



UNITED STATES NAVAL ACADEMY
DEPARTMENT OF ECONOMICS
WORKING PAPER 2011-31

“How the Housing and Financial Wealth Effects have changed over Time”

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How the Housing and Financial Wealth Effects have changed over Time

Abstract

We measure the “evolution” of the housing and financial wealth effects over time by estimating the dynamic responses of consumption to both forms of wealth in the United States over different time periods from 1952 to 2009. To understand how the housing and financial wealth effects have changed over time, we use a combination of recent time series techniques, including system structural break tests and linear projections to estimate impulse response functions over relatively short sub-samples. Our key results are that the housing wealth effect gets larger over time, with the largest effect apparent after 1998; while the financial wealth effect diminishes over the same sub-samples, even over periods that include the equities boom of the 1990s. Our results provide insight into what mechanisms may explain the differing responses of consumption to wealth.

Key words: Impulse response functions, structural breaks, consumption, linear projections.

JEL codes: E21, C32

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1. Introduction

Economic research is ambiguous on the strength of the wealth effect for the United States and there is even disagreement on whether there is a direct wealth effect at all. Estimates of the wealth effect vary in the literature according to whether times series or household-level data are examined; the type of wealth that is considered (housing versus equities); whether single-equation or system methods are applied; the sample estimated; or whether one assumes there is a stable, long-run relationship between consumption, income and wealth (Paiella (2009) provides a review). Moreover, it is not clear to all that the connection between wealth and consumption represents a causal relationship or is driven or amplified by some common factor (see Muellbauer (2007), and Attanasio *et al.* (2009)).

In this paper, we measure the “evolution” of the relative wealth effects over time by estimating the dynamic responses of consumption to both tangible wealth and financial wealth in the United States over different time periods from 1952 to 2009. To do so, we follow a three-part strategy designed, in part, to avoid many of the difficulties in assessing the wealth effect common in the literature. First, we specify a five-variable vector autoregression (VAR) that includes consumption of non-durables plus services, liabilities, tangible assets, financial assets, and disposable income.¹ We estimate our system as a VAR in log-levels instead of specifying a VECM model (vector error correction model). The VAR provides us flexibility on two fronts. One, recent Monte Carlo evidence by Ashley and Verbrugge (2009) suggests that even in the presence of non-stationarity and cointegration, estimating a VAR in levels provides impulse response functions that are robust to those specification issues. Given there is uncertainty in the literature on whether consumption, income and wealth are cointegrated, by assuming a simple VAR in log-levels in the spirit of Ashley and Verbrugge (2009), we remain agnostic on the issue of whether the relationships should be modeled with a cointegrating vector or not. And two, with the simple VAR we can easily test for structural breaks in our system, while testing for structural breaks in a VECM poses challenges.²

¹ Our specification is motivated by various research on the structural relationship between consumption, income and total household wealth. We discuss the literature in section 3.

² For example, one cannot (reliably) jointly estimate the correct cointegrating rank and test for structural breaks. See Perron (2006) for lengthy discussion.

For the second part of our empirical strategy we apply the Qu and Perron (2007) method for indentifying structural breaks in our system without *a priori* knowledge of when those breaks actually occur. As evident from Paiella's (2009) review, the potential instability of the wealth effect shows up in two forms in the literature. Wealth effect estimates vary depending on which sample period is used in a particular paper; and, estimates vary depending on whether researchers assume the cointegrating vector (if it is found) is stable over the available sample period.

Despite these issues, the incorporation of break tests in the literature is scant, though not for lack of good reason.³ Long sample periods are required to identify cointegrating relationships, yet break tests necessarily require splitting up such long periods into shorter sub-samples (Lettau and Ludvigson (2004) discuss the practical difficulty in identifying breaks in the cointegrating vector between the variables in question; see also Perron (2006) for a detailed discussion of this issue). However, as noted by Paiella (2009), and Carroll *et al.* (2011), changes in regulation, other aspects of market structure, or household preferences may lead to structural breaks in long time series. With Qu and Perron's (2007) methodology we are able to account for parameter instability in our VAR system, and exploit the instability to compare the wealth effect across time periods.

Finally, we estimate the dynamic responses of consumption to our wealth measures with Jordà's (2005, 2009) local projection method for estimating impulse response functions and accompanying conditional standard errors. Jordà's (2005, 2009) methodology proves particularly useful for assessing changes in the wealth effect across sub-periods, and we are able to identify statistically significant impulse response functions in our relatively short sub-samples (measured at the quarterly frequency). VAR-based estimates of impulse response functions measured over relatively short time spans can suffer from low power leading to little evidence of statistically significant responses.⁴ Lastly, with the local projections, we can compare the relative wealth effect

³ Ludwig and Sløk (2004) do compare wealth effect estimates pre- and post-1985. Their sample split, however, is admittedly *ad hoc* and structural break tests are not applied.

⁴ Jordà (2005) shows impulse response functions produced from one-step-ahead forecasts by linear projection are robust to misspecification. Moreover, Jordà (2009) shows how to calculate conditional standard errors for each horizon of the dynamic response; this method provides more precise standard error

for financial and tangible wealth across our sub-samples in response to the same magnitude of shock.

From our three-part estimation strategy, our key result for the wealth effect is as follows: We find that the response of consumption to a change in tangible wealth gets larger over time, based on our estimated break dates in 1973, 1985, and 1998. The tangible wealth effect before 1973 is negative, but turns generally positive thereafter, and by the 1998 to 2009 sub-sample, the response of consumption to a shock to tangible wealth is relatively large and persistent. The evolution of the tangible wealth effect over our sub-samples is consistent with Muellbauer's (2007) "credit channel" explanation of the wealth effect—the tangible wealth effect may be negative or negligible in constrained credit markets, but becomes significant and positive when those constraints are reduced.

In addition to our result for the tangible wealth effect, we find the effect of a change in financial wealth on consumption gets weaker over our sub-samples. While the path of consumption's impulse response function is generally the same across periods, the magnitude diminishes in each sub-sample. Moreover, in each sub-sample, the peak or trough of the response to tangible wealth shock is larger (in absolute value) than in the corresponding period for the financial wealth shock. The differing responses of consumption to shocks to tangible and financial wealth, respectively, confirm various studies that show the wealth effect is different for general asset type. Also, our result of the relatively weaker effect for financial wealth is consistent with previous research (as detailed by Paiella (2009)). In this paper, however, we offer the perspective that while the wealth effect through financial wealth has diminished over time, the effect for tangible wealth has strengthened.

We believe both the empirical approach of this paper, and the results generated from our approach, offer insight to the literature on the wealth effect. Our results provide some understanding as to why the wealth effect arises at all—whether the empirical connection between consumption and wealth represents a direct phenomenon or is the manifestation of something else. In particular, the responses of consumption over our sub-samples to the two forms of wealth appear to support Muellbauer's (2007) "credit

estimates for the impulse response coefficients at each horizon than are typically generated from VARs. We discuss this further in Section 3.

constraints” explanation of the wealth effect. The negative response of consumption we find in the 1952 to 1973 period is consistent with the lack of a home-price-wealth effect Muellbauer (2007) infers from U.K. data before 1980. Such a result arises when credit-constrained households (typically young and renting) must save more when housing prices increase. Conversely, the positive response of consumption to the tangible wealth shock we find in the 1998 to 2009 sub-sample is consistent with the idea that if credit constraints are reduced, young households can save less and consume (and borrow) more, while established home-owners can more easily capitalize on their housing collateral. The emphasis on credit constraints to explain the wealth effect is distinct from the “common cause” explanation of Attanasio *et al.* (2009), or even from the idea of a direct wealth effect; we discuss these possibilities in more detail in Section 3.4.

Our results also offer some insight into understanding whether the relationship between consumption and wealth may or may represent an asset price bubble. White (2006) defines an asset price boom as “an improbably long period of large positive returns that is cast into sharp profile by a crash.” With respect to the wealth effect, an asset price boom might also be represented by “excessive sensitivity” of consumption to a change in wealth.⁵ Based on the Permanent Income Hypothesis (PIH), the impulse response of consumption to a one-time (*i.e.* temporary) change in wealth, income, or liabilities, should be small in magnitude (see Dejuan and Seater (2006), or Manitsaris (2006) for a description of the PIH). However, if households mistake a temporary change in wealth for a permanent one, consumption may show a relatively large response during periods of large increases in wealth. The impulse response function we estimate for consumption in response to a shock to tangible wealth for the 1998 to 2009 sample (relative to earlier periods) may be consistent with a housing bubble; in contrast, we do not see any indication of “irrational exuberance” in consumption spending related to the 1990s stock market boom. We speculate further on these implications in section 3.4.

In the next section we provide a description of the time series properties of our data; in particular, we apply univariate structural break tests and cointegration tests. The descriptive results from the next section allow us to clarify if the consumption and

⁵ In the literature testing the Permanent Income Hypothesis (PIH), the response of consumption to a temporary change in income, contrary to the prediction of the (PIH), is referred to as consumptions’ “excessive sensitivity” to income (see Brady (2008) for a review).

wealth-related data are stable over time and if they are, in fact, cointegrated. In doing so we provide a brief overview of the challenges with estimating the wealth effect in time series data found in the literature, thereby motivating our empirical approach. In section 3, we discuss our VAR model, structural break tests on the VAR system and then finally, we estimate and analyze the impulse responses of consumption to our system variables.

