wealth effect is insignificant when an alternative measure of housing wealth is used. Similarly, using a model with endogenous house prices, Sinai and Souleles (2005) find evidence affirming that the housing wealth effect is negligible in the US, but the financial wealth effect is significant.

Given this context of discord, we re-examine the housing wealth effect using the Carroll et al. (2011) aggregate time series data set, in addition to a contemporary update of this data set. Importantly, by supplementing our analysis with the use of this period-extended sample, we are uniquely able to investigate potential complications related to the recent financial crisis. As in Carroll et al. (2011), our estimation approach is based on the application of instrumental variable techniques to the model of consumption stickiness developed by Carroll and Slacalek (2006). But, this paper adds to the literature by explicitly accounting for the potential issue of weak identification in the use of IV methods for this purpose. We emphasize that many recent studies have shown that in the presence of weak identification, standard asymptotic procedures are invalid (see the survey of Dufour (2003)). For this reason, various methods have been proposed to deal with the presence of weak instruments. As applied within the Canadian context by Kichian and Mihic (2015), this study circumvents potential issues of weak identification in the estimation of wealth effects with application of the Anderson-Rubin procedure. In so doing, we are able to produce more reliable estimates for the separate wealth effects. Our results tend to support the previous estimates of Carroll et al. (2011) for the period prior to the recent financial crisis; however, there are significant differences observed in our updated estimates.

This paper proceeds as follows. In section 2, we review the theoretical framework employed in Carroll et al. (2011)'s estimation of the respective financial and housing wealth effects. Section 3 provides a description of our empirical methodology. In Section 4 we discuss the data employed in this study. Section 5 presents our empirical results, and Section 6 concludes.

1 Theoretical Framework

This section begins with a simple representation of the consumption function as outlined in Friedman (1957)'s Permanent Income Hypothesis and its subsequent formalization within the perfect foresight framework. This starting-point facilitates a discussion of the "wealth effect" and leads into the empirical foundations of the estimation method employed in Carrol, Otsuka and Slacalek (2011)(henceforth COS used interchangeably).

Consumption and the Wealth Effect

In the standard consumption function for an economy with perfect capital markets, homogeneous consumers and no uncertainty, the standard determinants of consumption C, are wealth W and non-market human wealth H. Where we assume that households are infinitely lived, labour income derived from non-market human wealth grows at a constant rate g to the of and consumers have constant relative risk aversion ρ, the consumption function can be written as follows[5]:

$$C_t = \kappa W_t + \gamma Y_t \tag{1}$$

[5] Given the assumption that non-market human wealth is expected to grow at a constant rate g, the continuous time approximation H_t is equivalent to the current value of permanent labour income Y_t divided by $(r - g)$, where r is defined as the constant real interest rate.

with $\kappa = (R - (R\beta)^{1/\rho})/R)$ and $\gamma = \kappa/(r - g)$ defined respectively as the aggregate marginal propensities to consume out of wealth and permanent income.[6] When we separate wealth as defined in (1) into two component parts, namely housing wealth W_h and financial wealth W_f, an extension of the above equation that accounts for different marginal propensities to consume out of housing wealth and financial wealth can be given by:

$$C_t = \kappa_h W_t^h + \kappa_f W_t^f + \gamma Y_t \tag{2}$$

In this model, κ_h (κ_f) can be interpreted as measuring the housing (financial) "wealth effect", which is to say that if housing wealth W^h were to increase by one dollar, consumption would be expected to change by κ_h dollars. Estimation of these wealth effect measures is the main concern of this paper, and equation (2) provides an intuitive framework; however, the equation in this form exhibits empirical issues which we suggest precludes valid estimation of the key parameters. There are two key empirical issues relating to a regression of the model above[7].

Iacoviello (2010) notes that, while we may interpret changes in the right hand side variables of this equation as exogenous at the level of the individual, in the aggregate, it is reasonable to assume that all of the variables outlined above are endogenous. Take for instance a shift in the aggregate value of the housing stock. On one hand, this shift may reflect a genuine shift in housing/consumption preferences; alternatively it could result from financial innovations such as the introduction of asset backed securities, which by widening the availability of credit, might affect both consumption levels and housing prices. Our estimation method must take account of this presumed

[6] As is standard, R denotes the constant after-tax interest rate equal to $(1 + r)$ and β is defined as the discount factor for infinitely lived consumers.

[7] Although not explicitly referenced in the main text, a third and likely unbenign empirical issue relating to estimation of the parameters in (2) is suggested by Sommer (2007) for the US context. This study purports that a significant portion of quarterly variation in consumption can be attributed to measurement error and transitory consumption shocks resulting from factors exogenous to the consumption model; severe weather for example.

endogeneity.

A second problem, argued by COS relates to the common application of cointegration methods for estimating wealth effects (see, for example, Byrne and Davis (2003); Lettau and Ludvigson (2004); Galli (2016)). In the model above, even under the assumption that changes in either W_t^h or W_t^f represent exogenous shocks, any persistent changes in the deep parameters g, r or ρ over the sample period could lead to incorrectly identified time invariant measures for κ_h and κ_f (assuming that they exist). In a simulated experiment COS showed that with a persistent shock to g the wealth effects in the underlying structural model could not be identified using the cointegration approach. This finding is the basis for their development of an alternative method for estimating the respective wealth effects, by exploiting aggregate consumption stickiness. We adopt aspects of this method as outlined below.

The Sticky Expectations Model

In lieu of two popular theoretical frameworks for aggregate consumption growth stickiness, namely the separate habit formation theories of Muellbauer (1988) and Dynan (2000); Carroll and Slacalek (2006) present a model that generates sticky aggregate consumption growth, but is theoretically under-pinned by consumers who are assumed to be mildly inattentive to economic indicators (as in, Sims (2003) and Reis (2006)). Within this framework, consumers only gradually respond (by way of altering levels of consumption) to macro-developments such as shifts in the unemployment rate. In this sense, the behaviour of consumers is suggested to be similar to that of firms in the price setting model of Calvo (1983). Using this model, Carroll and Slacalek (2006) find results that contradict the theoretical benchmark of Hall (1978); ie, the suggestion that aggregate consumption growth follows a random walk process[8]. Indeed, their results indicate that changes in aggregate consumption for a given period

[8] More accurately, Hall (1978)'s specification is premised upon agents that accord with Permanent Income Hypothesis consumers.

