HOUSEHOLD BALANCE SHEETS, CONSUMPTION, AND THE ECONOMIC SLUMP

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HOUSEHOLD BALANCE SHEETS, CONSUMPTION, AND THE ECONOMIC SLUMP*

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We investigate the consumption consequences of the 2006–9 housing collapse using the highly unequal geographic distribution of wealth losses across the United States. We estimate a large elasticity of consumption with respect to housing net worth of 0.6 to 0.8, which soundly rejects the hypothesis of full consumption risk-sharing. The average marginal propensity to consume (MPC) out of housing wealth is 5–7 cents with substantial heterogeneity across ZIP codes. ZIP codes with poorer and more levered households have a significantly higher MPC out of housing wealth. In line with the MPC result, ZIP codes experiencing larger wealth losses, particularly those with poorer and more levered households, experience a larger reduction in credit limits, refinancing likelihood, and credit scores. Our findings highlight the role of debt and the geographic distribution of wealth shocks in explaining the large and unequal decline in consumption from 2006 to 2009. *JEL* Codes: E21, E32, E44, E60.

I. INTRODUCTION

How does consumption respond to large negative shocks to household wealth? Do households with different levels of wealth have different marginal propensities to consume out of a dollar lost? These questions are fundamental in macroeconomics and finance, and the answers have profound implications for how we model the economy, how wealth shocks translate into business cycle fluctuations, and how policy should respond when asset prices collapse.

For example, most traditional models of the macroeconomy adopt a representative agent framework, implicitly assuming

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that individual households are hedged against household-specific wealth shocks. However, if this assumption is grossly violated in data, then we may need to adopt heterogeneity in our models. An important source of heterogeneity emphasized by the literature on consumption under uncertainty is that the marginal propensity to consume (MPC) out of wealth declines with wealth. Such heterogeneity in the MPC implies that the *distribution* of dollar losses across the economy matters for consumption dynamics.

These questions are especially important when considering severe recessions. In the United States, both the Great Depression and Great Recession were preceded by a large accumulation of household debt and followed by a collapse in asset prices and consumption.[1] Prominent economists such as Irving Fisher, Mervyn King, and James Tobin have argued that a higher MPC out of wealth for borrowers versus savers explains why elevated private debt burdens are associated with economic downturns.[2] However, we do not know of any extant research showing that more levered households have higher MPCs.

This article provides detailed empirical evidence on the distribution of wealth shocks across the U.S. population at the onset of the Great Recession and on the consumption consequences of these wealth shocks. We construct a new data set that enables us to observe changes in household consumption and wealth at the county and ZIP code levels.

We begin by documenting the percent change in household net worth at the ZIP code level between 2006 and 2009 that comes from the collapse in house prices, what we call the *housing net worth shock*. ZIP codes across the United States vary tremendously in the impact of the housing shock on their balance sheets. For example, the bottom decile of ZIP codes lost 45% of their net worth, whereas the top decile experienced a slight increase in net worth.

We then examine whether the collapse in housing net worth affects consumption. If households have sufficient mechanisms to

1. See for example Persons (1930), Temin (1976), Mishkin (1978), and Olney (1999) for evidence on the Great Depression. For the Great Recession, National Income and Product Accounts and census retail sales data show a definitive collapse in durable consumption even before the fall of 2008.

2. Cross-country business cycle studies by the International Monetary Fund (2012), Jordà, Schularick, and Taylor (2012), and Glick and Lansing (2009, 2010) show that the presence of a high level of household debt leads to deeper recessions.

insure their consumption against wealth shocks, as implicitly assumed by representative agent models, we should not see local consumption responding to the local housing net worth shock. However, the data clearly reject the consumption risk-sharing assumption. We estimate an elasticity of consumption with respect to the housing net worth shock across counties of between 0.6 and 0.8.

The consumption theoretical literature (e.g., Carroll and Kimball 1996) emphasizes that when households are faced with uninsurable income and wealth risk, their MPC out of wealth declines with wealth; that is, the consumption function is concave in wealth. Similarly, King (1994) highlights that the MPC out of wealth may be higher for credit-constrained households. Understanding whether there is heterogeneity in the MPC is important because heterogeneity implies that the *distribution* of wealth losses, not just their overall level, may affect aggregate consumption.

We estimate an average MPC between 5–7 cents for every dollar decline in home values. The MPC varies by the type of expenditure, with the MPC highest for durable goods such as automobile purchases and smallest for groceries. However, the key question is whether this MPC varies by household income, wealth, or leverage.

We find evidence supportive of heterogeneity in the MPC by household income and leverage. For example, the MPC for households living in ZIP codes with an average annual income of less than $35,000 is three times as large as the MPC for households living in ZIP codes with more than $200,000 in average income. Similarly, ZIP codes that entered the Great Recession with a housing loan-to-value (LTV) ratio of 90% had an MPC out of housing wealth that was three times as large as the MPC of households in ZIP codes with only a 30% housing LTV ratio. Taken together, these results show that the distribution of wealth losses matters, not just the level.

Our estimation strategy exploits cross-sectional variation in housing wealth shocks across the United States. An important factor driving cross-sectional variation is differences in housing supply elasticity across counties. Earlier work such as Mian and Sufi (2009, 2010, 2011) has used housing supply elasticity as an instrument for house price growth from 2002 to 2006. A reversal of the same cross-sectional pattern generates substantial variation in the cross-sectional decline in housing wealth from 2006 to 2009. We therefore use housing supply elasticity as an instrument for a city's exposure to the housing boom–bust cycle.[3]

Our estimated MPC includes three channels through which the change in housing wealth might affect household spending. The first channel is the direct "wealth effect." The second is the indirect effect due to the feedback effect from the nontradable employment sector. In particular, given the decline in spending is so dramatic in hard-hit areas, nontradable employment is disproportionately affected (see Mian and Sufi 2012 for evidence). This knock-on effect on local nontradable employment further depresses local spending. Third, housing net worth serves as collateral for access to credit; a decline in housing net worth can force households to cut back spending due to credit constraints.

We provide direct evidence for the credit constraints channel. We find that for a given decline in home values, ZIP codes with a high housing LTV ratio and low income experience a larger drop in home equity limits and a reduced ability to refinance into lower interest rates. Moreover, for a given dollar decline in home value, more levered ZIP codes and poorer ZIP codes see a larger drop in credit scores.

Our key contribution is to highlight the heterogeneity in MPC with respect to income and leverage in response to a financial shock. A natural implication of heterogeneity in the MPC is that an economy's ability to share risk across households matters for the aggregate economy (e.g., Carroll 2013). For example, higher leverage in the economy concentrates losses on debtors. If leverage also increases the MPC for indebted households, the real effect of a given aggregate loss in wealth may be amplified. We discuss the quantitative implications of our results in more detail later.

The remainder of our article is structured as follows. We discuss the theory, related literature, and relevant general equilibrium questions in the next section. Section III presents the data and summary statistics. Section IV discusses variation in net worth shocks across counties. Sections V and VI present the results, and Section VII concludes.

3. See our discussion in the empirical section for why the cross-sectional variation in housing wealth is not spuriously correlated with industry-specific shocks, such as the construction sector.

II. THEORY

II.A. Benchmark

How should household consumption respond to wealth shocks? The benchmark representative agent model assumes that households can perfectly insure each other against consumption risk. Hence consumption growth for household i is completely insensitive to the idiosyncratic changes in wealth.

Let $\Delta logC_t^i$ be the natural log change in consumption for household i, and $\Delta LogX_t^i$ be the natural log change in wealth. The representative-agent consumption risk insurance assumption implies an elasticity of consumption with respect to wealth, β, of zero:

$$\Delta logC_t^i = \alpha_t + \beta * \Delta logX_t^i + \varepsilon_t^i. \tag{1}$$

Equation (1) can be derived under the assumption of complete markets (Cochrane 1991). However, Constantinides and Duffie (1996), Telmer (1993), and Heaton and Lucas (1992, 1996) point out that the relationship in equation (1) can also be obtained under less restrictive assumptions of incomplete markets and limited borrowing capacity. Moreover, in the context of housing wealth, Campbell and Cocco (2007) and Sinai and Souleles (2005) have shown that consumers are naturally hedged against negative housing wealth shocks since they must consume housing services going forward. This is another reason β may be zero in equation (1) when the change in housing wealth is the right-hand variable.