2. Univariate Stability Tests

In this section, we perform stability tests on consumption, income, and wealth, and the relationships between them. We do so to address two general challenges (or perhaps, disagreements) that arise in attempting to identify a wealth effect in time series data—finding conclusive evidence of a cointegrating relationship between the variables in question; and if such a relationship is found, whether the relationship is stable over time. The evidence on the stability of the relationships between consumption and income, and consumption and wealth is mixed. Carroll *et al.* (2011) argue that assuming stable long run relationships between the variables is neither justified by theory, nor likely to hold in reality. In practice, evidence for or against a cointegrating relationship between the variables is a function of the time period and the variable definitions. Rudd and Whelan (2006) do not find convincing evidence of a cointegrating relationship between their measures of wealth, income and consumption; nor do Benjamin *et al.* (2004) in their study on the disaggregated wealth effects under consideration in this paper. In contrast, Lettau and Ludvigson (2004) present evidence in favor of cointegration between the variables, as does Morley (2007) for consumption and income if income is measured with real GDP (instead of some measure of labor income).⁶

Given the uncertainty over the stability or even existence of the long run relationships between the data, we perform stability tests on three different forms of our data. First, we test for structural breaks in the log levels of consumption, disposable income and our measures of wealth, respectively (all variables are in per capita, real

⁶ Lettau and Ludvigson (2004), for example, use consumer nondurables and services as a proxy for total consumption—a strategy which is common in research on consumption behavior more broadly—while Rudd and Whelan (2006) offer a different version of total consumption. With respect to income in cointegration tests, Rudd and Whelan (2006) cite both the use of nondurables as a proxy for total consumption, and the choice of a deflator for nominal income, as issues that arise in correctly identifying cointegration.

terms). This provides an initial assessment of the time series properties of our data. As noted by Carroll *et al.* (2011), only if each sector of the economy is unchanging should one expect the long run stability between the variables in this study to be unchanging. Second, motivated by the possible long run connections between the data, we then test for structural breaks in the ratios of these variables; the ratios of these variables may contain important information on the stability of the long run relationships between them (for examples, see Lettau and Ludvigson (2001, 2004), Carroll *et al.* (2011), and Rudd and Whelan (2006)). Lastly, we test the stability of the residual series from a single-equation regression of consumption on income and wealth; the residuals from this regression are used in the cointegration literature to identify the long run relationship between the variables.

2.1 The Data

Our choices to measure consumption, income and wealth are based on the general strategy in the literature. We include monthly real per-capita consumption of non-durables plus services, real per capita disposable income, and two measures of wealth: financial assets of households and tangible assets of households. All variables are defined as log-levels, seasonally adjusted and in 2005 constant dollars. To construct our per-capita variables, we use population measured as the civilian non-institutional population over 16 (provided the Bureau of Labor Statistics (BLS)). The consumption and income series are available from the Bureau of Economic Activity (BEA). Non-durables and services consumption is used in keeping with standard practice with the permanent income hypothesis literature (to avoid issues in imputing the value of durable goods—see Lettau and Ludvigson (2004) for an example).⁷

The wealth data are from the Flow of Funds provided by the Federal Reserve Board, with wealth measured as the net worth from the balance sheets of households and non-profit organizations. Since we are interested in the effects of different types of

⁷ While Palumbo, Rudd and Whelan (2006), and Rudd and Whelan (2006) criticize this practice we defer to Lettau and Ludvigson (2004) and many others in the literature that use, for much-discussed reasons, the non-durables category to analyze consumption behavior. While we find Palumbo, Rudd and Whelan (2006), and Rudd and Whelan (2006) compelling (as evidenced by the numerous cites in this paper), we admittedly err on the side of being conservative in our choice in keeping our analysis comparable to the majority of consumption research.

wealth, we decompose net worth into its component parts: liabilities; tangible assets, which include real estate, equipment owned by non-profits, and durable goods; and financial assets, which include bank accounts, equities, debt holdings, and life insurance.⁸ All data are converted to real terms using the personal consumption expenditure deflator. The quarterly sample spans the first quarter of 1952 through the last quarter of 2009. The first panel of Figure 1 provides a glimpse of these variables over the 1952 to 2009 span.

2.2 Stability tests on the individual series

We fit simple autoregressive integrated moving average (ARIMA) models to each of these individual series and test for structural breaks using the methodology of Bai and Perron (1998, 2003). For each series we test three models: ARIMA(1,0,0), ARIMA(0,1,0), and ARIMA(1,1,0). Notice that a break test search on an ARIMA(0,0,0) would be looking for mean shifts in a series. Each series shows evidence of a time trend, which suggests that model inappropriate. In specifying these simple models, we follow Timmerman (2001) who suggests simplicity is preferable when the nature of the structural model is unclear (see also Brady (2008), Brady and Greenfield (2010), and Stimmel (2009) for distinct applications). Moreover, we use the three simple ARIMA specifications to remain flexible about the true process driving each series; for example, with the ARIMA(0,1,0), and ARIMA(1,1,0) models we allow for the presence of a unit root, a common finding for these series.⁹

Bai and Perron (1998, 2003) develop a least-squares algorithm for estimating unknown break dates, a set of hypothesis tests for unknown breaks (in both coefficients and covariances), and a method for interpreting the results of those tests.¹⁰ Similar issues to Bai and Perron (1998, 2003) are discussed in Andrews (1993) and Andrews, Lee, and

⁸ We follow Benjamin *et al.* (2004) by using the Flow of Funds data; however, they restrict tangible wealth to include only real estate wealth. We assume the most general definition provided by the data. We did examine a model that restricted tangible wealth to only real estate. The results were robust to those using the broader definition that we report in Section 3.

⁹ We applied the Augmented Dickey-Fuller (ADF) and Phillips-Perron tests to our data; we do not report the complete results here. We fail to reject the null of a unit root in all cases (i.e., inclusion of trend and intercept, or just trend, etc.) for consumption, disposable income, and financial assets. For most of the tests, we also fail to reject the null hypothesis for liabilities and tangible assets; we reject the null for liabilities and tangible assets only in the ADF test including a trend and intercept. The full results are available upon request.

¹⁰ The Gauss code is generously available on Pierre Perron's website at people.bu.edu/perron/code.html.

Ploberger (1996) and we refer interested readers to those sources. We follow Bai and Perron's (1998, 2003) suggested selection method relying on a sequence of hypothesis testing for " x " breaks versus " x plus one" breaks mainly using *supF* tests. Key choices for researchers include the maximum number of possible breaks to be allowed, the "trimming percentage," which defines the minimum size of a sub-sample or regime, and the significance level for the hypotheses tests. We allowed for a maximum of five possible breaks, a trimming percentage of 10 percent, and a 5 percent test size. Our choices were motivated by trying to allow as much flexibility in identifying structural break dates.

Table 1 displays the results of the break tests. Neither consumption nor financial wealth shows any evidence of a structural break. Disposable income shows evidence of a single break in 1975 for one specification, though the confidence interval is very wide (spanning roughly 20 years). For tangible wealth, two of the specifications place a break in the 1980s. Only the simple autoregressive model for liabilities shows any evidence of multiple breaks, in 1983, 1990, and 2003.

2.3 Stability tests on Descriptive Ratios

For an alternative perspective on the properties of the consumption, income and wealth data, we test for structural breaks in the ratios of these variables. The stability of the long run relationships between these variables may be evident in these ratios. For example, Figure 1 shows the ratio of liabilities to disposable income, the ratio of income to tangible assets, to financial assets, to net worth, and to total assets. Figure 1 also shows the ratio of consumption to income, and to net worth. We include the National Bureau of Economic Research (NBER) recession dates as shaded bars for visual reference.

From Figure 1, we can see that liabilities to disposable income generally rose over time, with a plateau roughly between the mid 1960s and early 1980s before resuming a clear acceleration upwards. This indicates that that in general the pace of borrowing outstripped increases in income. For the ratios of income to assets and net worth we can see a slight downward move over time, and that the dynamics of income to tangible assets seems to differ from the income to financial assets, net worth, and total assets, which seem comparable to each other. This is especially noticeable in the post-1990

period. Particularly, the drop in income to tangible assets appears to lead much smaller drops in the other ratios as well as peak before the 2000 recession rather than after. This contrasts for example with the 1982 recession where each income to asset or net worth ratio peaks after the recession and then experience a decline. The ratio of consumption to income shows a clear rise post-1982 recession. Arguably the most noticeable aspect of the ratio of consumption to net worth is the drop post-1980 and the apparent increase in volatility.

Table 2 displays the break test results on the ratios (along with 95 percent confidence intervals). Note, for the ratios, we also report results for a simple ARIMA(0,0,0) model—it is less obvious that each series in this case has a trend (as is the case with the individual series) so we include this version of the model in our battery of tests. Structural breaks are found for each series primarily in the simplest ARIMA(0,0,0) model at a number of points (in most cases the highest specified number of breaks (5) is chosen). However, as noted in Section 2.2, this may not be the appropriate specification for all the series given the possibilities of a trend, an autoregressive parameter, or a unit root.

For the ARIMA(1,0,0) model breaks are found in only two of the ratios—in liabilities to disposable income in 1965, 1984, 1992 and 2002; and for consumption to income in 1989. For the liabilities to income ratio under the ARIMA(1,1,0), we find a break in 1963 and in 1984. For the ARIMA(1,1,0) model, we also find a break in 2002 in the income to tangible wealth ratio, the income to total wealth ratio; and in 2002 in the consumption to net worth ratio. We should note, though, the 95 percent confidence intervals for some of the break dates in Table 2 are rather wide. While the ratios may not be as unstable as suggested by the simplest model, there is some evidence of structural breaks in these relationships for the more “elaborate” ARIMA models.

2.4 Stability tests on the Cointegrating Relationship

While we do find breaks in the simple ratios of the data, the wide confidence intervals for some of the break dates, and lack of consistency across the ARIMA models leaves us wanting for more conclusive evidence. In this section we apply the Bai and Perron (1998) break tests to the residual series from a single-equation regression of consumption

on income and wealth. As noted above, the residuals from this regression are used to identify the long run relationship between the variables. For example, Rudd and Whelan (2006) test for a unit root in the residuals from a regression of log consumption on log income and household assets. If the residual series is stationary (does not contain a unit root), then a cointegrating relationship exists between the variables (assuming the variables themselves contain a unit root). For comparison with previous papers (and with our more updated span of data) we first apply cointegration tests to various versions of the residual series then test the series for structural breaks.¹¹

Table 3 displays the results from Augmented Dickey-Fuller, and Phillips-Perron unit root tests on the residuals from a series of regressions of log consumption on log disposable income and the different components of the household balance sheet from net worth to the disaggregated components. For each residual series, and for each variant of the unit root test, in almost all of the cases we reject the null of a unit root at the 5 percent significance level, and in all of the cases we reject the null at the ten percent level. Only in the model with an intercept and trend do we fail to reject at the five percent level (see the third and sixth column).¹² In other words, our results are clearly on the side of finding a cointegrating relationship between the variables.

Our results offer general support to previous findings of a cointegrating relationship between consumption, income and wealth (see Ludvigson and Lettau (2004) and Rudd and Whelan's (2004) tests using Ludvigson and Lettau's (2004) variable definitions). However, our rejection of the null from the regression involving the disaggregated wealth components differs from Benjamin *et al.* (2004). They fail to reject the null using the Engle-Granger Test for a unit root on data defined slightly different from ours (as mentioned they use aggregate consumption, for example) and over a shorter sample period. Here, however, using more recent data and different variations for the wealth data, our findings agree more with the more common finding of a cointegration relationship.