(ΔC_t) can be approximated by an AR(1) process, where the estimated autocorrelation coefficient[9] approximates the share of inattentive consumers (degree of consumption growth stickiness) $(1 - \Pi)$. This process is then written as:

$$\Delta C_t = \gamma + \chi \Delta C_{t-1} + \epsilon_t \tag{3}$$

where χ is equivalent to $(1 - \Pi)$. Their result is important in relation to our goal of estimating housing and financial wealth effects. For a given value of χ - assuming that shocks to wealth at time t are independent from shocks at time $t-1$, we can re-write (3) as:

$$\Delta C_t \approx \kappa \chi (\Delta W_{t-1} + \chi \Delta W_{t-2} + \chi^2 \Delta W_{t-3} + \chi^3 \Delta W_{t-4}) + \eta_t \tag{4}$$

$$W_t = (W_t^h, W_t^f)$$

where as in COS four lags of wealth are included to account for observed sensitivity of the coefficient estimates to the inclusion of other instruments. The reader may notice that (4) is written in terms of the growth rate of consumption. Therefore performing IV regression with lagged variables of the change in wealth used to instrument for lagged consumption growth would provide an estimate of the relationship between consumption growth and wealth growth. Clearly, this is not equivalent to our desired estimate of the respective MPC's out of housing and financial wealth. A simple solution to this identification problem is proposed in COS, and is described as follows: Defining the ratios of one period differences in consumption and wealth respective to a specified

[9] The results in Carroll and Slacalek (2006) support a coefficient of approximately 0.7, thereby contradicting Hall (1978)'s hypothesized value of zero. This lends support to the later position of Campbell and Mankiw (1989).

initial level of consumption as[10]

$$\partial C_t = (C_t - C_{t-1})/C_{t-5}$$

$$\overline{\partial} W_{t-1} = (\Delta W_{t-1} + \chi \Delta W_{t-2} + \chi^2 \Delta W_{t-3} + \chi^3 \Delta W_{t-4})/C_{t-5} \tag{5}$$

we are able to derive a first stage regression of the form:

$$\partial C_t = \alpha_0 + \alpha_h \overline{\partial} W^h_{t-1} + \alpha_f \overline{\partial} W^f_{t-1} + \eta_t \tag{6}$$

The coefficients α_h and α_f in (6) are interpreted as equivalent to $\kappa_h \chi$ and $\kappa_f \chi$. In other words, these coefficients provide a respective measure of the current period marginal propensity to consume out of shocks to W^h or W^f in the previous period. As in COS, we refer to α_h and α_f as the immediate first period (in our case, first quarter) MPC. Importantly, this equivalence can be extended for the purpose of finding the medium-run (defined approximately as a period of one to three years) or 'eventual' MPC's for our two concepts of wealth. Predicated on our dynamic model of consumption, and given estimated values for χ and α_j for $j \in \{h, f, \}$, the 'eventual' MPC for wealth type j is defined as:

$$\overline{\kappa}_j = \alpha_j/(\chi(1-\chi) \tag{7}$$

Note that the theoretical framework described up to this point has closely followed the outline of COS. However, pertinent criticisms have been raised regarding their empirical methods (see, Kichian and Mihic (2015)). This methodology can be summarized

[10] Carroll et al. (2011) define the 'initial level' of consumption to be C_{t-5} given the consideration that the final estimation procedure uses variables with lags up to $(t-4)$.

in steps as follows:[11]

1. Estimate a value for χ, by IV estimation of (8) using two-quarter lags of the change in housing wealth, financial wealth and other control variables (including changes in the unemployment rate, consumer expectations and the effective federal funds rate), to instrument for ∂C_{t-1}.

$$\partial C_t = \gamma + \chi \partial C_{t-1} + \epsilon_t \tag{8}$$

2. Using this estimated value for χ, separately construct estimates for $\overline{\partial} W^h_{t-1}$ and $\overline{\partial} W^f_{t-1}$ as defined in (5).

3. Estimate α_h and α_f with regression of the form in (6).

4. Given estimated values for χ, α_h and α_f, derive the 'eventual' MPCs as in (7).

This cyclical estimation procedure implies that χ is determined exogenously to the respective MPCs out of wealth. We consider this implicit assumption to be strenuous. Where χ is defined as consumption sluggishness, it is reasonable to assume that our parameters of interest may be determined endogenously; ie, it is likely that estimates for χ are sensitive to the wealth variables $\overline{\partial} W^h_{t-1}$ and $\overline{\partial} W^f_{t-1}$[12]. With this consideration in mind, we borrow from the method proposed in Kichian and Mihic (2015), wherein the specification in (6) is expanded and estimated in terms of a structural equation.

[11] To the best of our knowledge, three others studies apply this cyclical procedure (see, Slacalek (2009); Sousa (2010b); Barrell and Constantini (2015).

[12] It is interesting to note that the 'preferred' estimates reported in Carroll et al. (2011) result from a backing out procedure that is employed to account for this sensitivity. We suggest that it is slightly misleading that this procedure is included only as a discursion.

2 Estimation Methodology

Model Specification

We consider here an expanded representation of the specification in (6), with the addition of control variables that are assumed to be exogenously determined. In the outline that follows, to simplify presentation, the notation $\alpha\overline{\partial}W_{t-1}$ is used to denote $(\alpha_h\overline{\partial}W^h_{t-1}\ ,\ \alpha_f\overline{\partial}W^f_{t-1})$. By subbing (5) into (6) we have:

$$\partial C_t = \alpha_0 + (\delta_1\Delta W_{t-1} + \delta_2\Delta W_{t-2} + \delta_3\Delta W_{t-3} + \delta_4\Delta W_{t-4})/C_{t-5} + \lambda X_{t-1} + \eta_t \quad (9)$$

$$\delta_1 = \alpha \quad , \quad \delta_2 = \alpha\chi \quad , \quad \delta_3 = \alpha\chi^2 \quad , \quad \delta_4 = \alpha\chi^3$$

where X_t is a matrix of exogenous variables, including the effective federal funds rate, changes in the unemployment rate and consumer expectations[13]. Of course, a regression of the form in (9) is not absent concerns of endogeneity bias as described in Iacoviello (2010). Valid estimates for our key parameters are therefore conditional upon the use of IV methods. In accordance with the identification strategy employed in COS, we include two-quarter lags of X_t in our set of instruments. Primarily these variables are included to capture changes in monetary conditions and economic uncertainty, both of which are perceived to affect our separate components of wealth and next period consumption. Also, we include a four-quarter lag of ΔW_{t-1} in our set of instruments. Our selection of this last instrument likely requires further explanation (it wasn't arbitrary). Empirical evidence supports the argument that the growth rates of both housing wealth and financial wealth show significant positive autocorrelation (see Slacakek (2009); Zhao (2012)). At the same time, validity of the IV method requires exogeneity of the instrument. Where we have explicitly defined $\sum_{i=1}^{4}\Delta W_{t-i}/C_{t-5}$ as