There is a large literature devoted to estimating equation (1). Most of these studies reject the strict hypothesis of full risk-sharing (e.g., Cochrane 1991; Attanasio and Davis 1996). However, Schulhofer-Wohl (2011) argues that accounting for heterogeneity in risk preferences and endogenous job selection brings consumption close to full risk-insurance in the data.[4]

Our own estimation of equation (1) easily rejects the consumption risk-sharing hypothesis (see Section V.A). One possible

4. See also the large literature on the housing wealth effect, which is too large to be completely summarized here. It includes Attanasio and Weber (1994), Muellbauer and Murphy (1997), Lehnert (2004), Case, Quigley, and Shiller (2005, 2013), Haurin and Rosenthal (2006), Campbell and Cocco (2007), Greenspan and Kennedy (2008), Bostic, Gabriel, and Painter (2009), and Carroll, Otsuka, and Slacalek (2011).

explanation for the rejection of the perfect risk-sharing model is heterogeneous beliefs. With heterogeneous beliefs households may deliberately choose to load up on idiosyncratic risk that they are more optimistic about. Once the optimistic scenario does not pan out, optimistic agents—assuming log utility as in Merton (1971)—cut consumption because consumption is a constant fraction of wealth.

II.B. Consumption under Limited Risk-Sharing and Uncertainty

The analytics of consumption under uncertainty are summarized by Carroll and Kimball (1996). The authors show that with labor and asset price uncertainty, households with a precautionary savings motive (i.e., $u''' > 0$, such as in constant relative risk aversion preferences) have a concave consumption function. The consumption function is concave in wealth and permanent income. Consequently, the marginal propensity to consume out of a wealth shock, $\frac{\partial C_t^i}{\partial NW_t^i}$, declines with wealth. We can test for the concavity of consumption function by estimating:

$$\Delta C_t^i = \alpha_t + \beta_1 * \Delta NW_t^i + \beta_2 * NW_{t-1}^i + \beta_3 * \Delta NW_t^i * NW_{t-1}^i + \varepsilon_t^i. \tag{2}$$

Equation (2) is estimated using differences in nominal amounts (dollars). The key term of interest is β_3, which measures the degree to which the MPC out of a wealth shock varies by the ex ante net worth position of the household. The Carroll and Kimball (1996) framework implies that $\beta_3 < 0$, that is, the consumption of low-net-worth households responds more aggressively to changes in wealth.

While Carroll and Kimball (1996) emphasize a precautionary savings channel, a similar prediction emerges in models of credit constraints where net worth is a measure of such constraints (e.g., Bernanke and Gertler 1989; Kiyotaki and Moore 1997). For example, if the financial sector requires households to have sufficient net worth as collateral for borrowing, households with lower net worth would also show a higher MPC out of wealth shocks. As Carroll (2001) notes, “for many purposes the behavior of constrained consumers is virtually indistinguishable from the behavior of unconstrained consumers with a precautionary motive." A negative β_3 may be interpreted as either capturing precautionary savings or credit constraints.

II.C. Leverage, Financial Shocks, and Aggregate Implications

Equation (2) implies that the total reduction in consumption in response to a negative aggregate wealth shock depends on where the wealth shock is concentrated. If the wealth shock is concentrated among those with a high marginal propensity to consume, then the total effect is more severe. This observation provides an insight into why the decline in wealth of a levered asset class such as housing is often associated with a severe downturn in real activity. First, debtors tend to be less wealthy than average. Second, debt concentrates losses on the balance sheet of the debtors. The combination of these two factors implies that for a given decline in aggregate wealth, the consumption decline is larger when there is more debt in the economy.

Of course, the foregoing logic does not necessarily imply an aggregate consumption decline in general equilibrium. General equilibrium effects could mitigate the aggregate impact of lower spending by certain households. Such general equilibrium effects include changes in interest rates, goods prices, exchange rates, and investment. For example, a fall in the interest rate in response to a negative wealth shock may convince certain households to bring forward their consumption, thereby alleviating some of the initial adverse impact on aggregate consumption.

Although such general equilibrium forces are helpful, they may not be sufficient to prevent a dramatic decline in economic output. Several recent papers emphasize frictions in the economy, such as the zero lower bound on nominal interest rate, that make it difficult to reduce real interest rates sufficiently. Eggertsson and Krugman (2012) emphasize the zero lower bound friction in a general equilibrium model where a reduction in borrowing capacity forces levered household to cut back on consumption.

Guerrieri and Lorenzoni (2011) and Hall (2011) also highlight the zero lower bound friction in generating aggregate reduction in consumption. Midrigan and Phillipon (2011) emphasize liquidity shocks and wage rigidity that lead to a reduction in aggregate activity even away from the zero lower bound constraint. Huo and Rios-Rull (2012) generate an aggregate consumption-driven slump due to frictions in shifting from consumption to investment. Their model emphasizes the difficulty in quickly switching from investment in the production of nontradables to investment in the production of tradables in response to a consumption shock.

Much of this theoretical work has been inspired by the Great Recession, where evidence on these frictions is strong. For example, the federal funds rate and interest rates on short-term Treasury bills have been pinned at zero for an extended period. Despite massive expansion of the Federal Reserve's balance sheet, realized and expected inflation have remained very low by historical standards. There is considerable evidence of downward rigidity in wages despite elevated level of unemployment (Daly, Hobijn, and Wiles 2011; Fallick, Lettau, and Wascher 2011; Daly, Hobijn, and Lucking 2012). The external trade balance of the United States has not shown much improvement relative to the slowdown in the domestic economy. We have not seen much of an increase in investment despite firms maintaining large cash balances.

We do not attempt to identify the precise macroeconomic friction that is operative in the economy. It could very well be the case that many of these frictions are present. Instead, we focus on the drop in consumption itself that makes the macroeconomic frictions relevant.

III. DATA, MEASUREMENT, AND SUMMARY STATISTICS

This study introduces a new data set covering consumption and household wealth at the ZIP code and county level over time. We describe these data here.

III.A. Consumption

Micro-level consumption data are hard to obtain. They are either only available at an aggregate level[5] or measured through self-reported surveys that can have measurement issues.[6] This article introduces two new sources of consumption data based

5. Exceptions include Zhou and Carroll (2012) and Case, Quigley, and Shiller (2013), who measure spending at the state level based on sales tax revenues and disaggregated retail sales and employment data.

6. See, for example, Attanasio, Battistin, and Ichimura (2007) and Cantor, Schneider, and Edwards (2011) for criticism of the Survey of Consumer Expenditure in particular. Koijen, Van Nieuwerburgh, and Vestman (2012) match actual auto sales data with reported auto purchases in a survey and find an enormous amount of under-reporting by households.

on actual household expenditure, as opposed to survey responses. The first is ZIP code–level auto sales data from R.L. Polk from 1998 to 2012. These data are collected from new automobile registrations and provide information on the total number of new automobiles purchased in a given ZIP code and year. The address is derived from registrations, so the ZIP code represents the ZIP code of the person who purchased the auto, not the dealership.

The second source of consumption data is at the county level from 2005 to 2009 from MasterCard Advisors. These data provide us with total consumer purchases in a county that use either a credit card or debit card for which MasterCard is the processor. The data are based on a 5% random sample of the universe of all transactions from merchants in a county. An important advantage of the MasterCard data is that they break down total consumer expenditure by the NAICS code attached to the merchant providing the data. There are 10 categories for merchants we use: furniture, appliances, home centers (i.e., home improvement), groceries, health-related such as pharmacies and drug stores, gasoline, clothing, sports and hobby, department stores, and restaurants.[7] We group the MasterCard purchases into three categories: durable goods (furniture, appliances, home centers), groceries, and other nondurable goods (all remaining categories).