For an additional perspective (to test for the possibility of more than one cointegrating relationship), Table 4 displays the results from applying Johansen's

¹¹ See, again, footnote 9 for a brief summary of our unit root tests on the individual series.

¹² While we report the results for the tests including a trend, a visual inspection of the residual series (not pictured here) suggests a trend is not appropriate.

Cointegration Trace test to the data (see Johansen (1995)). The results suggest that at least two cointegrating vectors likely exist, and perhaps even a third. In each version of the test, we reject the nulls of at most zero, and one cointegrating relationship, respectively. For the null hypothesis of at most two cointegrating vectors, we reject the null under most of the assumptions on the underlying trend.

Finally, given the evidence of a cointegrating relationship from the residuals from the consumption regressions, we apply the Bai and Perron (1998, 2003) method to the four residual series. Table 5 displays the estimated break dates and associated confidence intervals. We find breaks for the simplest model but not for the additional variants (the ARIMA(1,0,0), etc.). Across the residual series, the estimated break dates are the same with only small differences in the associated confidence intervals. Generally, we find breaks in the early 1960s, sometime in 1974, in 1983, in 1990, and then for three of the four series, in 1997.

2.5 Implications of the Break Results

Our break tests provide mixed evidence on the stability of not only the variables themselves (and their ratios), but also on the cointegrating relationship between the variables over our sample. With respect to the latter, the simple ARIMA(0,0,0) suggests there are a number of breaks in the mean of the residual series at various points in the sample (Table 5); in other words, the break estimates suggest that the cointegrating relationship identified with the residual series is not stable. However, these breaks are not found under the additional specifications and it is not clear the simplest ARIMA model is the appropriate specification.¹³ With respect to univariate series and the ratios, there is a smattering of estimated break dates displayed in Tables 1 and 2, but not for all of the variables, and the results are not consistent across the specifications.

Of course, there is evidence of structural breaks in the data and the relationships between the variables, which is consistent with the argument made by Carroll *et al.* (2011), that the stability of the relationship is unlikely.¹⁴ However, consistent with the

¹³ Visual inspection of the residuals (not reported) suggests the ARIMA(1,0,0) may be the most appropriate specification.

¹⁴ Note, Carroll *et al.* (2011) do not test explicitly for stability, but instead provide a test of the wealth effect that is not dependent on the long run relationship, stable or otherwise, between the variables.

various examples of disagreement in the literature—as noted by Paiella (2009), time series evidence on the wealth effect is sensitive to the sample period chosen, assumptions on the stability of the long run relationships, etc.—our stability tests are not conclusive across the different specifications to which the break tests are applied. Moreover, with the break tests on the residual series we are confined to testing the stability of the one implied cointegrating relationship, whereas, the Johansen test suggests more cointegrating relationships are likely between the variables.

As noted at the outset of this discussion, the disagreements in the literature on the appropriate way to model and estimate the wealth effect motivates the empirical strategy in this paper. And the discussion in this section, and the results from our various structural break and cointegration tests, provide some empirical verification of the challenges noted by others in finding the appropriate specification for carrying out wealth effect studies. In the next section we pursue a strategy to estimate the dynamic relationship between consumption, wealth, income and liabilities that is flexible with respect to these issues.

3. Estimating the Wealth Effect over time

In this section we estimate the wealth effect based on a three-part strategy intended to minimize typical challenges faced in estimating the wealth effect over time. The basis of our approach is our five-variable system that includes the real per capita variables of consumption of non-durables and services, liabilities, tangible assets, financial assets, and disposable income over the quarterly sample from the first quarter of 1952 through the fourth quarter of 2009 (see section 2.1 for data sources).¹⁵ We specify our model based on consumption and wealth literature. For example, Gali (1990) establishes the structural relationship between consumption, income and total household wealth derived from the theory of intertemporal choice; while Ludvigson and Steindel (1999) and others estimate a single-equation structural equation with consumption as a function of wealth and labor income (note, too, the financial accelerator model of Bernanke and Gertler (2001)

¹⁵ We also examined more restricted versions of this general model. We examined a four variable VAR that dropped liabilities, a four variable VAR that combined financial and tangible assets as total assets, and a three variable VAR that replaced assets and liabilities with net worth. We also examined a version that used only real estate wealth rather than the broader tangible wealth category. Results were robust across these specifications to the general model presented here.

provides a theoretical reason why the wealth effect for housing would be larger than the wealth effect for equities).

The first part of our strategy is a deviation from the VECM approach often used in the wealth effect literature. Instead we estimate our system as a VAR in log-levels. Specifying the relationships between macroeconomic variables in the form of a log-level VAR has a well-established pedigree (see Hoover and Jordà (2001), Stock and Watson (2001), and Christiano, Eichenbaum and Evans (1999) for reviews). Moreover, recent Monte Carlo evidence by Ashley and Verbrugge (2009) suggests that even in the presence of non-stationarity and cointegration, estimating a VAR in levels provides impulse response functions that are robust to those specification issues. Given both the uncertainty in the literature about the appropriateness of estimating wealth effects from a vector error correction framework, and the uncertainty over the stability of the cointegrating relationships discussed in section 2, we follow Ashley and Verbrugge's (2009) suggestion and estimate in VAR log-levels.

The simple VAR specification allows us to implement the second part of our strategy, applying the Qu and Perron (2007) method for indentifying structural breaks in our system. This allows us to account for the possibility that the relationships between the variables are not stable—as suggested by some of our tests in section 2 and by Carroll *et al.* (2011). While these break tests are not a direct test of cointegration, *per se* (as in the case of the tests in section 2.4), by identifying breaks in the system we can at least identify sub-periods over which the system's parameters are stable.

Finally, for the third part of our strategy we use Jordà's (2005, 2009) methodology to estimate the impulse responses of consumption to shocks to each variable in the system, for each sub-sample. These impulse response functions provide our comparison of the relative wealth effects over time. Estimating the relative wealth effects with impulse response functions is a useful exercise for a few reasons. First, consumption may not respond contemporaneously to a change in wealth, instead responding with a lag, or the full wealth effect only becoming apparent after a few lagged

periods.¹⁶ The lag dynamics of a VAR model captures such a lag effect with the impulse response functions. Second, impulse responses show the dynamic evolution of a variable to a one-time, unexpected shock to a variable. And by construction, we can compare the wealth effect over time in response to the same type of shock (say, a one percent change in a variable).

Moreover, estimating the impulse response functions from Jordà's (2005, 2009) methods is particularly attractive for our purposes. As has been noted, possible misspecification with respect to the relationships between consumption wealth and income is particularly acute in the wealth effect literature. Jordà (2005) shows impulse response functions calculated from one-step-ahead forecasts by linear projection are less prone to misspecification (than VAR-generated impulse response functions). As Jordà (2005) explains, impulse response functions from VARs are calculated for long horizons, yet VARs are optimally designed for one-period ahead forecasting. Hence, errors in the forecast from misspecification are compounded as the horizon increases. Moreover, even if the VAR does accurately represent the true data generating process, Jordà (2005) shows the efficiency loss from using the local project method is trivial.

In addition, Jordà (2009) shows how to calculate conditional standard error bands by exploiting the temporal ordering of impulse response functions. Jordà (2009) provides Monte Carlo evidence that his conditional confidence bands have superior power in smaller samples compared to typical VAR-generated confidence intervals.¹⁷ With the reduced degrees of freedom from estimating impulse responses in sub-samples, Jordà's (2009) more precise conditional standard error bands are particularly attractive here. In the interest of brevity we refer the reader to Jordà (2005) and Jordà (2009) for further details of the local projection method.¹⁸

¹⁶ Dynan and Maki (2001) show that the financial-wealth effect follows a lagged process, which may arise from households not paying attention to changes in their wealth on a regular basis. See also Case *et al.*, 2005, and Kennickell and Starr-McCluer, 1997 for discussions.

¹⁷ Chang and Killian (2000) discuss the potential inaccuracy of impulse response confidence intervals estimated for VAR systems typical of applied VAR research. Moreover, it is not uncommon in the applied VAR literature for authors to report only one-standard deviation intervals (since statistical significance is often wanting at the two-standard deviation level). Stock and Watson's (2001) review of VARs provides one example of this practice.

¹⁸ For comparison, we estimated impulse response functions in the typical manner from a VAR, along with typical asymptotic standard errors. The VAR-IRFS are similar to the linear projection-IRFs; the qualitative inference we report in section 3 does not change. However, the statistical significance of the former is

3.1 Motivation

With our empirical strategy, we test the following propositions:

- The response of consumption to a change in tangible wealth will be different than a shock to financial wealth.
- The response of consumption to a change in either form of wealth will be different depending on the time period considered (i.e. there are, in fact, structural breaks in the relationships between the variables).
- Given the difference across time, the wealth effect from financial wealth may be stronger than the wealth effect from tangible wealth in one period, but perhaps weaker than the tangible wealth effect in a different period (or vice versa).

The first statement is well-supported in the literature. A number of studies have documented differences in a wealth effect for financial wealth and a wealth effect for non-financial (or tangible) wealth.¹⁹ With respect to financial wealth, in a study of OECD countries, Catte *et al.* (2004) found the marginal propensity to consume (*mpc*) out of financial wealth to be between three to seven percent for Australia, Canada, Japan, the Netherlands, the UK, and the US. They found the *mpc* out of financial wealth to be one to two percent for France, Germany, Italy, and Spain. Mankiw and Zeldes (1991) find a wealth effect for stock market wealth only for households that own stock, while Juster *et al.* (1999) find a higher marginal propensity to consume out of stock market wealth higher than overall wealth (see also Dynan and Maki (2001)).

However, despite the existence of a stock-wealth effect, the magnitude of this effect may be of little economic significance for consumption. Though there is evidence that the consumption of wealthy households does make up a notable share of total consumer spending (about 12 percent, see Poterba (2000) and Sabelhaus (1998)) the concentration of stock market wealth among these households is a reason why the wealth effect of a stock market increase may be negligible for aggregate consumer spending.

lacking, as expected given the discussions in Jordà (2005, 2009). Our VAR results are available upon request.

¹⁹ We provide a brief review here, and refer the reader to Paiella (2009) for a detailed literature review. In particular see Paiella's (2009) Tables 1 and 2 for a comprehensive list of wealth effect estimates.