[13] We note that additional variables that are often used to capture monetary conditions and uncertainty (for instance, inflation expectations) are omitted in this set of exogenous variables. Our rationale for this follows from our objective to provide a re-estimation of the model employed in COS (2011), using the same variables employed in their study.

having an effect on ∂C_t, we are compelled to use a further lag, whose strength as an instrument is potentially contentious.

Instrument exogeneity is only one of the pre-conditions for valid IV estimation. A second condition, instrument relevance - or its absence, 'weak identification' - has received increased attention by applied and theoretical researchers (see for example, Staiger and Stock (1997); Dufour (1997); Wang and Zivot (1998); Stock and Wright (2000); Stock and Yogo (2001); Kleibergen (2002); Moreira (2003b); Dufour and Taamouti (2006); among others). These studies conclude that in the presence of weak instruments, standard asymptotic procedures are 'fundamentally flawed', regardless of sample size. IV-inference is therefore untenable since point estimates, hypothesis tests and confidence intervals are all invalid.

To account for the issues associated with weak instruments, a number of procedures have been proposed (see Dufour (2003) for a survey). Although conservative relative to similar procedures, in accordance with Kichian and Mihic (2015), we apply a method based on Anderson Rubin (1949)'s pivotal F-statistic. This decision is based on a characteristic of the Anderson Rubin (AR) procedure that is particularly appropriate for our purposes, specifically that it is robust to missing instruments. Understandably, the list of instruments employed in this study is not exhaustive[14]. In the following section we provide a succinct outline of the statistical properties of the AR-procedure and our application of this procedure to the structural model in (9).

The Anderson-Rubin Procedure

Due to considerations of space, we abstain from providing a rigorous theoretical treatment of the Anderson-Rubin Procedure (for such a description, the reader is referred to the appendix of Dufour, Khalaf and Kichian, 2006; upon which the following procedural outline is drawn from).

[14] Similar studies commonly include the recent performance of the SP 500, and lags of disposable income growth rates (see, Campbell and Mankiw (1989)).

Consider the structural equation defined in (9), written in the following form:

$$C = W\delta + X_1\lambda + \eta,$$

where C is the T-dimensional vector of observations on ∂C_t, W is the $T \times m$ matrix of observations on our endogenous variables $\overline{\partial} W_{t-1}$, and X_1 denotes the $T \times k_1$ matrix of observations for our exogenous variables for consumer expectations, the unemployment rate and the effective interest rate. The associated reduced form equation for our matrix of endogenous variables is then given by:

$$W = X_1\Pi_1 + Z\Pi_2 + \upsilon,$$

where Z denotes a $(T \times k_2)$ matrix of observations on our set of instruments. To test a joint hypotheses of the form $H_0 : \delta = \delta_0$, where δ is composed of the various transformations of α_h, α_f and χ as defined in equation (9), we derive the Anderson-Rubin F-statistic which takes the form[15]:

$$AR(\delta_0) = \frac{(C - W\delta_0)'(M_1 - M)(C - W\delta_0)/k_2}{(C - W\delta_0)'M(C - W\delta_0)/(T - k_1 - k_2)}$$

where, letting $X = (X_1, Z)$, we have defined

$$M = I - X(X'X)^{-1}X', \qquad M_1 = I - X_1(X_1'X_1)^{-1}X_1'$$

Under the null hypothesis, with the standard assumptions of i.i.d. normal errors, we have that $AR(\delta_0)$ follows a central Fisher distribution with degree of freedom k_2 and $(T - k_1 - k_2)$. Note that in contrast to the standard case of IV estimation, the AR procedure does not translate to a direct estimation of the structural equation,

[15] A key property of the Anderson-Rubin F-statistic relates to the fact that no endogenous variables are included in the right hand side of the implicit regression equation

which we have shown suffers from identification issues. Rather, in the context of this study, we are effectively concerned with estimating the Anderson-Rubin F-statistic for the specified parameter set δ_0; where α_{h0}, α_{f0} and χ_0, are parameter values which fit with theory and are specified by the practitioner prior to testing[16]. As shown more rigorously in Dufour, Khalaf and Kichian (2006), this identification-robust test can be inverted to obtain a set of non-rejected parameter values, for a given significance level a.

Based on this procedure, it would be possible to test the preferred IV estimates reported in COS. However, in practice we are concerned with finding the least rejected set of parameters values (the set with the highest associated ρ-value), alternatively referred to as the Hodges-Lehmann point estimate (see Hodges and Lehmann, 1963). To do so, a three dimensional grid of economically meaningful parameter values is constructed. For this study, based on theory and previous research, we specified an acceptable range for both α_h and α_f of (0 , 0.25), with values within this range incremented by .001. For χ we specified a range of (0.5 , 0.9), with values within this range also incremented by 0.001. All parameter sets within this grid are then tested using the procedure outlined above, and a (1 - a) level confidence set is subsequently constructed by filtering for all of the parameter sets that yield a ρ-value greater than the pre-determined significance level a (which in this study is set to 0.05). Bear in mind that there is no guarantee that any parameter set within this grid (in this study we estimate over 12.16 million separate combinations) will provide a ρ-value greater than 0.05. Should this be the case, we are inclined to infer that the model in question is rejected at the specified significance level, and base no conclusions on the least-rejected parameter set.

[16] In practice we select a range of values for the parameters which widely encompass the prior results of studies on consumption stickiness and wealth effects (for example, Calomiris et al. (2009); Sousa (2010b); COS (2011); Kichian and Mihic (2015),among others).