Further details on the MasterCard data are provided in the Online Appendix. In particular, we report how the MasterCard data compares to the aggregate retail sales information from the census. We also address concerns that cross-sectional consumption growth measured using credit card and debit card purchases may be affected by possible changes in the share of purchases conducted via credit and debit cards in the aftermath of the financial crisis. As we highlight in the Online Appendix, our alternative measure of consumption based on auto sales data from R.L. Polk does not depend on credit card purchases because it represents the universe of all new auto purchases regardless of the method of payment. The auto sales data can therefore be used as a cross-check on the results using MasterCard data. We also

7. These correspond to three-digit NAICS codes of 442, 443, 444, 445, 446, 447, 448, 451, 452, and 722, respectively. For more information on the exact types of stores included in each NAICS, see http://www.naics.com/free-code-search/sixdigitnaics.html?code=4445. These categories are identical to those used by the census measures of retail sales.

show in the Online Appendix that our results are robust to the use of census state-level sales tax revenue data as our measure of household spending. We therefore do not believe that using the MasterCard data as a measure of spending biases our results.

Because we want to estimate the marginal propensity to consume for a dollar change in housing wealth, we have to scale up the total spending in the MasterCard data to match *total* spending in a county. We do so using the census retail sales data in the following way.[8]

We match the three major spending categories in MasterCard data (nonauto durables, groceries, and other nondurables) to the nationwide census data. For each of these categories, we use the MasterCard data to calculate the fraction of spending in that category that belongs to a given county in 2006. In other words, a fraction is calculated for each county based on the proportion of total MasterCard purchases for that category. We then multiply this fraction by the *census* nationwide spending in that category in 2006 to convert the MasterCard spending number into the implied retail spending for that category in a county.

Our procedure is based on a simple proportionality assumption. For example, aggregate retail sales of groceries for the United States recorded in the census data as of 2006 was $525 billion. If a given county had MasterCard grocery purchases that were 5% of nationwide MasterCard grocery purchases in 2006, we would allocate 5% of $525 billion (or $26.25 billion) of grocery spending to the county. We then have an estimate of total expenditures on groceries in this county as of 2006, and by construction the total expenditures across all counties adds up to total retail sales from the census. We then use the growth in MasterCard expenditures from 2006 to 2009 to project the estimate of 2009 total grocery expenditures. We repeat this procedure for the remaining two expenditure categories: other durables and other nondurables.[9]

8. The census retail sales data are produced by the Bureau of Economic Analysis and are an estimate of aggregate expenditures by industry. They can be found at http://www.census.gov/retail/.

9. An alternative approach would be to only use the growth rates in spending in the MasterCard data themselves. For specifications estimating elasticities, this would be sufficient because elasticities are unit-independent. We conduct such specifications in the Online Appendix. However, for specifications estimating the MPC out of housing wealth, we must have the total level of expenditures to match the total dollar change in wealth.

For auto sales, we do not have expenditures. Instead, we only have the quantity of new autos purchased. We implement the same procedure, using the share of quantity purchased to allocate total census retail sales expenditures on autos. So a county with 10% of total R.L. Polk autos purchased in 2006 would be allocated 10% of all expenditures from the census retail sales on new autos in 2006. This introduces measurement error, as we do not have information on the change in prices across counties. If prices changed equivalently across all counties from 2006 to 2009, then there would be no measurement error. Whereas a disadvantage of the auto sales data is that we do not have prices, an important advantage is that we can measure new auto purchases at the more disaggregated ZIP code level.

III.B. Net Worth

The second key variable in our analysis is household net worth. We define net worth for households living in ZIP code i at time t as $NW_t^i = S_t^i + B_t^i + H_t^i - D_t^i$, where the four terms on the right-hand side represent market values of stocks, bonds, housing, and debt owed, respectively. We refer to stocks and bonds collectively as "financial wealth" and abstract away from human capital in our definition of net worth for now.

We start with an estimate of household net worth at the ZIP code level for the end of 2006, just prior to the onset of the housing and financial collapse. We compute the market value of stock and bond holdings (including deposits) in a given ZIP code using IRS Statistics of Income (SOI) data. The SOI data report the total amount of dividends and interest income received by households in a ZIP code. Under the assumption that a typical household is holding the market index for stocks and bonds, the share of total dividends and total interest income received by a ZIP code gives us the fraction of total U.S. stocks and bonds held by that ZIP code. We therefore allocate total financial assets from the Federal Reserve's Flow of Funds data to ZIP codes based on the proportion of total dividend and interest income received by the household.

The assumption that all households hold the market index of bonds and stocks introduces potential errors. For example, we know from work such as Coval and Moskowitz (1999) that individual investors exhibit home bias in their portfolio choice. We ignore cross-sectional variation in financial wealth that is driven by differential exposure to individual stocks. However, this omission is unlikely to materially bias the cross-sectional ranking of ZIP codes according to our measure relative to the true financial wealth. For example, as we show in the Online Appendix, our measure of financial asset holdings is highly correlated with income and education.

The key limitation of our methodology is that it is not very accurate for tracking time-series changes in financial wealth at the ZIP code level. The assumption that everyone holds the market index implies that cross-sectional differences in changes in financial wealth are entirely driven by differences in exposure to different asset classes as of 2006. As a result, we are going to underestimate the cross-sectional heterogeneity in changes in financial wealth.[10]

Although we are limited by data in computing ZIP code–level changes in financial wealth (i.e., stocks and bonds), the same is not true for computing housing wealth. We estimate the value of housing stock owned by households in a ZIP code using the 2000 decennial census data as the product of the number of homeowners and the median home value. We then project the housing value into later years using the Core Logic ZIP code–level house price index and an estimate of the change in homeownership and population growth. Finally, we measure debt using data from Equifax Predictive Services that tells us the total borrowing by households in ZIP code i in a given year. Mian and Sufi (2009) describe the ZIP code–level Equifax data in detail.

We use the foregoing procedure to compute household net worth as of 2006. The change in total net worth between 2006 and 2009 can then be computed as $\Delta NW^i_{06-09} = \Delta logp^S_{06-09} * S^i_{2006} + \Delta logp^B_{06-09} * B^i_{2006} + \Delta logp^{H,i}_{06-09} * H^i_{2006}$, where $\Delta logp_{06-09}$ denotes the natural logarithmic change in the relevant price index from 2006 to 2009. Throughout, we split the change in net worth into the change in *financial wealth*, $(\Delta logp^S_{06-09} * S^i_{2006} + \Delta logp^B_{06-09} * B^i_{2006})$, and the change in housing wealth, $\Delta logp^{H,i}_{06-09} * H^i_{2006}$. The financial wealth and housing wealth changes can also be expressed in percentage terms as $\frac{(\Delta logp^S_{06-09} * S^i_{2006} + \Delta logp^B_{06-09} * B^i_{2006})}{NW^i_{2006}}$ and $\frac{\Delta logp^{H,i}_{06-09} * H^i_{2006}}{NW^i_{2006}}$, respectively. The latter term is what we call the *housing net worth shock*.

10. Unfortunately, the 2009 SOI data from the IRS are not yet available, so we cannot measure the financial wealth distribution as of 2009. This is why we use the aggregate market indices to project forward financial wealth in a ZIP code.

The assumption that households hold the market index implies that there is no i superscript for the log change in stock and bond prices. However, house prices are measured at the ZIP code level using the Core Logic housing index. We have assumed that debt is fixed in nominal terms for simplicity, and hence it drops out of the change in net worth calculation. This assumption can be a concern because households can default and walk away from their debts. However, our Equifax data on household debt have very accurate information on defaults and write-downs. We show in the Online Appendix that accounting for debt write-downs does not change any of our core results.