Starr-McCluer (1998) examines the University of Michigan's Survey of Consumers and finds that 85 percent of the households did not change their consumption in response to the stock market increase over the 1990s. Any stock market wealth effect only appears to matter for the richest households (see also Case *et al.* (2005)).

Moreover, many studies have found that the wealth effect out of tangible wealth (primarily determined by housing assets) is larger than the wealth effect from a change in financial assets. This may be due to the relative distributions of the types of wealth. As detailed by Poterba (2000), even after the increase in stock ownership over the 1990s, stock ownership is not as widespread as home ownership. In addition, the distribution of home ownership is not as skewed as stock ownership.²⁰

Catte *et al.* (2004) find an *mpc* for housing wealth of between 5-8 percent for Australia, Canada, the Netherlands, the United Kingdom, and the United States. They find a smaller *mpc* of one to two percent for Italy, Japan and Spain and no statistically significant *mpc* for France and Germany. Muellbauer (2007), finds an *mpc* out of housing wealth in the United Kingdom of 3 percent, and tentative estimate of an *mpc* of 6-7 percent for the United States. Campbell and Cocco (2007) also find a wealth effect for homeowners in the United Kingdom. While earlier research such as Elliot (1980) found little evidence of an wealth effect in data from the 1970s and before, Case (1992) documents a wealth effect for the late 1980s-real estate price boom in New England. Also, Engelhardt (1996a) finds that households that experience a capital loss from declining house prices reduce consumption, but the consumption of households who realize a capital gain from price appreciation is generally insensitive to the increase in wealth. Finally, Case *et al.* (2005), and Carroll *et al.* (2011) provide corroborating recent evidence that the housing wealth effect is larger than from stock market wealth (see also the references in Paiella (2009)).

With respect to the second and third hypotheses above, there is less in the literature on how the relative wealth effects have changed over time. We consider this question below.

²⁰ Poterba (2000) notes the bottom 80 percent of households owning stock in the late 1990s accounted for only 4.1 percent of the total value of equity holdings, while the bottom 80 percent of households with the lowest real estate holdings accounted for 29 percent of all holdings.

3.2 Structural Break Tests on the system

We test for structural breaks our system with Qu and Perron's (2007) method. Qu and Perron (2007) extend the methodology of Bai and Perron (1998, 2003) to test for unknown (*a priori*) structural breaks in a system of equations. To implement the Qu and Perron (2007) method, the researcher must make choices analogous to those discussed in Section 2. For example, because there are more parameters to estimate for the system, we increased the trimming percentage (size of sub-sample or regime) to 20 percent restricting the maximum number of breaks to no more than three (four sub-samples or regimes). If we allowed four breaks the selection of break dates would be trivial, and five (or more) is not possible given the trimming percentage. Also, we use two lags in the VAR to limit the number of parameters (with quarterly data, VAR systems are typically appropriately specified with at least two lags). Even so, we were concerned about flexibility in placing the breaks, so for robustness we also examined limiting the maximum to one or two breaks to check that the placement of any found breaks were consistent. The break dates are significant with a 5 percent test size and we report the dates and 95 percent asymptotic confidence intervals in Table 6.

Table 6 shows the break dates are consistent across the number of maximum breaks chosen and, as the 95 percent confidence intervals suggest, are rather precisely estimated (contrary to some of the break estimates reported in section 2). Structural breaks in the system are found in 1973, 1984 or 1985, and 1998.

In the estimation of our VAR, we use the estimated break dates from the bottom row of Table 6 to define the sub-periods over which we compare the response of consumption spending to shocks to wealth and income. This leads in particular to the four sub-samples of, 1) the first quarter of 1952 through the third quarter of 1973; 2) the fourth quarter of 1973 through the first quarter of 1985; 3) the second quarter of 1985 through the second quarter of 1998; and, 4) the third quarter of 1998 through the fourth quarter of 2009.

3.3 Impulse Responses

In Figures 2 through 5 we report the impulse response functions (from the linear projection method) of consumption in response to one-percent shocks to liabilities, tangible wealth, financial wealth, and income over the different time periods.²¹ We follow common practice in the VAR literature and assume short run restrictions in order to estimate structural impulse functions. This is achieved by Wold recursive ordering; we impose the following order: consumption, liabilities, tangible wealth, financial wealth, and income. The order is motivated by the implied contemporaneous causal relation in Gali (1990) where consumption contemporaneously depends on wealth and income. In our ordering, consumption is contemporaneously affected by all variables and disposable income is contemporaneously unaffected by all variables. We order the stock variables (tangible and financial wealth) before their flow variable (income) so that the flow contemporaneously affects the stocks but not the reverse. There is less intuition about the ordering of liabilities. Our rationale is that changes to financial and tangible wealth are more likely to contemporaneously affect the ability of households to accumulate liabilities than the reverse (for example, an increase in asset values makes it easier for households to obtain credit, while an increase in debt is unlikely to lead to a change in the value of their home or stock portfolio).²² Lastly, we estimate each sub-sample with two lags.²³

In the next four sub-sections we provide detailed discussion of the impulse responses. Thereafter, in section 3.3.5, we discuss the economic significance of the results.

²¹ We omit the responses to a shock to consumption as we are specifically interested in the wealth effects and the effects of debt and income. The impulse responses for a shock to consumption are available upon request.

²² We did not investigate all 119 alternative Wold causal orderings, but we did investigate the reverse ordering. The results were qualitatively similar and impulse response figures are available from the authors upon request.

²³ The adjusted-AIC chose one lag for each sub-sample, which is not surprising given the small samples. However, to be consistent with most VAR studies, where at least two lags seem to be appropriate for most VARs, we impose two lags. There is not a notable difference between the IRFs estimated with one lag or the IRFs estimated with two lags, except the former are more likely to be statistically significant. By choosing two lags, and given the relatively small sample sizes we estimate over, we set a more stringent standard by which to conclude any given IRF is statistically significant.

3.3.1 Consumption and a shock to Tangible Wealth

Figure 2 displays the responses of the variables to a one-percent shock to tangible wealth, along with 95 percent conditional confidence bands. In each sub-sample, consumption shows an initial positive, but generally small, response to the shock. In the earliest period (through the middle of 1973), after a very brief and immediate increase, consumption declines for most of the horizon (though the response is not statistically significant for essentially the entire horizon). In contrast, from 1973 through the first quarter of 1985, the response of consumption is more or less positive for up to 15 quarters after the shock (with statistical significance occurring initially for a few quarters, then again only briefly at various points over the horizon). Only after the 15th quarter or so, consumption becomes negative. Similarly, in the next period, from the middle of 1985 through the middle of 1998, the response of consumption starts out positive, though only for up to seven quarters after the shock, then becomes negative. However, two years after the shock, the response oscillates around zero (and is statistically significant initially for a few quarters and then periodically over the horizon).

Finally, in contrast to the earlier periods, in the 1998 to 2009 sample, consumption increases steadily following the shock (and is statistically significant) for approximately two years after the shock. The response becomes negative, however, after about the 15th quarter. Across the sub-samples, too, the magnitude of the response of consumption is largest in the 1998 to 2009 sample (reaching a peak at 0.59 percent in quarter 13).

3.3.2 Consumption and a shock to Financial Wealth

Figure 3 displays the responses of the variables to a one-percent shock to financial wealth. In contrast to case with tangible wealth, in the early sub-sample, consumption increases after the positive shock to financial wealth and remains positive (and statistically significant for about eight quarters). The response turns negative and then positive once again, but is not statistically significant for the rest of the horizon. In the 1973 to 1985 sub-sample, the response of consumption is initially positive and statistically significant for a few quarters, similar to the same period in Figure 2, but then becomes negative after about six quarters (unlike the case in response to the tangible

wealth shock). The response of consumption remains negative and mostly statistically significant for up to 16 quarters after the shock, then actually is positive again late in the horizon.

In the 1985 to 1998 period, the response of consumption after the shock is small, positive and statistically significant for two to three quarters after the shock. For the remainder of the horizon the response is generally zero and statistically insignificant except for a brief decline late in the horizon. Finally, similar to case for tangible wealth, in the 1998 to 2009 period, the response of consumption to the financial wealth shock is positive and statistically significant. However, in contrast to the former case, the response of consumption only remains positive for about five quarters and reaches a magnitude less than 0.10 percent (in contrast to a peak of almost 0.6 percent shown in Figure 2). After about 14 quarters the response of consumption is again statistically significant, but unlike the initial response, is negative for the rest of the horizon.

3.3.3 Consumption and a shock to Liabilities

Figure 4 displays the responses of the variables to a one-percent shock to liabilities. In each sub-sample, consumption shows an initial positive response. Thereafter, the behavior of consumption differs in each time period. Up through the middle of 1973, consumption shows a sustained positive response to the shock to debt (there is some statistical significance for brief spells up to two years after the shock). In contrast, from 1973 through the first quarter of 1985, the response of consumption is more or less positive for about four quarters after the shock, then declines to a trough of about 1.2 percent and is below zero for the most of the horizon thereafter (for the majority of the duration the response is statistically significant). In the next period, from the middle of 1985 through the middle of 1998, the response of consumption is negative with a short period of statistically significant (after the brief initial positive response) through about the sixth quarter, then becomes positive and statistically significant for most of the remainder of the horizon (with a maximum amplitude of about one percent). Finally, in the post-1998 sample, the response of consumption oscillates for the first few quarters, then is negative for a year and half (from the seventh quarter to the 14th quarter after the

shock), and then becomes positive again in the latter part of the horizon (and is statistically significant over parts of the horizon).

3.3.4 Consumption and a shock to Income

Finally in Figure 5, in the first two samples, through 1973 and then in the 1973 to 1985 sample, the response of consumption is positive (peaking at approximately 0.60 and 0.50 respectively) for around two years after the shock (in the first period, statistical significance lasts up to about the 8th quarter; in the latter period, statistical significance lasts until about the 10th quarter). The response of consumption to the income shock in the 1985 to 1998 period is relatively similar to the period just prior, except the negative trough is shallower and the consumption becomes positive again late in the horizon (with initial and late periods of statistical significance). In the latest sample period, from 1998 through 2009, the response of consumption is positive and remains positive (and is mostly statistically significant) for the entire horizon. In contrast to the earlier periods, the response reaches a peak late in the horizon rather than in the first few quarters after the shock.