3 Data and Descriptive Statistics

Our main purpose in undertaking this study is to provide a valid re-estimation of housing and wealth effects; using a new method as described above, with explicit reference given to the COS study. Given this objective, we find it is appropriate to use both their original data set as well as an updated version of their data set.

The two quarterly data sets respectively span the periods 1960Q1 to 2007Q4, and 1960Q1 to 2016Q1. Aside from their disparate spans, all series are extracted from the same sources. Data series for consumption, population, and the implicit price deflator are extracted from the National Income and Product Accounts, Bureau of Economic Analysis; data series for wealth and its components are extracted from the Flow of Funds Accounts, Board of Governors of the Federal Reserve System; data series for the unemployment rate and effective federal funds rate are extracted from the Fred II database, St. Louis Fed; and lastly, data on consumer expectations is supplied by the University of Michigan Survey Research Center.

The consumption data used in this study are total non-durable consumption expenditures. Non-durable consumption is preferred to other measures of consumption for two main reasons. Our first concern is measurement error: quarterly service sector consumption data is prone to measurement error as a number of components that are employed in the computation of this measure are interpolated from annual data (see, Bureau of Economic Analysis (2006)). In addition, Paradiso, Casadio, and Rao (2012) suggest that durable consumption measures are more acutely influenced by business cycle patterns, asset market dynamics and exogenous shocks such as natural disasters. Our second concern is based on the findings of Mankiw (1982); this study finds that durable consumption growth is significantly negatively autocorrelated, a property that may lead to attenuated estimates for χ [17].

[17] Carroll et al. (2011) do not explicitly comment on this property of durable consumption, but it is referenced in Carroll, Slacalek and Sommer (2008). It seems likely therefore that this is accounted for in the later research of Carroll and Slacalek.

The net financial wealth data employed in this study is defined as the sum of equity held by households, and corporate equity held by private pension funds, government retirement funds, bank trusts and estates, closed end funds, mutual funds and life insurance companies. We note that this list may not be exhaustive, but we are constrained to use the same computations as in COS. The housing wealth data is calculated as the difference between net-worth (as defined by the FRB, and our computed measure for net financial wealth. We should emphasize that Calomiris et al. (2009) revealed that a significant portion of this measure (in some quarters approximately 50 percent) consists of non-housing wealth. Our study explicitly accounts for this potential measurement error in our robustness checks, wherein we swap in the FRB Flow of Funds series for real estate held by households and non-profit organizations.

All measures for consumption and wealth used in this study are inflation adjusted and in per-capita terms. Data series for these variables are plotted in Figures 1-2, with each figure corresponding to one of the respective data sets. The following comparisons can be made between the first and seconds moments of the separate data sets (see Table 1):

Table 1: Growth Rate Descriptive Statistics

Full Data Sets						
	COS Original Data		**Updated Data**		**Percentage Difference**	
	Mean	***Var.***	***Mean***	***Var.***	***Mean***	***Var.***
Financial Wealth	1.20%	0.75%	1.23%	0.79%	1.76%	5.15%
Housing Wealth	0.57%	0.01%	0.54%	0.01%	-4.48%	103.92%
Consumption	0.36%	0.01%	0.26%	0.01%	-26.49%	66.67%
Financial Crisis Sub-Samples						
	Pre-Crisis Sample		**Post-Crisis Sample**		**Percentage Difference**	
	Mean	***Var.***	***Mean***	***Var.***	***Mean***	***Var.***
Financial Wealth	1.31%	0.76%	1.08%	0.95%	-17.42%	24.91%
Housing Wealth	0.57%	0.01%	0.42%	0.02%	-25.93%	143.53%
Consumption	0.31%	0.01%	-0.02%	0.02%	-106.35%	281.25%

1. There are clear differences in the mean growth rates of real per-capita housing wealth for our two data samples, whereas observed differences in the mean growth rates for financial wealth are, in relative terms, less significant. In particular, the mean growth rate of financial wealth is approximately 1.76% higher for the updated data set, while the mean growth rate of housing wealth is approximately 4.48% lower for the updated data set. This variation exhibited between our two data sets is to be expected given that the data set used in COS does not incorporate data for the period pertaining to the financial crisis and the post-financial crisis bull-market. In terms of magnitude, the differences in first moments for housing wealth growth are muted relative to the observed variation in second moments. In particular, housing wealth growth is 103.92 % more volatile in the updated data set, while financial wealth growth is approximately 5.15% more volatile. Of course, it should be noted that the volatility of financial wealth is typically much larger regardless of the sample period.

2. Again, we observe differences between the two data sets for the first and second moments of per-capita non-durable consumption. The mean growth rate of consumption declines by approximately 26.49%, while its volatility increases by 66.67%. These differences appear to fit more closely with those exhibited for housing wealth.

3. As an instructive exercise, we subset our updated data set into a pre-crisis sample and a post-crisis sample; tabulating the mean and variance for consumption and wealth series as above (see, Table 1). The results tend to confirm that observed differences between the original COS data set and our updated data set stem from the post-crisis period.

Figure 2 captures the dramatic increase in housing prices that characterized the period starting in the late 1990's and terminating early in 2008 [18]. Real per-capita consumption appears to follow this trend. In the wake of the financial crisis there is a remarkable decline in both consumption and housing wealth, with a subsequent reversal around 2010. Although there appears to be some co-movement between consumption and housing wealth in the post-crisis period, consumption is observed to have plateaued at pre-crisis levels, while housing wealth continues to increase above pre-crisis levels.

Regarding financial wealth, it is shown to be smaller in proportion to housing wealth across the entire sample period. Aside from the Internet bubble period, the gap between housing wealth and financial wealth remains stable. Similar to the case of housing wealth, we find that movements in financial wealth appear to be positively associated with movements in consumption. This relatively high level of volatility exhibited by financial wealth is the basis for the suggestion in Mishkin (2007) that any financial wealth effect may be diminished due to concerns about the persistence of financial wealth gains. Inspection of table 1 reveals that this logic could - at least qualitatively - be linked to a perceived attenuation of housing wealth effects in the post-crisis period.