Finally, our net worth definition ignores human capital. There may be a concern that this omitted variable is spuriously correlated with the observed change in net worth at the ZIP code level. For example, perhaps areas more dependent on the construction sector suffer a larger human capital shock, which in turn drives both house prices and consumption. We discuss this issue in detail later.

Our net worth procedure results in a population-weighted leverage ratio of 0.21 and a housing wealth to (housing wealth + financial wealth) ratio of 0.27. The same ratios from the Federal Reserve Flow of Funds data are 0.18 and 0.33, respectively (see Online Appendix for details).

III.C. Other Variables

There are a number of other data sources we use in the analysis, all of which are standard in the literature. House price growth is measured using Core Logic data, which are available at the ZIP code level. We measure the employment share of various industries at the county level using the County Business Patterns of the Census. Income at the ZIP code level is available from the IRS SOI. We use a number of other variables from Equifax, including home equity limits, credit card limits, and the fraction of subprime borrowers in an area. All Equifax data are available at the ZIP code level. In the Online Appendix, we offer a table with all of the data sources, the level of aggregation, and contacts for obtaining the data.

III.D. Summary Statistics

We combine all of the data described above into a county-year level data set. Table I presents summary statistics. Given that

TABLE I

SUMMARY STATISTICS

	N	Mean	Std. dev.	10th	90th	Weighted mean	Weighted std. dev.
Housing net worth shock, 2006–9	944	−0.063	0.083	−0.169	0.003	−0.092	0.097
Financial net worth shock, 2006–9	944	−0.096	0.011	−0.108	−0.084	−0.094	0.010
Change in home value, $000, 2006–9	944	−28.4	38.4	−79.1	1.2	−47.5	49.1
Spending growth, 2006–9	944	−0.059	0.135	−0.229	0.110	−0.092	0.113
Change in spending, $000, 2006–9	944	−1.7	4.6	−6.7	3.3	−3.4	4.4
Change in auto spending, $000, 2006–9	944	−2.6	1.6	−4.5	−1.0	−3.3	2.0
Change in other durables spending, $000, 2006–9	944	−0.6	1.3	−2.0	0.5	−1.1	1.1
Change in grocery spending, $000, 2006–9	944	0.5	0.9	−0.2	1.5	0.5	0.7
Change in other non-durable spending, $000, 2006–9	944	1.0	2.8	−1.6	4.0	0.5	2.4
Employment share in construction, 2006	944	0.119	0.054	0.065	0.182	0.125	0.048
Employment share in tradables, 2006	944	0.130	0.102	0.032	0.247	0.110	0.071
Employment share in other, 2006	944	0.522	0.232	0.274	0.830	0.667	0.268
Employment share in nontradables, 2006	944	0.210	0.067	0.137	0.283	0.216	0.051
Income per household, $000, 2006	944	52.2	15.9	38.2	70.2	59.9	18.9
Net worth per household, $000, 2006	944	429.9	246.7	230.5	684.5	520.8	288.8
Housing leverage ratio, 2006	944	0.616	0.229	0.360	0.902	0.608	0.179
Housing supply elasticity, Saiz	540	2.192	1.044	0.943	3.589	1.715	0.968
Number of households, thousands	944	98.2	187.5	12.8	237.8	455.9	666.2
Change in home equity limit, $000, 200–9	944	−0.473	4.786	−3.323	2.857	−0.725	3.637
Change in credit card limit, $000, 2006–9	944	−1.043	2.419	−3.567	1.778	−1.574	1.781
Change in fraction of subprime borrowers, 2006–9	944	−0.010	0.024	−0.038	0.019	−0.004	0.024
Change in refinancings, $000, 2006–9	944	1.236	6.646	−5.247	7.263	−1.268	8.518

Notes. This table presents summary statistics for the counties in our sample. The sample is restricted to 944 counties for which we have data on the value of housing stock. These counties represent 82.1% of total U.S. population in 2006. The housing net worth shock reflects the growth in total net worth due to the growth in housing net worth. The financial net worth shock reflects growth in total net worth due to growth in financial net worth. The housing net worth shock and the financial net worth shock sum up to the growth in total net worth. Other durables include purchases at furniture, home appliance, and home center stores. Other nondurables include purchases at health, gasoline, clothing, hobby and sporting, and department stores. See the text for the corresponding NAICS codes.

some counties in our sample are quite small (only 13,000 households at the 10th percentile), we estimate all specifications with counties weighted by the total number of households in the county. The last two columns show the population-weighted mean and standard deviation.

The housing net worth shock, $\frac{\Delta logp^{H,i}_{06-09} * H^i_{2006}}{NW^i_{2006}}$, represents the shock to total net worth that comes from the decline in house prices. When we weight by population, the average housing net worth shock was almost 10%. Using the Flow of Funds data from the Federal Reserve, the aggregate shock to household wealth from the collapse in home equity was 8%. The average financial net worth shock was similar. Using the weighted average, households on average lost \$48,000 of housing wealth. Spending from 2006 to 2009 fell by 6%, which represents a reduction of about \$1,700 per household. The drop in spending on autos and other durables was largest.

IV. NET WORTH SHOCK

IV.A. *The Cross-Sectional Variation in Net Wealth Changes*

Our key right-hand-side variables are the financial and housing net worth shocks, defined in percentage terms as $\frac{(\Delta logp^{S}_{06-09} * S^i_{2006} + \Delta logp^{B}_{06-09} * B^i_{2006})}{NW^i_{2006}}$ and $\frac{\Delta logp^{H,i}_{06-09} * H^i_{2006}}{NW^i_{2006}}$, respectively. In this section, we explore the cross-sectional variation across the country in these net worth shocks.

The main component of the net worth shocks is movement in asset prices from 2006 to 2009. Figure I shows the movement in prices for housing, stocks, and bonds from 2006 onward. All indexes are set to 100 as of 2006. Stock prices track the S&P 500 index and bond prices track the Vanguard Total Bond Index. House prices for the nation as a whole fell 30% from 2006 to 2009 and stayed low. Stock prices also fell dramatically during 2008 and early 2009, but rebounded strongly afterward. Bond prices experienced a strong rally during the recession as they are inversely related to interest rates, rising by almost 30% during the period.

Table I shows that the (population-weighted) average decline in net worth between 2006 and 2009 is 18.6%, and it is split almost evenly between housing and financial asset losses. More important, most of the cross-sectional variation in net worth

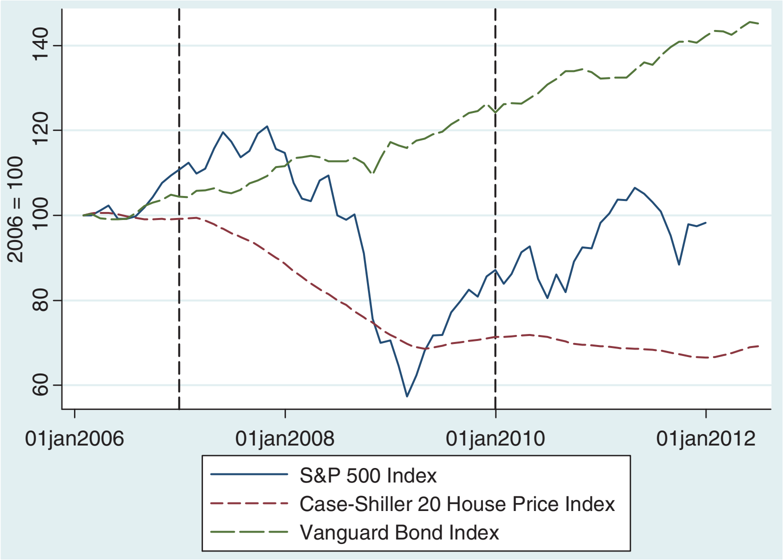

FIGURE I

Wealth Shocks during the Great Recession

This figure plots returns on the S&P 500, the Case-Shiller 20 MSA house price index, and the Vanguard Bond Index. All three indices are scaled to be 100 at the beginning of 2006. The dotted lines represent the end of years 2006 and 2009.

is driven by variation in net worth due to housing. The population-weighted standard deviation of the housing net worth shock is almost 10 times larger than the standard deviation of the financial net worth shock. As we discussed in Section II, the difference in standard deviations is partly driven by the fact that we assume households in different counties hold the same overall market portfolio.[11]

Given little cross-sectional variation in the financial net worth shock, our main focus is on cross-sectional variation in the housing net worth shock. A remarkable feature of the 2006–9 housing collapse is its very large variation across the

11. Case, Quigley, and Shiller (2013) measure financial wealth at the state level using data on mutual fund holdings at the state level, which they use to allocate financial wealth in a similar way. The best data on financial wealth are from Zhou and Carroll (2012), who use ZIP code level data from a private company. Even with this precisely measured data, Zhou and Carroll (2012) find little evidence of an effect of financial wealth shocks on spending.