3.4 Implications of the Impulse Responses

With respect to the first hypothesis cited in section 3.1, the impulse response functions displayed in Figure 3 and 4 support previous findings that consumption does indeed respond differently to the two general forms of wealth. Our results accord with the general findings of Carroll *et al.* (2011), Case *et al.* (2005), and Benjamin *et al.* (2004), among others (see Paiella (2009) for more examples). With respect to the latter two hypotheses, our results offer some new insight. Three results stand out.

- First, in each sub-sample, the peak or trough of the response to tangible wealth shock is larger (in absolute value) than in the corresponding period for the financial wealth shock.
- Second, the response of consumption to financial wealth diminishes over time.

- And third, the direction of the response of consumption has changed for tangible wealth; the response is predominately negative in the earlier sample and positive in the latest period.

With respect to the first two points, given that financial assets are roughly twice as large as tangible assets, and financial wealth has increased over time, we might have expected that the response to the financial wealth shock to have increased over time, all else equal. Moreover, the response to financial wealth may have been larger than the response to tangible wealth at least in the 1985 to 1998 period over which an increase in stock market wealth is well-documented, and before the notable appreciation in home values that occurred thereafter (changes in total wealth in the U.S. have been found to be highly correlated with stock market changes, in particular—see Paiella (2009)).

However, while stock ownership has increased over time, a significant amount of financial wealth is bound up retirement accounts. In this light, we should observe little to no financial wealth effect from an increase in stock market wealth that occurred, for example, in the late 1990s (see Poterba (2000), Poterba and Wise (1998), and Starr-McCluer (1998) for more discussion). Thaler (1990) notes that households view forms of wealth differently and, as are result, households will form “mental” accounts to determine how they spend wealth. If so, then the response of consumption to this form of wealth may be small. Our results across the sub-samples accord with the notion that consumption may not respond much to a change in financial wealth. Here we document that this phenomena appears to be consistent across time periods; and that, in fact, the effect of a shock to financial wealth on consumption has become weaker over time.

With respect to tangible wealth, the absolute magnitude of response of consumption is about the same in the earliest and the latest sample; however, the response is primarily negative in the earliest part of the sample, and primarily positive in the 1998 to 2009 period. The positive response of consumption is also evident at times from 1973 through 1998, but becomes most apparent in the latest era. The negative response of consumption in the earliest period is inconsistent, of course, with a wealth effect.

While Poterba (2000) and others offer help in understanding the relationship between financial wealth and consumption, the reasons for the response of consumption to tangible wealth (Figure 2) in our sub-samples are less clear. We speculate on three possibilities related to the literature on the wealth effect—that changes in the tangible wealth effect are correlated with financial market liberalization in the U.S., that there may be a “common cause” behind the apparent wealth effect, or that households’ confuse transitory shocks to wealth with permanent ones.

3.4.1 Credit Markets and the Wealth Effect

The evolution of the tangible wealth effect across the sub-samples might be explained by the coincident evolution of credit markets in the U.S. over the same time period. For the latter, the transformation of mortgage markets is well-documented, as is the liberalization of credit markets more broadly (see Fernandez-Corugedo and Muellbauer (2006), Brady (2011), and Berger *et al.* (1995) for discussions). The availability of credit across time might amplify the effect of an increase in home prices on consumption.

Muellbauer (2007) explains the effect of a change in housing prices on consumption is driven by two credit-related factors. First, when credit markets are tight (households are liquidity-constrained), an increase in housing wealth will reduce consumption relative to income for younger households, which are more likely to be renters than homeowners. Young households will save more in anticipation of higher rents and higher down payments needed to buy a house. As credit markets become “looser”—lenders are more willing to lend to households previously considered liquidity-constrained—the young households can save less, borrow more, and consume more. Second, for older households (homeowners) the liberalization of credit markets allows homeowners to access their increase in collateral more easily (*e.g.*, they receive lower interest rates, get more out of a given level of equity, or face lower refinancing costs). In times where credit markets are tight, the effect of an increase in housing wealth will be relatively smaller, but in times where credit is eased, the effect of an increase in housing wealth will be relatively larger. Muellbauer (2007) provides evidence of this sort of credit channel between wealth and consumption for data on the U.S., United Kingdom and other countries.

Muellbauer's (2007) explanation may explain what we observe in Figure 2. Each period, the response of consumption to the tangible wealth shock mirrors closely the response of liabilities to the same shock (this is also the case in Figure 3 with the financial wealth shock). In the earliest period, in particular, the correlation between the responses of consumption and liabilities may reflect decisions by some households (manifested in the aggregate) to reduce debt and consumption to boost savings. Sheiner (1995) and Engelhardt (1996b) provide evidence that high home prices do, in fact, induce renters to save more. The response of consumption in the 1952 to 1973 suggests there is little to no wealth effect for that period; the lack of a wealth effect in the early period for our data is consistent with Muellbauer's (2007) finding that there is no housing price-wealth effect in U.K. data before 1980 (Muellbauer's analysis on the U.S. only extends back to 1978).

By the 1998 to 2009 sample, the responses of consumption and liabilities to the tangible wealth shock are both positive (with the responses larger and with sustained statistical significance in contrast to the preceding sub-samples). This is what one might expect to see if all households have greater access to credit thereby reducing the constraint on consumption for younger households and allowing home owners to reap the gains of their housing collateral as suggested by Muellbauer (2007). While Muellbauer (2007) suggests the effect for young households may not be as strong in the U.S. as he documents for the U.K., the response of consumption from 1998 to 2009 suggests the effect may indeed be economically significant.²⁴

Moreover, the "collateral" effect may explain the differing responses of consumption from 1998 to 2009 to tangible wealth and financial wealth, respectively. In Figure 3, liabilities increase only briefly in response to the financial wealth shock (while consumption increases for at least a year after the shock). In Figure 2, the increase in liabilities is relatively large and sustained. The change in tangible wealth results in Muellbauer's collateral effect; whereas, it is unlikely a household would borrow against a change in their retirement portfolio as they would with housing wealth.

²⁴ Muellbauer (2007) cites survey data on U.S. households for 1978 to 2005. We did not compare our results to data on the U.K., nor did we compare the behavior of young households to older households. However, applying the empirical approach of our paper to data on the U.K. (or other countries), or to data that includes information on demographics are obvious extensions of our paper.

3.4.2 Common Cause versus a direct Wealth Effect

Attanasio *et al.* (2009) argue that the apparent wealth effect is actually the response of both consumption and housing wealth to a change in expectations on future income generated, say, by a change in productivity (see also Attanasio and Weber (1994)). The implication of their productivity hypothesis is that the consumption of both young (primarily renters) and old households (primarily homeowners) will increase in concert with an increase in housing prices, but the increase for the young will be more substantial. The young should benefit more from an expected increase in lifetime income (*i.e.*, they are not as close to retirement as the older, homeowners).

If a direct wealth effect is evident, however, the wealth effect will be evident primarily for older homeowners. In contrast, young households are less likely to increase their consumption following an increase in housing prices (and may even reduce consumption if they have to save more to afford a down payment). With data on U.K. households, the authors find in favor of the “common cause” or productivity hypothesis. Attanasio *et al.s*’ (2009) result, however, stands in direct contrast to Campbell and Cocco (2007) who find little evidence of a wealth effect for “young renters.” Instead, the latter find a significant wealth effect for older homeowners in the U.K. (note, Paiella (2009) provides discussion and a number of references related this debate).

While our data does not provide for a cohort analysis, the differing responses of consumption over time to the two forms of wealth may provide some insight into the “common cause” debate. In the U.S., it is well known that productivity growth slowed down in the 1970s and picked up in the 1990s. Consistent with the “common cause” argument, we would expect the 1970s to be an era of tempered expectations about future income and evidence of a relatively muted wealth effect for either tangible or financial assets. In contrast, the 1990s, in particular, would be an era of rising expectations of future income, which may be consistent with and strong wealth effect for financial or tangible assets. Of course, with the stock market boom of the 1990s, the link between income expectations, consumption and financial wealth may be most apparent; while after 1998, the productivity hypothesis may be more evident for the tangible wealth (in so

far as income expectations were sustained into the 2000s or emboldened by the increase in housing prices over that decade).

From Figures 2 and 3, evidence for the productivity hypothesis is mixed. First, consistent with the productivity hypothesis, in the 1998 to 2009 sub-sample, both tangible assets and consumption show their strongest, and sustained, positive responses to a shock to tangible assets (as does income). In the prior two sub-periods, neither the response of consumption nor income shows the same strong responses as in the post-1998 sample. However, while we would expect the weakest responses in the second sub-sample of 1973 to 1985—consistent with the productivity slow down—instead we find the weakest tangible wealth effect in the 1953 to 1973 sample.

In Figure 3, the productivity hypothesis might be evident in the 1985 to 1998 period. The latter half of this sub-sample, at least, is associated with an increase in productivity. From 1985 to 1995, income shows its most sustained positive response to the financial wealth shock of any period, with an accompanying increase in consumption. However, the responses of consumption in the earlier periods, as discussed above, are actually larger in magnitude than the 1985 to 1998 sample which suggests that the financial wealth effect does not appear to be somehow uniquely associated with the 1990s.²⁵

In Figure 5, the shock to income provides similar mixed evidence for the productivity hypothesis. The shock to income is the least persistent in the 1998 to 2009 period. Yet, there is still a positive response by consumption and tangible assets. Consistent with the productivity hypothesis, this could indicate an expectations effect whereby the increase in income leads to an increase in expected future income and corresponding increases in consumption and tangible assets.

On the other hand, though, we would expect particularly weak responses by consumption and tangible assets in the earlier periods. A shock to income in the earlier periods is less likely to be associated with an increase in productivity and a subsequent revision of expectations for higher future income. However, in the pre-1985 periods, the

²⁵ In defense of the productivity hypothesis, since the increase in productivity likely began sometime after 1990, our estimated sub-samples do not offer a “clean” vetting of the hypothesis—it may be the case that the early part of our 1985 to 1998 sub-sample is masking clearer evidence in favor of the Attanasio *et al.* (2009) argument.

responses of tangible wealth to the income are still statistically significant (though smaller in magnitude than for the 1998 to 2009 sample). Consumption, too, does not show any less of a response to the income shocks than in the 1998 to 2009 period (again, if the shock to income is associated with a change in expectations in any period, then the productivity hypothesis suggests the response of consumption would be larger).