[18] Since the stock of housing, especially in per-capita terms, is large relative to its flow, short-term fluctuations in housing wealth are determined primarily by fluctuations in the aggregate price of new and existing homes (Iacoviello (2010)).

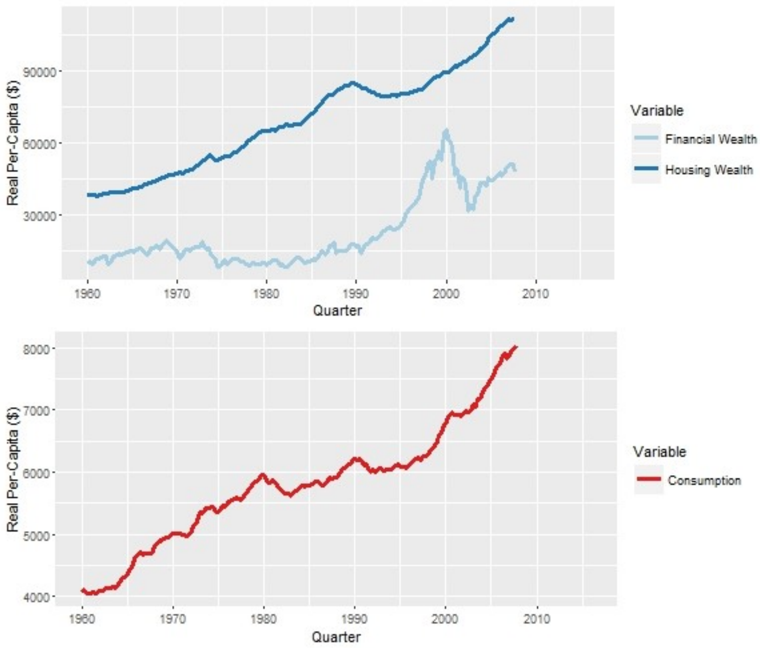

Figure 1: Original COS Data Set; Period 1960Q1 to 2007Q4

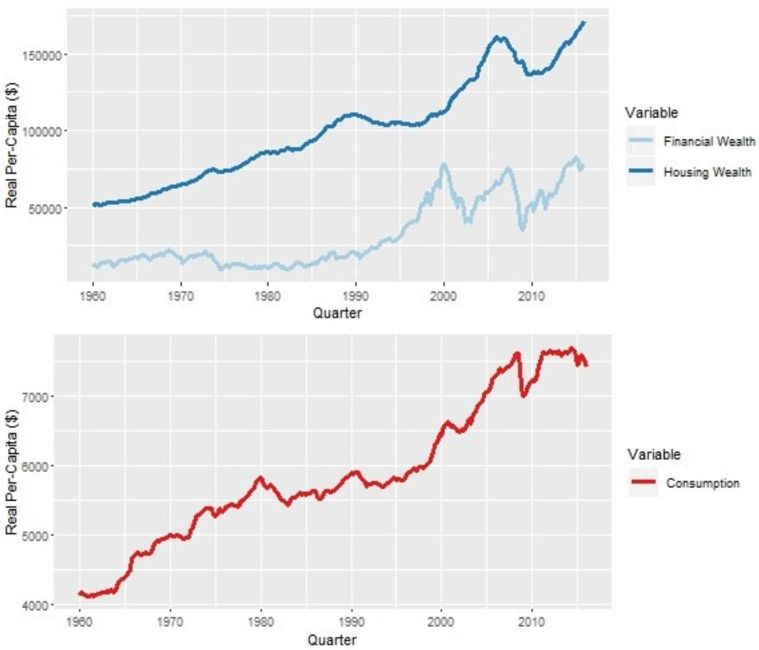

Figure 2: Updated Data Set; Period 1960Q1 to 2016Q1

4 Empirical Results

In this section we describe the results obtained by application of the above-defined Anderson-Rubin procedure to the model in (9); results are reported for both the COS data set, and our updated data set. The estimates tabulated below enable comparison between the results obtained for each data set and we suggest possible explanations for the observed differences.

Our first step corresponds to an assessment of the 'preferred estimates' reported in COS. In applying the AR-procedure to their original data set, we are able to test their parameter estimates of (0.719, 0.018, 0.008) for χ, α_h and α_f respectively. We find an associated ρ-value for this parameter triplet of approximately 0.249, which supports the conclusion that this parameter vector is compatible with the econometric model being tested (at a 95% significance level). However, out of approximately 12.16 million estimated parameter combinations, it should be noted that close to 58 thousand parameter triplets pass this threshold. Furthermore, the ρ-value for this specification is approximately 50 % smaller than our estimated ρ-value corresponding to the so-called Hodges-Lehmann point estimate (these least- rejected parameter set estimates are reported for our two data sets in the following rows of Table 2).

Application of the AR-procedure to the COS data yields a bounded set of non-rejected parameter triplets. In the left panel of Figure 3, a graphical interpretation of these non-rejected parameter combinations (all sets which yield a ρ-value that is greater or equal to 0.05) is given. Examination of this graph reveals two key characteristics. First, across all tested specifications of χ, we find corresponding non-rejected parameter values for α_h and α_f. Second, within our defined grid, all specifications for α_h and α_f which yield a ρ-value above 0.05 are positive. This second finding lends support to the basic premise that both housing and financial wealth effects exist for this period of analysis.

In Table 2 we report parameter estimates corresponding to the maximal ρ-value

Table 2: Wealth Effect on Non-Durable Consumption

Model	Immediate Effect of $1 Change in Wealth χ	α_h	α_f	Eventual Effect of $1 Change in Wealth $\overline{\kappa}_h$	$\overline{\kappa}_f$	***Max ρ-value***
Carroll et al. (2011) 'Preferred Results'	0.719	0.018	0.008	0.087	0.041	0.249
AR COS Data	0.653	0.018	0.012	0.079	0.053	0.495
AR Updated Data	0.763	0.004	0.007	0.022	0.038	0.281
AR Real Estate Wealth	0.758	0.003	0.008	0.016	0.043	0.268
AR Great Moderation	0.80	0.006	0.006	0.038	0.038	0.948

Notes: The eventual MPCs are not directly estimated but are instead calculated from α_j and χ using the formula in (7). The fourth panel of Table 2 pertains to a re-estimation of the updated model using real estate wealth held by household and non-profit organization as an alternative to the measure for housing wealth used in Carroll et al. (2011) .