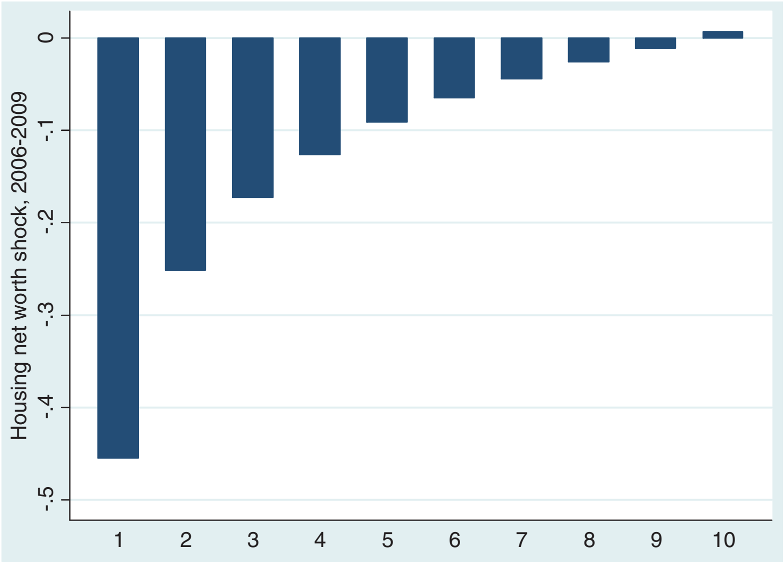

FIGURE II

Housing Net Worth Shock by Decile

This figure sorts ZIP codes into deciles (weighted by population) based on the housing net worth shock and shows each decile's housing net worth shock. The housing net worth shock reflects the growth in total net worth per household due to the growth in housing net worth.

country. Figure II sorts ZIP codes on the housing net worth shock into population-weighted deciles, so that each decile contains 10% of households. Households living in ZIP codes in the top two deciles hardly suffer any loss in their net worth, whereas households in the lowest decile lose almost half of their total net worth from the housing net worth shock. The geographic variation in the housing net worth shock shown in Figure II is what we use to test how consumption responds to changes in wealth.

IV.B. What Is the Source of Variation in the Housing Net Worth Shock?

Given that the housing net worth shock is our main right-hand-side variable, an explanation of its geographical variation is warranted. Mechanically, a ZIP code or county experiences a larger decline in housing net worth if (1) house prices drop more, and (2) homeowners are more levered. A single source of variation that explains both house price declines and leverage accumulated by homeowners is the housing supply elasticity variable introduced by Saiz (2010).

Using GIS maps, Saiz develops an objective index of the ease with which new housing can be expanded in a metropolitan area. The index gives a high elasticity score to a metropolitan area if it has a flat topology without many water bodies, such as lakes and oceans. In contrast, metropolitan areas with a hilly terrain or restricted supply of habitable land are given a low elasticity score.

Mian and Sufi (2009) show that the Saiz measure is a powerful predictor of house price growth between 2002 and 2006. As mortgage credit was extended throughout the country, it was areas with an inelastic supply of housing that experienced the largest house price boom. Mian and Sufi (2011) show that leverage also increased the most in inelastic metropolitan areas as homeowners in these areas borrowed against the rising value of their houses.

When house price dynamics reversed in 2007, the same inelastic areas with high leverage and high house price growth suffered the largest decline in housing net worth. Rows 1 and 2 of Table II regress the change in housing net worth from 2006 to 2009 on housing supply elasticity. More inelastic housing supply counties saw a larger percentage (row 1) and dollar decline (row 2) in net worth coming from the housing collapse.

This discussion highlights why housing supply elasticity is a useful instrument for the housing boom-and-bust cycle. Indeed, Mian and Sufi (2011) show that housing supply elasticity generates variation in house price growth that is largely orthogonal to a number of important variables that one might otherwise view as endogenous to the determination of house price dynamics.

In particular, cities with inelastic housing supply did not experience any differential permanent income shock—as proxied by the change in wage growth—between 2002 and 2006 (row 3 of Table II). More important, cities with differential housing supply elasticity did not have significantly differential exposure to the construction sector (row 4), nor did they experience differential growth in the construction sector (row 5). In fact, despite having higher house price growth, more inelastic cities had slightly slower population growth (row 6).

TABLE II

HOUSING SUPPLY ELASTICITY AS A SOURCE OF VARIATION

		Housing supply elasticity	Constant	N	R^2
(1)	Housing net worth shock, 2006–9	0.046**	−0.174**	540	0.190
		(0.011)	(0.037)		
(2)	Change in home value, $000, 2006–9	27.795**	−95.740**	540	0.284
		(7.874)	(23.210)		
(3)	Change in wage growth (2002–6) − (1998–2002)	−0.002	−0.010	540	0.002
		(0.004)	(0.008)		
(4)	Employment share in construction, 2006	0.002	0.122**	540	0.003
		(0.003)	(0.008)		
(5)	Construction employment growth, 2002–6	0.005	0.940**	540	0.000
		(0.015)	(0.042)		
(6)	Population growth, 2002–6	0.012*	0.018	538	0.026
		(0.005)	(0.012)		
(7)	Income per household, 2006	−5.378**	69.392**	540	0.080
		(0.985)	(2.191)		
(8)	Net worth per household, 2006	−88.389**	674.620**	540	0.083
		(20.689)	(47.965)		

Notes. This table presents coefficients from county-level univariate regressions regressing variables on the housing supply elasticity instrument. Each row is a separate regression. The first two rows represent the first stage estimates of the housing net worth shock and the change in home value, respectively, on housing supply elasticity. Standard errors are heteroskedasticity robust, clustered at the state level. All regressions are weighted by the number of households in the county. **,* Coefficient statistically different than 0 at the 1% and 5% confidence levels, respectively.

The zero correlation between housing supply elasticity and the construction sector is important for the following reason. One may be worried that both the change in housing wealth and change in consumption at the county level are driven by exposure of the county to more "recession-prone" industries in general and the construction sector in particular. The fundamental shock in such a story is the decline in the construction industry, and the fall in housing wealth and consumption simply reflects the decline in construction. However, we can mitigate this concern using the housing supply elasticity as an instrument.

Housing supply elasticity is uncorrelated with the construction sector due to two countervailing forces. Because high supply elasticity makes it easier to build, elasticity and construction tend to be positively correlated. However, low supply elasticity translates housing demand shocks into higher house prices. Higher house prices generate demand for housing investment especially on the intensive margin—that is, an expansion and upgrade of existing housing. Our results show that the net effects of these two forces balance out, making housing supply elasticity a good candidate for an instrument.

Finally, inelastic cities differ from others in having higher income per capita and higher net worth per capita (rows 7 and 8). However, these differences are constant; such fixed differences will be differenced out in our specification. As we pointed out earlier, there is no evidence of a stronger permanent income shock in more inelastic cities during the credit boom years.