3.4.3 Permanent versus Transitory Changes to Wealth

Alternatively, the varying responses of consumption across time and across wealth might be explained by “bubble” behavior—households may mistake a transitory change in wealth for a permanent one. Based on the permanent income hypothesis an anticipated change in permanent income (comprised of labor income and wealth) will have an effect on consumption while a temporary change in current income or wealth should not; moreover, if a household experiences an unexpected shock to current wealth, consumption may respond in so far as households perceive the change to affect permanent income.²⁶ With respect to “bubble” behavior, asset price bubbles represent temporary increases in household wealth, which may be mistaken for an increase in permanent income. *Ergo*, an increase in consumption in response to a temporary rise in asset prices is consistent with an asset price bubble (see White (2006) for a thorough discussion of asset price bubbles).

Impulse response functions—which measure the response of a variable to an exogenous, one-time transitory shock—are a useful tool for us to consider the possibility that the differing responses we document indicate a bubble. If individuals properly identify a temporary change for what it is, then for a transitory positive shock to income or wealth (again, the two components of permanent income), we expect essentially no response. If individuals do not properly identify the shock as temporary, then they will respond as if the shock is permanent. In that case, for a positive shock to income or wealth, we expect to see “excess” consumption, which we define as a larger than expected, positive response in consumption.²⁷

²⁶ See DeJuan and Seater (2006) or Manitsaris (2006) for a description of the traditional permanent income hypothesis.

²⁷ As there is no precise definition, we simply define larger than expected to mean a noticeably positive and statistically significant response by consumption. The “excess sensitivity” of consumption to a change in

The relative responses of consumption displayed in Figures 2 may be indicative of excess consumption in the 1998 to 2009 period; in the least, we speculate as much based off the varying sub-samples and varying response to the wealth categories. In the 1998 to 2009 period, we observe a positive and sustained response by consumption to a temporary shock to tangible assets. Also, following the shock, there is a sustained rise in tangible assets (a possible bubble) and corresponding rise in liabilities, making that era look like a debt-fueled binge. As discussed in Section 3.4.2, in this same era, the transitory shock to income leads to a rise in tangible wealth and consumption that seems unusually positive. Taken together, the sustained positive responses in liabilities, tangible assets, financial assets, and disposable income may be evidence of an asset bubble in the 1998 to 2009 period.

4. Conclusion

While impulse response functions cannot provide conclusive evidence about why consumption responds (or does not) to a change in wealth, we interpret the response of consumption across the sub-periods as supporting Muellbauer’s (2007) “credit-constraints” explanation of the wealth effect. Of course, our analysis does not rule out Attanasio *et al.s*’ (2009) productivity hypothesis; however, in the least, our various sub-sample comparisons does not offers compelling support of that story. Moreover, the framework we have used in this paper to analyze the dynamic changes in consumption—sub-sample comparisons of the change in consumption to the same, unanticipated shock to wealth—may offer some insight into whether an asset price bubble might explain the observed wealth effect. Of course, given the difficult in identifying asset price bubbles, our discussion on this point is thus far only speculative.

On a more conclusive note, we have identified in this paper a change in the relative wealth effects over time. Not only is the response of consumption to tangible wealth shock larger (in absolute value) than in the corresponding period for the financial wealth shock, the apparent economic significance of the tangible wealth effect gets larger over time, while the response of consumption to financial wealth diminishes over time.

income is referred to in the permanent income literature when consumption responds to a change in current income.

Moreover, we are able to measure statistically significant responses of consumption across our sub-samples with the methodology of Jordà (2005, 2009). Typically, the small sample sizes inherent to sub-sample comparisons (especially at the quarterly frequency) would mean our estimation may suffer from low power—leading to a tendency for the impulse responses to not be statistically different from zero. That might mean a bias towards concluding there is little to no response of consumption to a shock to wealth. Here are able to avoid that problem as well as other issues in the wealth effect literature. As such, the findings and approach of this paper may be useful to those interested in the wealth effect; our approach can easily be applied to estimating the wealth effect for other countries, or over different or shorter time periods.

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Table 1 Estimated Breaks for Single Variables 1952:1 to 2009:4

Series	<i>Consumption</i>	<i>Liabilities</i>	<i>Tangible Wealth</i>	<i>Financial Wealth</i>	<i>Income</i>
ARIMA(1,0,0)		1983:1 (1982:3, 1983:4)			
		1990:1 (1988:3, 1990:3)			
		2003:2 (2002:1, 2003:2)			
ARIMA(0,1,0)			1988:1 (1973:2, 1998:1)		
ARIMA(1,1,0)			1981:4 (1978:4, 1988:2)		1975:1 (1965:4, 1985:3)

Notes: Break dates (in bold) and 95 percent asymmetric confidence intervals (reported in parantheses) are estimated using the methods of Bai and Perron (1998, 2003). The variables, explanation of model specifications, and data sources are reported in the text. Results are not reported for the simplest ARIMA(0,0,0) model given a trend in each series.

Table 2 Estimated Breaks for Ratios 1952:1 to 2009:4

Series	<i>Liabilities to Income</i>	<i>Income to Tangible Wealth</i>	<i>Income to Financial Wealth</i>	<i>Income to Net Worth</i>	<i>Income to Total Wealth</i>	<i>Consumption to Income</i>	<i>Cosumption to Net Worth</i>
Model	Estimted Breaks						
	1959:4 (1959:3, 1960:2)	1963:4 (1963:3, 1964:2)	1958:1 (1957:3, 1958:3)	1969:3 (1969:1, 1969:4)	1957:4 (1957:2, 1958:4)	1963:4 (1963:1, 1964:2)	1958:2 (1956:2, 1961:2)
	1983:3 (1982:3, 1983:4)	1971:4 (1969:4, 1972:3)	1967:2 (1963:1, 1968:1)	1986:1 (1985:4, 1986:3)	1969:4 (1969:2, 1970:2)	1984:3 (1982:2, 1985:4)	1969:3 (1968:2, 1969:4)
ARIMA(0,0,0)	1989:2 (1989:1, 1990:2)	1978:4 (1978:3, 1979:2)	1973:1 (1972:4, 1973:4)	1997:1 (1995:4, 1997:2)	1986:3 (1986:2, 1987:1)	1990:2 (1990:1, 1990:4)	1985:4 (1985:2, 1986:1)
	1997:1 (1996:2, 1997:2)	1985:2 (1981:2, 1986:2)	1986:1 (1985:4, 1986:2)		1995:3 (1995:1, 1995:4)	1998:1 (1996:3, 1998:4)	1997:1 (1994:4, 1997:2)
	2002:4 (2002:3, 2003:1)	2001:3 (2000:2, 2001:4)	1995:2 (1994:4, 1995:3)		2003:4 (2000:1, 2004:4)	2003:4 (2003:2, 2004:2)	
	1965:2 (1959:4, 1967:3)						
	1984:3 (1983:3, 1985:1)						
ARIMA(1,0,0)	1992:4 (1992:1, 1996:1)					1989:1 (1986:2, 1993:2)	
	2002:2 (1996:3, 2002:3)						
ARIMA(0,1,0)	none	none	none	none	none	none	none
	1963:3 (1957:4, 1964:3)						
ARIMA(1,1,0)	1984:3 (1969:2, 1987:1)						
		2002:2 (1998:3, 2008:3)		2002:1 (1997:3, 2009:3)	2002:1 (1996:3, 2009:4)		2003:2 (1999:3, 2009:4)

Notes: See notes to Table 1. See section 2 for variable definitions.

3. Unit Root Tests for a Cointegrating Relationship, 1952:1 to 2009:4

Test: Ho: Variable is I(1)	Augmented Dickey-Fuller Test			Phillips-Perron Test		
	No Intercept or Trend	Intercept Only	Intercept and Trend	No Intercept or Trend	Intercept Only	Intercept and Trend
Consumption on Constant, Disposable Income, Net Worth	-3.21 [0.00]	-3.20 [0.02]	-3.27 [0.07]	-2.99 [0.00]	-2.98 [0.04]	-3.18 [0.09]
Consumption on Constant, Disposable Income, Total Assets	-3.47 [0.00]	-3.46 [0.00]	-3.53 [0.04]	-3.39 [0.00]	-3.38 [0.01]	-3.44 [0.04]
Consumption on Constant, Disposable Income, Financial Assets, Tangible Assets	-3.34 [0.00]	-3.33 [0.01]	-3.39 [0.06]	-3.34 [0.00]	-3.33 [0.01]	-3.17 [0.09]
Consumption on Constant, Disposable Income, Financial Assets, Tangible Assets, Liabilities	-3.37 [0.00]	-3.36 [0.01]	-3.44 [0.05]	-3.15 [0.00]	-3.14 [0.03]	-3.34 [0.06]

Notes: The residual series are generated from regressions of log consumption on log disposable income and the balance sheet variables listed, respectively. The null hypotheses of both unit root tests are that the series contains a unit root. The table displays test statistics with associated p-values (in brackets). Augmented Dickey-Fuller test with Schwarz criterion selection of up to 8 Lags. Phillips-Perron test with automatic bandwidth selection using Newey-West bandwidth.

Table 4 Johansen Cointegration Test Statistics, 1952:1 to 2009:4

Test Assumptions About Data	No Trend in Data		Linear Trend in Data		Quadratic Trend in Data
Test Assumptions About Cointegrating Relationship	No Intercept or Trend	Intercept Only	Intercept Only	Intercept and Trend	Intercept and Trend
Number of Cointegrating Relationships At Most	Trace Statistics [p-value]				
0	120.09 [0.00]	141.56 [0.00]	83.09 [0.00]	107.93 [0.00]	104.27 [0.00]
1	53.30 [0.00]	71.74 [0.00]	49.41 [0.04]	74.03 [0.01]	70.51 [0.00]
2	20.21 [0.15]	38.24 [0.02]	20.52 [0.39]	43.21 [0.05]	39.99 [0.01]
3	7.27 [0.30]	16.12 [0.17]	8.46 [0.42]	16.61 [0.44]	13.56 [0.21]
4	1.71 [0.22]	4.73 [0.31]	0.14 [0.71]	4.62 [0.65]	4.60 [0.03]

Notes: In each test we specify one lag (in differences) or two lags (in levels). P-values for each statistic are displayed in brackets.