(equal to 0.495) found for the original COS data. Our estimated values are 0.653, 0.018 and 0.012 for χ, α_h and α_f respectively. Note that in practical terms, this parameter set is not equivalent to the 'preferred results' of Carroll et al. (2011). In comparison, we find that the 2SLS estimate for χ in COS (2011) suggests a slightly more acute level of consumption stickiness than is supported by our corresponding estimate, while their estimate for α_f indicates a notably weaker immediate financial wealth effect than is found with application of the Anderson-Rubin method. Interestingly, the estimated values for α_h are approximately equal to 0.018 for both parameter sets; which also preserves the relative importance of housing wealth. Lastly, the reported estimates for consumption stickiness and immediate wealth effects translate to eventual MPCs of 0.079 for housing wealth and 0.053 for financial wealth. These results can be largely be seen as analogous to their 'preferred' counterparts; however, we point out that in comparing the reported gaps separating the eventual effects of housing and financial wealth, our estimate of this gap is 44% lower than it is for the preferred estimates of Carroll et al. (2011).

A final qualification that pertains to our most-accepted parameter combination for the COS data requires discussion. In the right panel of Figure 3, we plot all accepted pairs of α_h and α_f that correspond to our Hodges-Lehmann estimate for χ. If we focus specifically on point estimates with a ρ-value larger than 0.4, the range of acceptable values for α_h and α_f remains quite large. For illustrative purposes, if we impose the additional constraint of $\alpha_f = 0.012$ (the Hodges-Lehmann estimate for α_f), we find that the range of α_h's accepted, with corresponding ρ-value's greater than 0.4, falls between between 0.015 and 0.023 [19]. In consideration of this, we must conclude that there remains uncertainty regarding our optimal estimates.

In comparison to our estimates for the COS data set, we find that the set of accepted parameter combinations is more acutely constrained for the updated data set. As before, application of the AR-procedure yields a bounded set of accepted parameter triplets (see Figure 4). Also, across all tested specifications of χ, we find corresponding non-rejected parameter values for α_h and α_f. Referring back to the concerns raised by Kichian and Mihic (2015), this observed characteristic exemplifies the issue of weak identification and the potentially endogenous determination of χ. In comparison to our previous results, within the defined parameter grid, not all accepted specifications for α_h are greater than zero. Indeed, as exhibited in the right panel of Figure 4, by fixing χ equal to its Hodges-Lehmann estimate, the distribution of accepted point estimates is considerably smaller. But, note that this distribution overlaps with the α_f axis - a result that shows there is some uncertainty about the existence of a statistically significant positive housing wealth effect in the updated data set. Moreover, the finding that there is an accepted range of negative values for α_h is difficult to account for theoretically, as this effect is incompatible with the micro-founded assumption of monotonic preferences. This result may slightly weaken the theoretical justification underlying our implicit aggregation of individual behaviour to

[19] Application, of a similar procedure, where instead α_h is constrained to be equal to its Hodges-Lehmann estimate, we find that the range of α_f's non-rejected with corresponding ρ-value's greater than 0.4 falls between approximately 0.01 and 0.018.

the behaviour of a representative agent.

Shifting our attention to the least-rejected parameter set (reported in Table 2), it is clear that there is a significant difference between this parameter set, and the optimal parameter set reported above for the COS data set. In this case, the results corresponding to the highest ρ-value (equal to 0.281), are (0.763, 0.004, 0.007); where we use the ordered set notation (χ, α_h, α_f). From this, three key differences are immediately noticeable: first, the Hodges-Lehmann estimate for χ has increased by more than 17%; second, the corresponding estimates for α_h and α_f have fallen by factors of 4.5 and 1.7 respectively; third, these results suggest that the eventual marginal propensity to consume out of financial wealth is larger than the associated MPC for housing wealth. Specifically, the eventual MPCs estimated for this data set are 0.022 for housing wealth and 0.038 for financial wealth.