IV.C. Using Housing Supply Elasticity as an Instrument and Interpretation of Results

The preceding discussion illustrates how housing supply elasticity serves as an instrument for the boom-and-bust housing cycle. A more inelastic metropolitan area experiences higher leverage and house price growth between 2002 and 2006, and then a more negative housing net worth shock from 2006 and 2009. Our instrumental variables (IV) estimate compares counties that experienced a large boom–bust cycle with counties that largely avoided the boom–bust cycle.

Furthermore, our estimate of the effect of net worth changes on household consumption is *inclusive* of general equilibrium feedback effects that work through the impact of an initial demand shock on the labor market. Mian and Sufi (2012) show that nontradable employment catering to the local economy declines by more in counties that experience a more negative housing net worth shock. The same is *not* true for tradable employment. The initial reduction in local demand due to the decline in wealth is amplified due to the feedback effect on local nontradable employment. Our estimate of the effect of net worth shock on consumption includes both the initial direct effect and the subsequent feedback effect.

V. CONSUMPTION RESPONSE TO THE HOUSING NET WORTH SHOCK

V.A. Elasticity of Consumption with Respect to Net Worth Shock: The Risk-Sharing Hypothesis

As Section II highlighted, representative agent models are built on the premise that household consumption is protected against unanticipated shocks such as those shown in Figure II.

FIGURE III

Elasticity of Spending with Respect to Housing Net Worth Shock

The scatterplot relates total spending growth in a county from 2006 to 2009 to the housing net worth shock over the same time period. The housing net worth shock reflects the growth in total net worth per household due to the growth in housing net worth. The scatterplot and regression line are weighted by the number of households in the county.

We evaluate the consumption risk-sharing hypothesis by estimating equation (1) and testing if β is zero. Figure III graphically illustrates the test by plotting the growth in spending in a given county against the housing net worth shock from 2006 to 2009. The plotted line represents the fitted values of the linear regression of consumption growth on the housing net worth shock, which corresponds to the specification reported in column (1) of Table III.

The consumption risk-sharing hypothesis is rejected. The elasticity of consumption with respect to the housing net worth shock is 0.63, and the coefficient is precisely estimated. In fact, the housing net worth shock variable explains 30% of the overall variation in spending growth across counties.

Column (2) adds the financial net worth shock. The coefficient on the housing net worth shock does not change, whereas

TABLE III

NET WORTH SHOCK AND CONSUMPTION GROWTH, 2006–9

	(1)	(2)	(3)	(4) IV	(5) State FE	(6) Excluding AZ, CA, FL, NV
Housing net worth shock, 2006–9	0.634**	0.613**	0.590**	0.774**	0.457**	0.869**
	(0.125)	(0.122)	(0.130)	(0.239)	(0.101)	(0.148)
Financial net worth shock, 2006–9		−0.595				
		(1.032)				
Construction employment share, 2006			−0.448**	−0.287	−0.171	−0.288
			(0.150)	(0.216)	(0.127)	(0.160)
Tradable employment share, 2006			0.051	0.011	0.042	−0.027
			(0.067)	(0.092)	(0.066)	(0.065)
Other employment share, 2006			−0.025	−0.045	−0.057	−0.058
			(0.038)	(0.050)	(0.037)	(0.039)
Nontradable employment share, 2006			0.193	0.095	0.228	0.106
			(0.157)	(0.167)	(0.137)	(0.158)
Ln(income per household, 2006)			−0.002	0.024	−0.006	0.028
			(0.033)	(0.047)	(0.046)	(0.045)
Ln(net worth per household, 2006)			−0.028	−0.035	−0.023	−0.034
			(0.018)	(0.023)	(0.020)	(0.025)
Constant	−0.034*	−0.092	0.167*	0.147	0.120	0.132
	(0.015)	(0.099)	(0.077)	(0.092)	(0.090)	(0.087)
N	944	944	944	540	944	833
R^2	0.298	0.301	0.355	0.319	0.547	0.230

Notes. Dependent variable: total spending growth, 2006–9 (%). This table presents coefficients from regressions relating spending growth to the housing net worth shock. The unit of observation is a county. The housing net worth shock reflects the growth in total net worth due to the growth in housing net worth. The financial net worth shock reflects growth in total net worth due to growth in financial net worth. The housing net worth shock and the financial net worth shock sum up to the growth in total net worth. Standard errors are heteroskedasticity robust, clustered at the state level. All regressions are weighted by the number of households in the county. **,* Coefficient statistically different than 0 at the 1% and 5% confidence levels, respectively.

the coefficient on the financial net worth shock is −0.595. The standard error on the latter coefficient is large because we do not have the statistical power to estimate the effect of shocks to financial wealth on spending. This is not surprising given the much smaller cross-sectional variation in the net wealth change due to financial assets variable and the fact that we do not have good data on direct holdings of financial assets at the household level.[12]

Column (3) adds a number of additional controls relating to industry specialization of a county and income. In particular, we want to address the concern that omitted industry-specific shocks might be driving both the cross-county variation in housing net worth shock and spending. Of particular concern is the construction sector. It is possible that counties that see a larger fall in housing net worth also employ more construction workers. Since the construction industry is naturally the more affected one, the effect on spending might be spuriously driven by the higher likelihood of construction workers losing their jobs.

However, column (3) shows that this is not the case by explicitly controlling for the share of employment in the construction sector. In fact, we can control for exposure to other industries as well—such as tradable and nontradable sector as defined in Mian and Sufi (2012).[13] We also control for other covariates including income per household and total net worth per household as of 2006. Despite the addition of these controls, the coefficient on net wealth shock does not change significantly.

A more direct way to limit the possibility of spurious channels driving our coefficient of interest is to instrument the housing net worth shock with housing supply elasticity. We do so in column (4), and the coefficient increases slightly.[14] Because

12. The work of Zhou and Carroll (2012) is reassuring. They have much better data on financial wealth at the state level and find almost no effect of changes in financial wealth on spending. Moreover, inclusion of financial wealth in Zhou and Carroll (2012) does not change the estimated effect of housing wealth on spending. Case, Quigley, and Shiller (2013) also find no effect of financial wealth, but are subject to a similar measurement error problem.

13. We have information on the share of each four-digit industry at the county level, allowing us to control for differences in industry structure at a much finer level than that reported in column (3). We cannot find any evidence that the cross-county spending growth patterns are spuriously driven by differential exposure to a specific set of industries.

14. The increase in coefficient is not driven by the smaller number of observations that have information on housing supply elasticity.

the instrument is uncorrelated with the level and growth in construction employment in a county, the IV is further confirmation that our coefficient is not driven by changes in the construction sector.

Column (5) puts in state fixed effects, using only within-state variation to estimate coefficients. The coefficient on the housing net worth shock goes down to 0.46. However, as we show later, there is no such attenuation in the coefficient when we estimate marginal propensities to consume instead of elasticities. Column (6) excludes the four states with the largest housing boom and bust. The elasticity coefficient is higher with these states excluded from the regression.

The results in Table III soundly reject the complete risk-sharing hypothesis. The estimated β in equation (3) is far different from 0 and the magnitude is large. Figure II showed that the housing net worth shock moves from -45% for the lowest decile of ZIP codes to 0% for the top decile. Using these values, the estimate in column (1) of Table III implies an additional fall in consumer spending of 30% for the bottom decile relative to the top decile.

V.B. Marginal Propensity to Consume

Given the failure of the full risk-sharing hypothesis, we test for concavity of the consumption function as implied by consumer theory under uncertainty and limited insurance. Doing so requires estimating the average MPC and then testing for heterogeneity in MPC.

The average MPC can be estimated by regressing the dollar change in total spending per capita on the dollar change in housing net worth. The left panel of Figure IV plots the county-level change in spending per household from 2006 to 2009 on the county-level change in home value per household over the same period. Given our goal of estimating an MPC, we keep units in terms of thousands of dollars. There is a strong positive relation between the change in home value and the change in spending. At the extreme, a county where households are experiencing a decline in home value of $150,000 sees a reduction in spending per household of almost $10,000. There is also some evidence of a nonlinear effect as the relationship is steeper for smaller declines in home value versus larger ones.