Table 5 Structural Break Tests on Residual Series 1952:1 to 2009:4

Residuals from Regression:	Consumption on Constant, Disposable Income, Net Worth	Consumption on Constant, Disposable Income, Total Assets	Consumption on Constant, Disposable Income, Financial Assets, Tangible Assets	Consumption on Constant, Disposable Income, Financial Assets, Tangible Assets, Liabilities
ARIMA(0,0,0)	1962:4 (1962:2, 1963:2)	1961:2 (1960:3, 1961:4)	1962:4 (1962:2, 1963:2)	1961:2 (1960:4, 1962:1)
	1974:1 (1973:3, 1975:4)	1974:1 (1973:3, 1975:4)	1974:1 (1973:2, 1975:3)	1974:1 (1973:2, 1975:3)
	1983:4 (1982:4, 1985:1)	1983:4 (1982:4, 1984:4)	1983:4 (1982:4, 1984:4)	1983:4 (1982:4, 1985:1)
	1990:1 (1987:1, 1990:2)	1990:1 (1989:4, 1990:2)	1990:2 (1990:1, 1990:3)	1990:2 (1990:1, 1990:3)
		1997:1 (1990:1, 1997:3)	1997:1 (1990:4, 1997:3)	1997:1 (1991:4, 1997:3)
ARIMA(1,0,0)	none	none	none	none
ARIMA(0,1,0)	none	none	none	none
ARIMA(1,1,0)	none	none	none	none

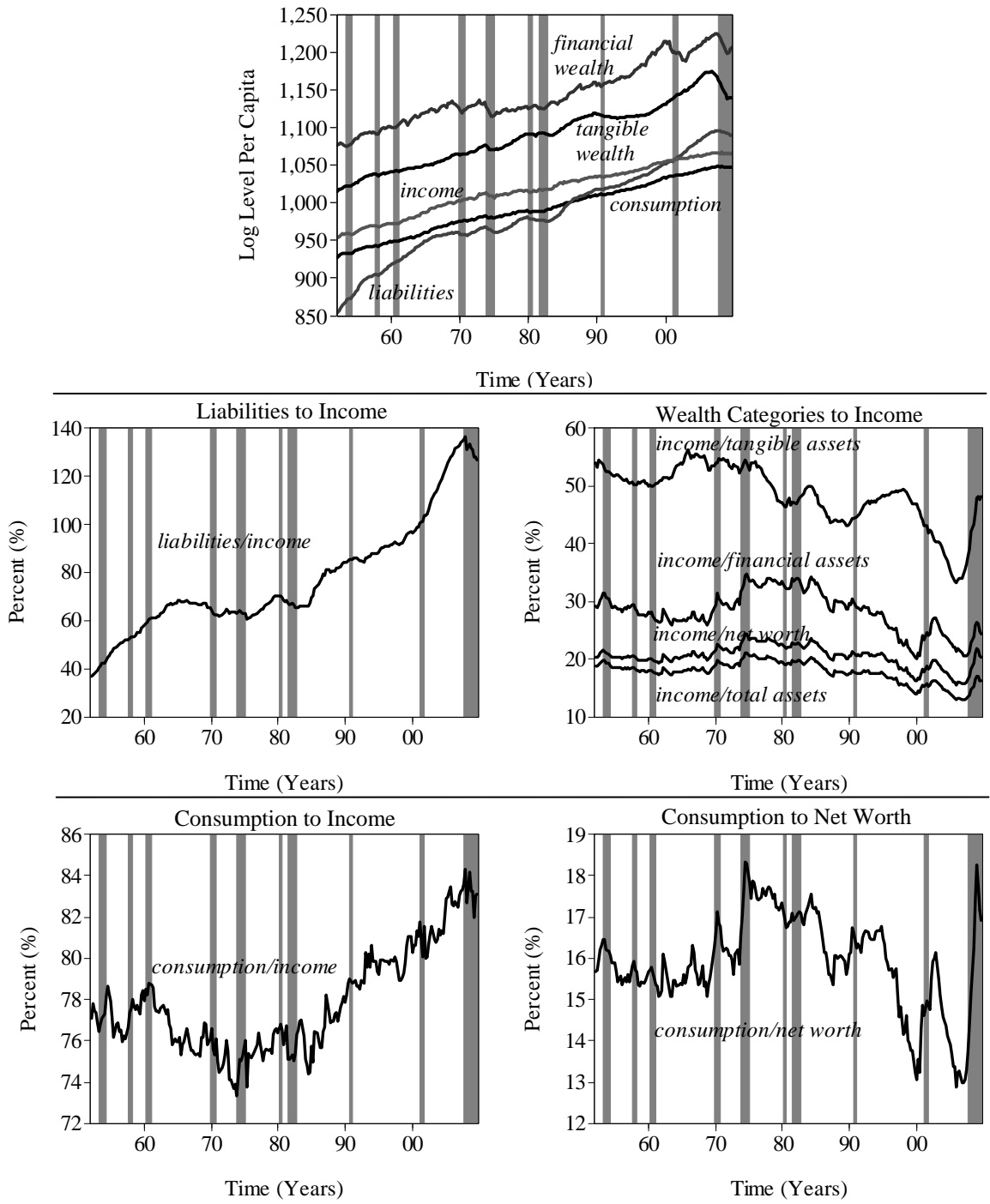
Notes: Break dates (in bold) and 95 percent confidence intervals (in parantheses) estimated using the Bai and Perron (1998, 2003) methodology. See notes to Table 3 (and section 2.4 in the text) for description of the residual series.

Table 6: Structural Breaks on 5-variable system: 1952:1 to 2009:4

Maximum Number of Breaks	Break Dates (95% Confidence Interval)		
1	1998:2 (1998:1, 1998:3)		
2	1984:4 (1984:3, 1985:1)	1998:2 (1998:1, 1998:3)	
3	1973:3 (1973:2, 1973:4)	1985:1 (1984:4, 1985:2)	1998:2 (1998:1, 1998:3)

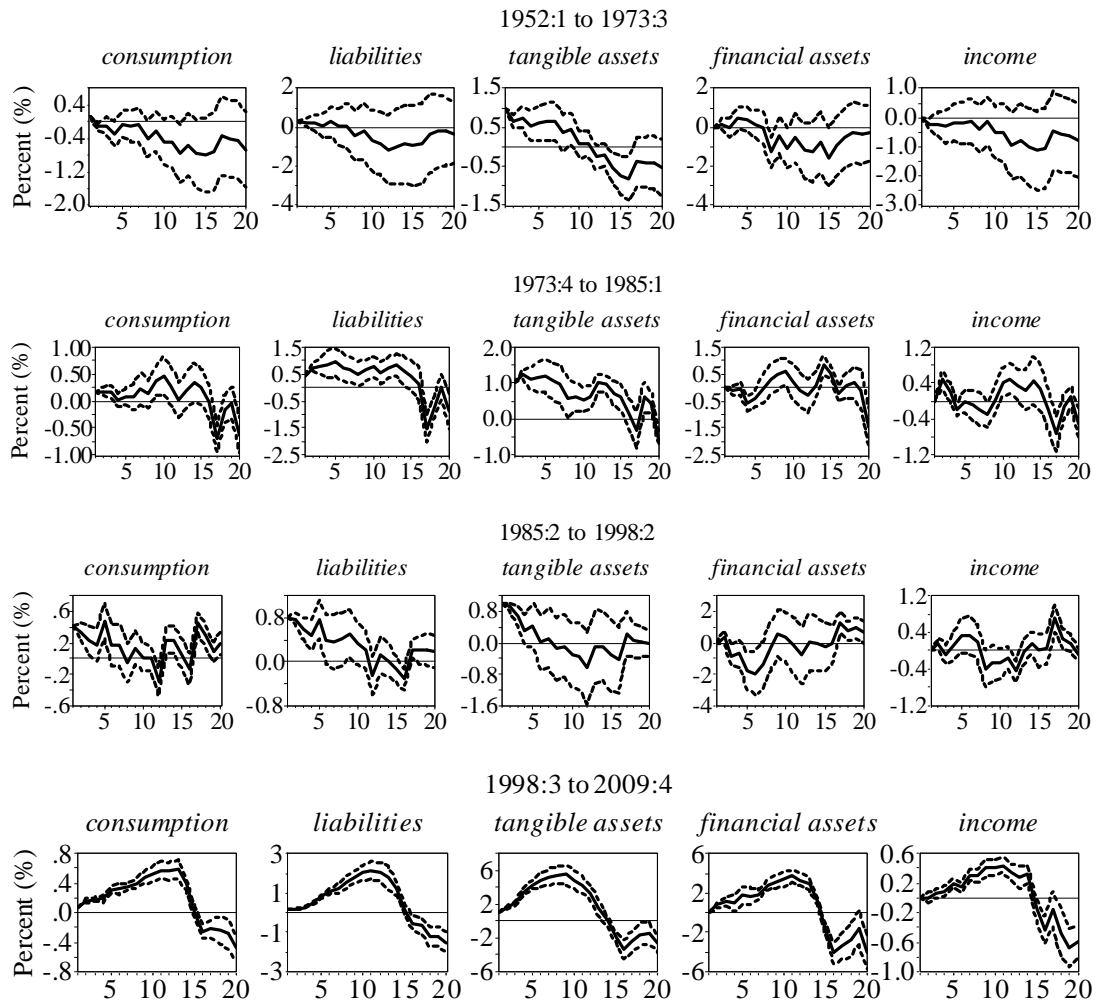
Notes: Break dates (in bold) and associated 95 percent confidence intervals (in parantheses) are estimated using the Qu and Perron (2002) method. The five-variable system includes consumption, income, liabilities, financial wealth, and tangible wealth. See text for additional details.