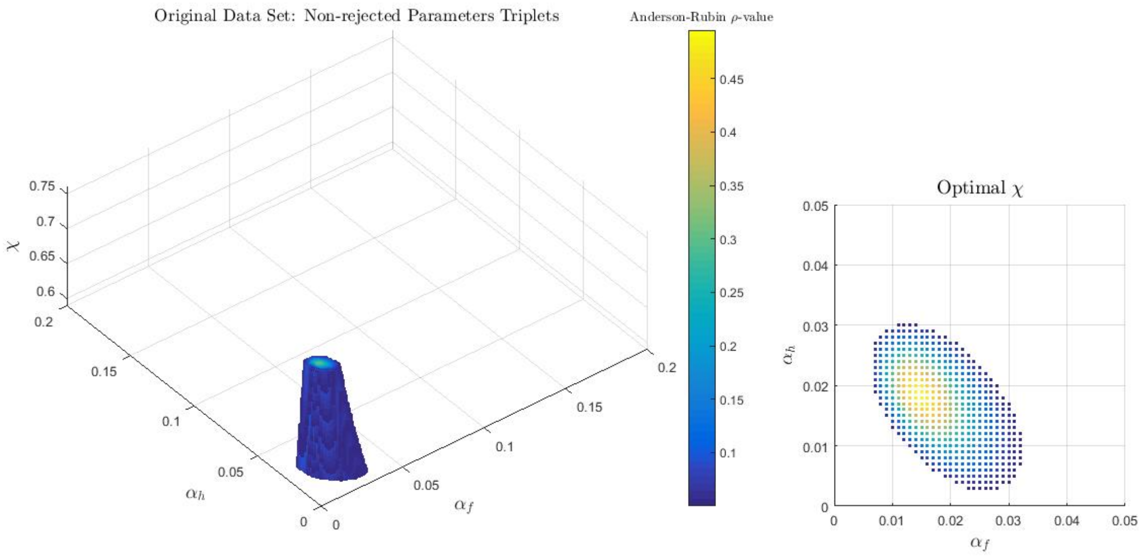

Figure 3: COS Data Set; Period 1960-Q1 to 2007-Q4

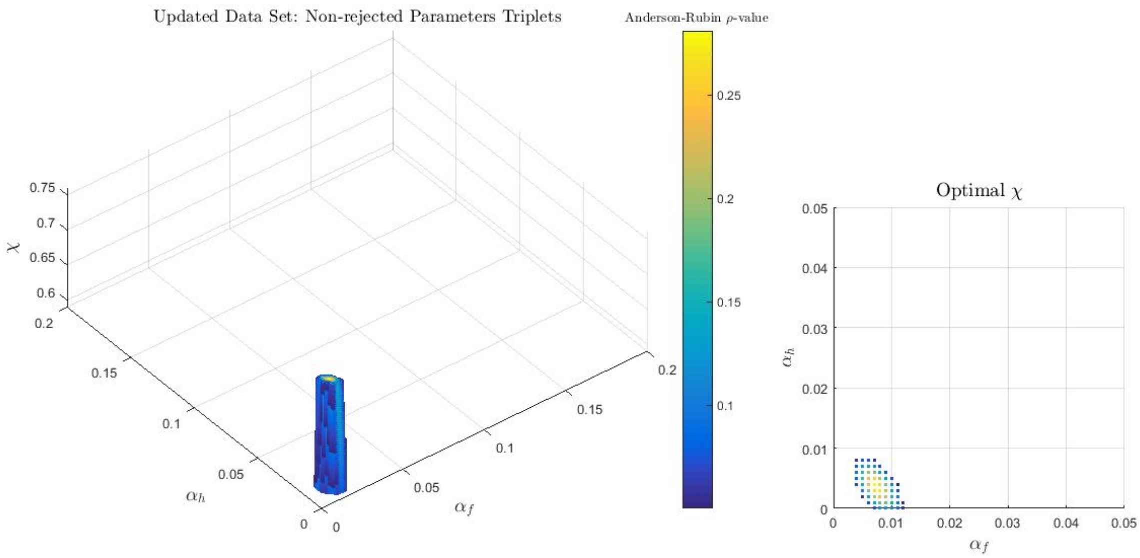

Figure 4: Updated Data Set; Period 1960Q1 to 2016Q1

Robustness Check

Previously it was highlighted that the housing wealth measure used in Carroll et al. (2011) potentially suffered from significant measurement error. As a means to test for resulting biases (this bias would presumably inflate our estimated housing wealth effect), we apply the AR-procedure to our updated data set, but alternatively employ the FRB Flow of Funds series for real estate held by households and non-profit organizations, as our measure for housing wealth. The results for this alternative specification are included in the section of Table 2 denoted with the label 'Robustness Check'. We note that the results tend to support the claim that a portion of housing wealth - as previously defined - consisted of non-housing wealth. α_h is observed to fall by 0.001, while α_f increases by an equivalent amount. Nevertheless, our conclusion remains largely unchanged - incorporating data from the period marked by the recent financial crisis, the relative magnitudes of the housing wealth and financial wealth effects are reversed.

Alternative Sample Period: Accounting for the Great-Moderation

Estimates reported in the final row of Table 2 pertain to a truncated sample period dated 1984-Q1 to 2007-Q4. Our results above show that there are notable differences between the estimated wealth effects for the COS sample period and our post-crisis sample period[20]. Our study is not the first to note observed sample-period dependence. Slacalek (2009) found that there was a statistically significant increase in housing and financial wealth effects after 1989 in the United States, while Iacoviello and Nero (2007), in applying a DSGE model with housing sector, reported a much larger consumption response to shocks in their post-1988 sub-period. We propose that the findings of these studies can be theoretically explained with reference to the "great moderation" - a period, as referenced in Stock and Watson (2002), to be characterized by reduced volatility of many key macroeconomic variables. Where consumer inattentiveness and consumption wealth effects are presumed to be influenced by consumer expectations, the reduced uncertainty associated with the "great moderation" period is, in accordance with the findings of the two studies referenced above, anticipated to result in stronger wealth effects.

Our results for this supplemental analysis do not tend to confirm the hypothesis of stronger wealth effects for the "great moderation" period, though our measure for consumer innattentiveness is markedly augmented, as is predicted by theory. We find that the estimated housing and financial wealth effects for this period are both equal to 0.06, while our estimate for χ is 0.8. A finding that suggests that neither effect has relative dominance. Most importantly though is our maximum estimated ρ-value which at 0.948 is markedly higher than the corresponding values for all other estimated samples. Relative to the results for all other specifications reported in Table 2, we find that their is much reduced uncertainty regarding our Hodges-Lehmann parameter set for this truncated period. Estimates of the eventual marginal propensities to consume

[20] We provide some interpretation of these observed differences in the following section.

out of housing and financial wealth are both approximately 0.038. Ultimately, the implications of this finding fall outside the scope of this study, though further analysis is certainly required.

Discussion

If the exhibited shifts in wealth effects can predominantly be attributed to the financial crisis, our results could be instructive regarding the theoretical underpinnings of the separate wealth effects. Such an assumption is consistent with our findings in the descriptive statistics section and the findings of other studies that compare pre and post-crisis wealth effects (see Soss and Mo (2013); Zandi, Poi and Hoyt (2015)). Imposing a structural break dated 2007-Q4, the above cited studies both find that wealth effects have decreased overall since the financial crisis, with a much larger decrease observed for the MPC of housing wealth relative to that of financial wealth[21].

We propose that a larger relative decrease in the housing wealth effect could be interpreted to result from two concurrent factors. The first relates to the accessibility of housing wealth. Households are largely constrained in their ability to channel housing wealth increases into cash-flow; in the short-run they are able to increase consumption only through mortgage equity withdrawals (see Duca and Kumar (2011)). Since the financial crisis, stricter regulations and protective lending practices have limited access to this primary monetization channel. The anticipated negative effect is consistent with the exhibited abatement of α_h. Second, as commented upon in the descriptive statistics section, since the collapse of the housing market in 2007, housing wealth growth has shown volatility that dramatically exceeds the historical norm. Within the framework of Mishkin (2007)'s argument, households will therefore be less likely to view an increase in housing wealth as permanent, and adjust their spending patterns accordingly. Borrowing from Kenneth Galbraith, "the pathological weakness of finan-

[21] To be clear, among these two studies, only the latter study finds evidence of a change in the relative prominence of financial wealth effects.

cial memory" may in this case remain forthcoming. An important implication of this result is that the macro-econometric models of the Federal Reserve Board could overstate their ability to increase consumption through managed expansions of housing prices[22].