Table IV presents coefficients from regressions corresponding to the left panel of Figure IV. The estimated average MPC

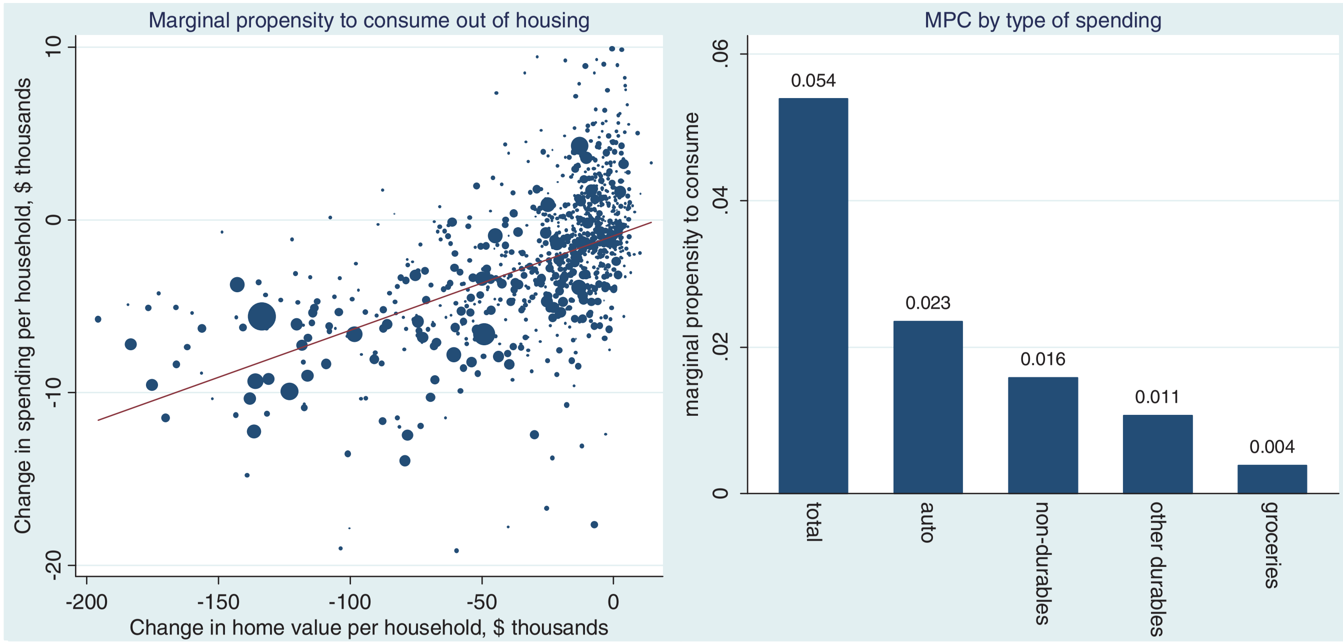

FIGURE IV

The Average Marginal Propensity to Consume

The left panel scatterplot relates the change in total spending per household in a county from 2006 to 2009 to the change in home values over the same time period. The scatterplot and regression line are weighted by the number of households in the county. The gradient of the line represents the average marginal propensity to consume. The right panel plots the marginal propensity to consume for various spending categories.

TABLE IV

AVERAGE MARGINAL PROPENSITY TO CONSUME OUT OF HOUSING WEALTH

	(1)	(2)	(3)	(4) IV	(5) State FE	(6) Excluding AZ, CA, FL, NV
Change in home value, \$000, 2006–9	0.054**	0.119**	0.051**	0.072**	0.051**	0.094**
	(0.009)	(0.015)	(0.011)	(0.021)	(0.013)	(0.017)
(Change in home value, \$, 2006–9)^2		0.432**				
		(0.076)				
Construction employment share, 2006			−9.748	−2.915	−7.449	−2.305
			(5.479)	(7.800)	(5.379)	(5.818)
Tradable employment share, 2006			2.034	0.438	1.516	−0.795
			(2.235)	(3.783)	(2.190)	(2.496)
Other employment share, 2006			−1.568	−3.037	−2.186	−2.629
			(1.459)	(1.850)	(1.418)	(1.466)
Nontradable employment share, 2006			−1.797	−3.256	−3.341	−4.106
			(5.438)	(5.983)	(5.048)	(5.349)
Income per household, \$000, 2006			−0.056*	−0.019	−0.043	−0.022
			(0.023)	(0.032)	(0.030)	(0.029)
Net worth per household, \$000, 2006			0.003*	0.002	0.002	0.002
			(0.001)	(0.001)	(0.002)	(0.001)
Constant	−0.830	0.263	3.311**	3.211**	3.396**	3.415**
	(0.536)	(0.554)	(0.678)	(0.928)	(0.861)	(0.837)
N	944	944	944	540	944	833
R^2	0.362	0.423	0.421	0.347	0.573	0.336

Notes. Dependent variable: change in spending 2006–9 (\$000). This table presents coefficients from regressions relating the change in household spending to the change in home value between 2006 and 2009. Both the change variables are in thousands of dollars. All regressions are at the county level. Standard errors are heteroskedasticity robust, clustered at the state level. All regressions are weighted by the number of households in the county. **,* Coefficient statistically different than 0 at the 1% and 5% confidence levels, respectively.

in column (1) is 5.4 cents per dollar. Column (2) confirms the nonlinearity of the effect. The positive coefficient on the squared term implies that the MPC is larger for small declines in home value, but gets smaller as the decline in home value gets larger. For smaller declines in home values, the MPC is quite large, above 10 cents per dollar.[15]

Column (3) includes control variables with little change in the MPC estimate. Column (4) presents the IV estimate, which is larger than the ordinary least squares (OLS) estimate. The IV estimate suggests an MPC of 7.2 cents per dollar of home value change. In column (5), we include state fixed effects, which do not affect the results. Finally, in column (6), we exclude the four largest boom and bust states. The MPC increases substantially to 9.4 cents per dollar. This reflects the nonlinearity already shown in column (2). The four excluded states have many counties with the largest declines in home values in the country. Excluding them isolates the sample to the part of the home value change distribution where the MPC is largest.

In the right panel of Figure IV, we split out the MPC by the four categories of spending we can measure. Each bar in the panel represents the coefficient on the change in home value from a regression identical to the one reported in column (1) of Table IV. All of the estimated MPCs are statistically distinct from zero at the 1% level. As the panel shows, the MPC is larger for autos and durables than for groceries. The higher MPC for durables is consistent with a larger elasticity of demand for these products with respect to income or wealth. It is also consistent with the importance of credit constraints, given the importance of financing availability when purchasing durable goods.

Is our estimate of the MPC large? Most of the extant literature puts the long run MPC out of housing wealth in the range of 5–10 cents per dollar, and our estimate fits within this range. However, our estimate is a contemporaneous effect, which has typically been estimated to be smaller (Carroll 2004). We are unaware of any other study that estimates an MPC out of housing wealth during the Great Recession.[16] A recent update of Case,

15. The nonlinearity could be driven by the fact that losses beyond a certain point do not matter for the homeowner since he has the option to "walk away," and also the option to declare bankruptcy.

16. Dynan (2012) examines whether household debt is holding back the recovery, and Melzer (2012) argues that debt overhang is an important friction holding down spending, but neither estimate an MPC out of housing wealth.

Quigley, and Shiller (2013) examines data through 2012, but does not provide estimates in terms of an MPC. Zhou and Carroll (2012) examine the correlation between housing wealth and consumption in the Great Recession using an estimate of the MPC from a period before the downturn, but do not provide an estimate of the MPC based on the 2006–9 period.

Another way of stating the magnitude is to examine aggregate data. Our estimate for the MPC varies between 0.054 for the OLS estimate to 0.072 for the IV estimate. Let us pick 0.06 within this range for convenience. What does this estimate imply about the aggregate spending effect of the collapse in home values? Total household net worth (i.e., assets minus liabilities) in the Flow of Funds data for 2006 was $64.7 trillion. The drop in value of housing between 2006 and 2009 is equal to $5.6 trillion, or 8.7% of total net worth.