Figure 1: Log-Levels and Ratios of Consumption, Wealth, Liabilities and Income



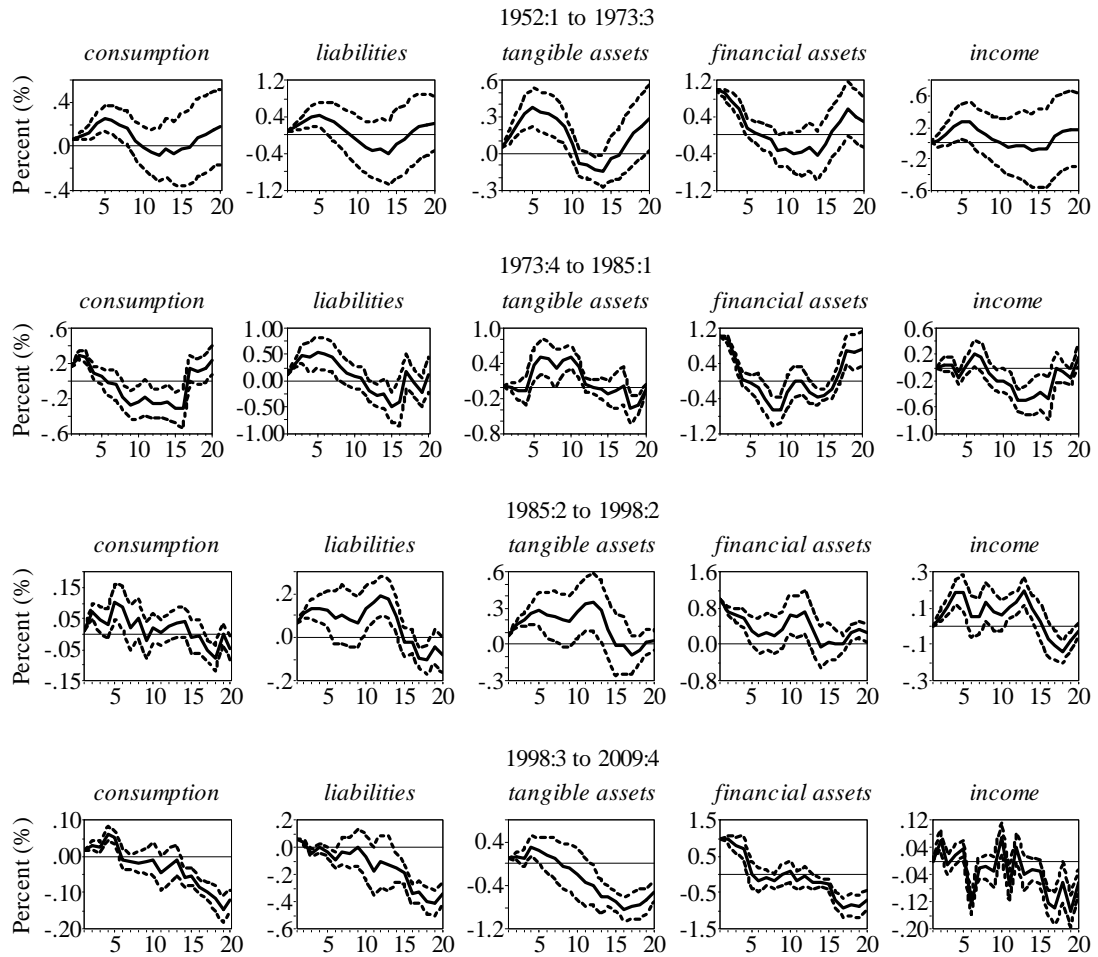
Notes: Shaded bars indicate NBER-dated recessions. Variables are defined in the text.

Figure 2: Impulse Responses from a one percent shock to Tangible Assets



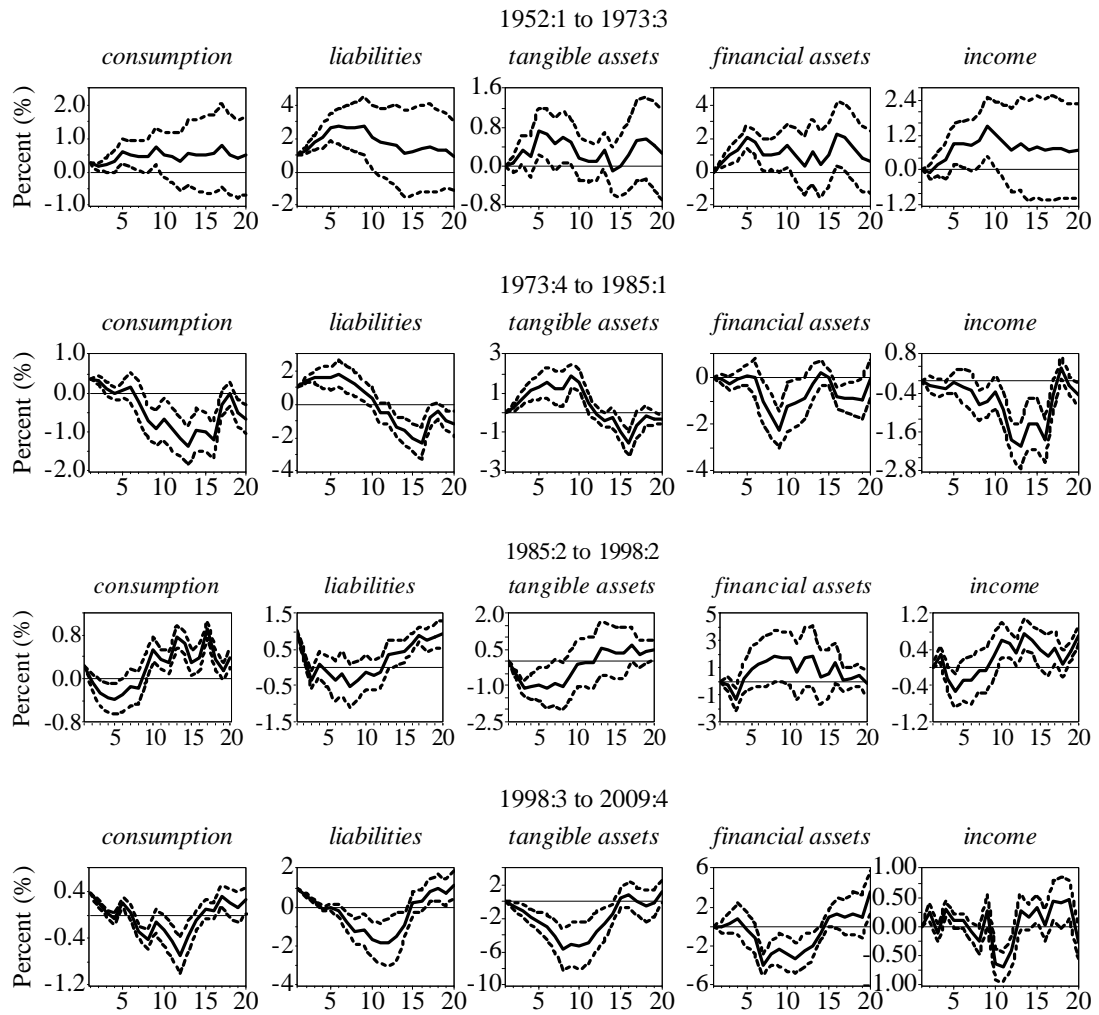
Notes: The impulse response functions (IRFs) are estimated using Jordà's (2005) linear projection technique. The solid line represents the impulse response function; the dashed lines are Jordà's (2009) 95 percent conditional confidence bands. The horizon for each IRF is measured in quarters. See text for variable definitions.

Figure 3: Impulse Responses from a one percent shock to Financial Assets



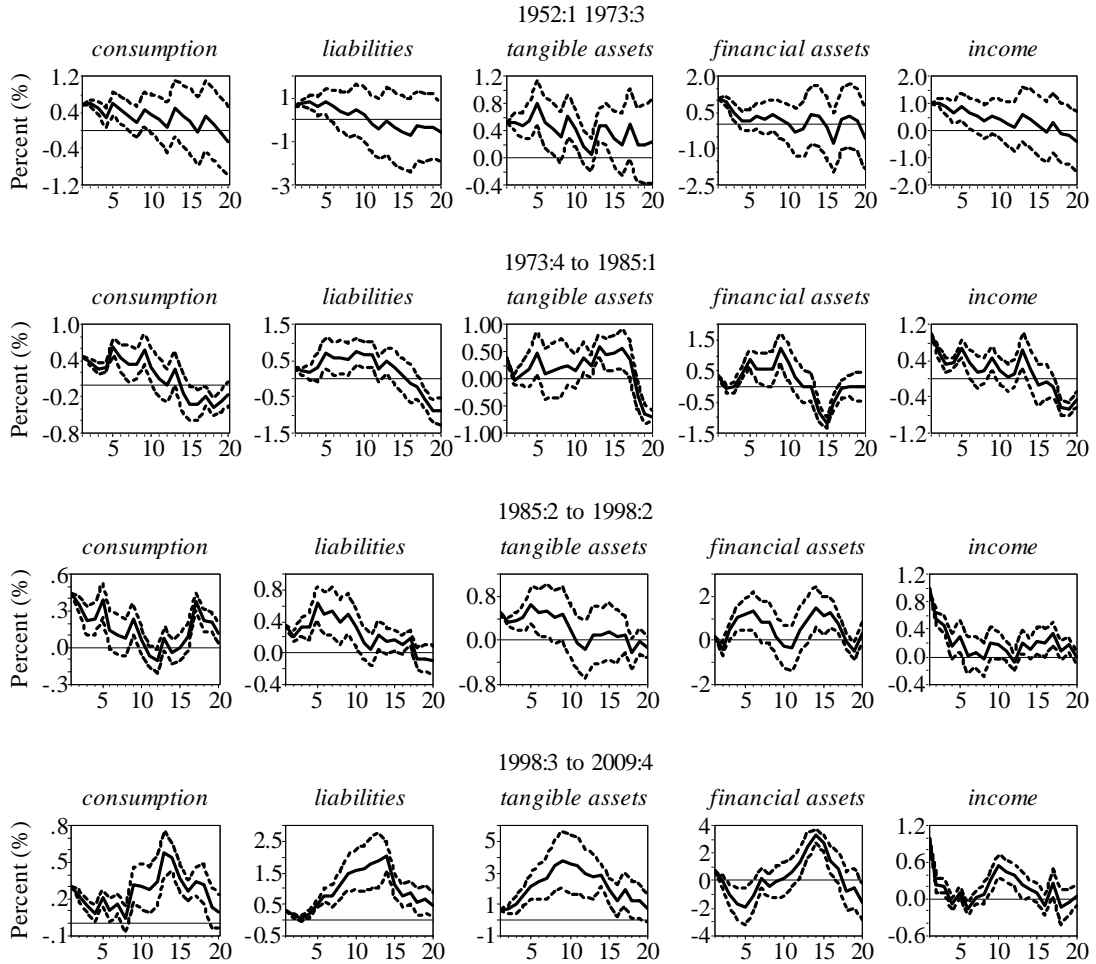
Notes: See notes to Figure 2.

Figure 4: Impulse Responses from a one percent shock to Liabilities



Notes: See notes to Figure 2.

Figure 5: Impulse Responses from a one percent shock to Income



Notes: See notes to Figure 2.