Consider now the comparatively small change observed for financial wealth effects. We point out that among the two factors described in the preceding paragraph, our interpretation of the financial wealth effect is specifically influenced by the 'financial memory' of consumers, rather than liquidity constraints. Intuitively, this could help to explain its smaller concurrent movement. Moreover, observed differences in the volatility of financial wealth between our two data sets are relatively mild (the volatility of financial wealth increases by 5.15%; compared to a 103.92% increase in housing wealth volatility). Nevertheless, the estimated change for α_f from 0.012 to 0.007 is not insignificant, especially when we take into account the sustained gains witnessed by US stock market indices over the last 24 quarters of the sample period[23].

Lastly, we touch upon the 17% rise in the estimate for χ. Explicitly, the theoretical rationale for Carroll and Slacalek (2007)'s consumption stickiness parameter is consumer inattentiveness. Yet, in terms of our empirical model, we are unable to distinguish between the alternative hypotheses of habit formation and sticky expectations. Prior to the financial crisis, our estimate for χ is consistent - albeit mildly smaller - with the findings of other studies that employ the sticky expectations model (Carroll and Slacalek (2006); Carroll et al. (2011); Kumar and Owen (2013)). At the same time, studies which employ the habit formation model to explain consumption persistence prior to the financial crisis (Dynan (2000); Fuhrer (2000); Grubber (2004); Sommer (2007)) find estimates for the equivalent of χ that range between 0.65 and 0.80. So our estimates for both data sets can be seen as consistent with either group

[22] One such method would of course be to lower interest rates.

[23] Since its low-point during the recession, stock wealth has increased by about $12 trillion, while housing wealth has increased by $6 trillion. Historically, housing wealth has on average been approximately 422% larger than stock wealth.

of consumption persistence studies. As far as we can tell, only one study has undertaken a comparative analysis of consumption persistence in the pre and post-crisis periods (Kumar and Owen (2013)). Their study finds that consumption persistence fell immediately following 2008-Q1 but rebounded after 2009-Q1. In terms of their theoretical framework (and ours), this implies that consumer attentiveness to macroeconomic indicators increased briefly during the crisis, and then dropped off again with the improvement of aggregate conditions in 2009. Unfortunately, their data sample terminated in 2012-Q1, and we are unable to tell whether or not a further increase in χ over the next four years (as our results seem to indicate) would be consistent with their model.

Taken collectively, the results reported in Table 2 and discussed above, can arguably be viewed as giving support to the findings of previous studies that claimed housing wealth effects in the US context exceeded financial wealth effects for their respective sample periods (see Case et al.(2005); Muellbauer (2007); Carroll et al. (2011)). All of these prior studies modelled data that spanned the period preceding the Great Moderation and terminated prior to 2008. Of course, evidence provided in this study suggests that within the contemporary context, the relative prominence of the housing wealth effect has been overturned. Nevertheless, we reason that this finding is plausibly indicative of a structural break linked to the recent financial crisis, rather than representing support for the dissenting conclusions of Sinai et al. (2005) and Calomiris et al. (2009).

5 Conclusion

In this study, we used the Anderson-Rubin inverted f-statistic procedure to test the empirical validity of Carroll et al. (2011)'s estimated wealth effects. Although the model used in this study does not perfectly correspond with the model used in their previous study, we suggest that for all intents and purposes, our results are directly comparable. This study used two data sets: the Carroll et al. (2011) original pre-crisis data set, and an extension of this data set updated to incorporate the period 2008-Q1 to 2016-Q1.

In either case we found evidence supporting the claim in Kichian and Mihic (2015) that weak identification and the endogenous determination of χ undermines the validity of the IV method described in Carroll et al. (2011). Our subsequent recommendation is perhaps all the more substantial given the recently popular application of the COS method in the empirical literature (see Slacalek (2009), Sousa (2010b) and Barrell and Constantini (2015)).

The maximal ρ-values estimated for both data sets tend to support the model as specified in (9). Regarding our results for the pre-crisis period, we find that the immediate first quarter effect for housing wealth is approximately equivalent to 1.8 cents on the dollar. Over the span of two to three years this effect has more practical significance, relating to a 7.9 cent increase in non-durable consumption. Consistent with previous studies, we do find that this medium run effect is smaller for financial wealth, but the spread is only 2.6 cents; compared to the 4.6 cent gap reported by Carroll et al. (2011). Turning to our results for the updated date set, we find that the efficacy of monetary policy, were its aim to specifically increase consumption through 'engineered' boosts to housing wealth, is muted within the contemporary context [24]. Whether this shift in the housing wealth effect can be viewed as transitive or indicative of a lasting structural change is beyond the scope of this study. At present, the necessary data for this analysis is unavailable.

Finally, we should emphasize that this study is best represented as an initial step towards a more rigorous treatment of the theoretical model developed in Carroll et al. (2011). In terms of future research, we highlight that the instruments used in

[24] We note that an expansionary monetary policy is anticipated to effect consumption through more important channels, including increased production and reductions in the unemployment rate. Nonetheless, given our results, the total effect on consumption would be diminished in conjunction with a smaller observed housing wealth effect.

this study, though theoretically founded, are less than optimal. Moving forward, we propose the use of instruments that are better suited to capturing plausibly omitted dynamics such as liquidity constraints and income expectations. One such instrument, as developed in Muellbauer, Armant and Williams (2015), would be a credit conditions index (CCI)[25]. These authors show that their estimate for credit conditions in the Canadian context account for shifts in non-price household credit conditions that are presumed to affect housing prices, but are not explained by economic factors. A second extension to our study would entail the application of estimated factors, to be employed as instrumental variables. As shown in Bai and Ng (2010), by using estimated macroeconomic factors one is able to condense information from a number of series into a smaller set of potentially stronger instruments that concurrently account for omitted macroeconomic dynamics. We expect that these empirical extensions would plausibly preserve the fundamental dynamics captured by our applied method, but could enhance the efficiency of our results.

[25] The multi-equation approach used to compute the Muellbauer et al. (2015) credit conditions index would be infeasible within the empirical framework of this study as it endogenizes housing prices (a significant component of our measure for housing wealth).

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