An MPC of 0.06 implies that the drop in consumption driven by a $5.6 trillion loss in home value is equal to $336 billion. The average nominal spending growth between 1992 and 2006 was 5.2%. Using this trend growth for nominal spending between 2006 and 2009, we estimate a total nominal decline in spending of $870 billion from 2006 and 2009 relative to the linear preperiod trend. The total drop due to the housing net worth shock implied by our MPC is almost 40% ($336 billion/$870 billion) of the spending decline relative to trend. An important caveat is that this aggregate calculation does not take into account any “level shifts” in aggregate consumption driven by possible general equilibrium forces between 2006 and 2009.

VI. HETEROGENEITY IN MPCs

VI.A. Heterogeneity across Wealth Distribution

The most important question of this study is to test whether the estimated MPC differs by household wealth and leverage. We do so by estimating equation (2), which interacts the MPC coefficient already estimated with the level of initial wealth. We use two variables for net worth: net worth per household in 2006 and income per household in 2006 (both in millions of dollars to make coefficients easily readable).

The first four columns of Table V report estimates from the interaction specification using county-level data. Columns (1) and (2) focus on total spending, whereas columns (3) and (4) focus on

TABLE V

HETEROGENEITY IN THE MPC BY WEALTH AND INCOME

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: ΔTotal Spending ($000), 2006–9		*Dependent variable: ΔAuto Spending ($000) 2006–9*		*Dependent variable: ΔAuto Spending ($000) 2006–9*		
	County-level analysis		County-level analysis		ZIP code–level analysis		
Δ Home value, \$000, 2006–9	0.076**	0.065**	0.034**	0.047**	0.018**	0.023**	0.025**
	(0.012)	(0.015)	(0.005)	(0.005)	(0.001)	(0.002)	(0.002)
Net worth, \$millions, 2006	−4.289*		−1.81**			−0.354	
	(2.132)		(0.665)			(0.243)	
(Δ Home value)*(Net worth, 2006)	−0.038		−0.024*			−0.007**	
	(0.024)		(0.009)			(0.001)	
Income per household, \$ millions, 2006		−64.042*		−31.814**			−4.020
		(28.158)		(7.819)			(3.136)
(Δ Home value)*(Income per household, 2006)		−0.180		−0.432**			−0.095**
		(0.332)		(0.100)			(0.022)
Constant	1.247	2.829*	−1.30**	−0.361	−2.075**	−1.883**	−1.809**
	(0.679)	(1.212)	(0.20)	(0.332)	(0.170)	(0.121)	(0.117)
N	944	944	944	944	6,263	6,220	6,263
R^2	0.462	0.478	0.427	0.440	0.153	0.161	0.163

Notes. This table presents coefficients from regressions relating the change in household spending to the change in home value between 2006 and 2009. Regressions in columns (1) through (4) are at the county level, and regressions in columns (5) through (7) are at the ZIP code level. The dependent variables is the change in total spending in columns (1) and (2), and the change in spending on autos in columns (3) through (7). Throughout, Δ signifies change in thousands of dollars. Standard errors are heteroskedasticity robust, clustered at the state level. All regressions are weighted by the number of households. **,* Coefficient statistically different than 0 at the 1% and 5% confidence levels, respectively.

auto spending. The interaction term coefficients are negative across all four specifications, which implies that richer ZIP codes have a lower spending sensitivity to the same decline in home value. But the estimates are imprecise. In particular, for total spending, we cannot reject the null hypothesis that the coefficient estimates on the interaction terms are 0.

Why are the estimates statistically weak? To estimate how the MPC varies across the net worth distribution, the data must have significant variation in the level of net worth across counties. In the extreme, if there were no variation in net worth across counties as of 2006, we would be unable to estimate the interaction effect.

The problem with county-level analysis is that there is relatively limited variation in the level of net worth per household across counties. However, there is significantly more variation in net worth per household at the ZIP code level as households often sort by income and wealth across neighborhoods. For example, the ZIP code level within-county standard deviation in net worth is almost twice as large as the between-county standard deviation ($440,000 versus $237,000).[17] Wealth inequality is much more a within-county phenomenon as opposed to an across-county phenomenon. As a result, ZIP code level analysis provides much stronger statistical power for estimating MPC heterogeneity.

To test heterogeneity in MPCs, we limit ourselves to auto expenditure because it is the only spending variable available at the ZIP code level. Although automobile expenditure is only one component of overall spending, it constitutes a large share of the change in spending during the Great Recession. For example, we saw in Figure IV that the MPC was the highest for automobile expenditure. Similarly, out of the $870 billion in lost spending in 2009 relative to trend, auto sales accounted for $380 billion.

Column (5) estimates an average MPC of 1.8 cents per dollar for auto spending at the ZIP code level. Columns (6) and (7) test how the MPC for auto expenditure varies by net worth and household adjusted gross income. The results show that wealthier and richer households have a significantly smaller MPC out of housing wealth. Comparing the standard errors in columns (6)

17. In the 2000 Decennial Census, there are approximately 31,000 ZIP codes and 3,136 counties. The average (median) number of households in a ZIP code is 3,646 (1,226). The average (median) number of households in a county is 36,946 (11,004).

and (7) with columns (3) and (4) illustrates the major advantage of ZIP code–level data. The standard errors on the interaction term are five to nine times bigger in county-level specifications relative to the ZIP-level ones.

The magnitude of the difference in the MPC between rich and poor households can be understood more clearly through Figure V. The figure is based on separately estimating the MPC for various income categories. We find that the MPC for households in ZIP codes with an average adjusted gross income (AGI) less than \$35,000 is almost three times as large as that for households in ZIP codes with an average AGI greater than \$200,000. For the same dollar decline in home value, households in poorer ZIP codes cut spending by significantly more.

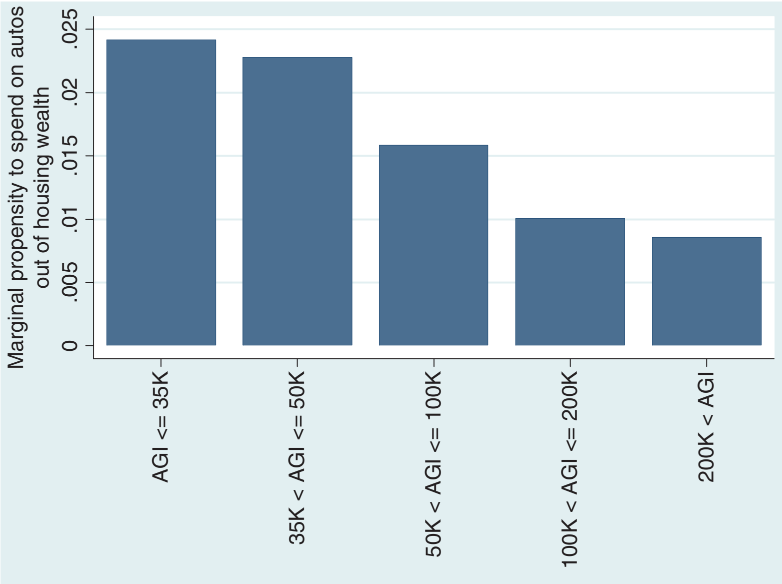

FIGURE V

Marginal Propensity to Consume out of Housing across 2006 Income Distribution

The figure plots the estimated marginal propensity to spend on autos based on 2006 per household income in a ZIP code. AGI is adjusted gross income. The MPC is estimated using ZIP code–level data and regressing the change in spending on automobile purchases between 2006 and 2009 on the change in

VI.B. The Role of Debt

A second rationale for heterogeneity in MPC comes from models that emphasize the importance of credit constraints. If credit constraints matter, then households with limited borrowing capacity may respond more aggressively to changes in housing value than unconstrained households.

We test this idea using variation across ZIP codes in the housing leverage ratio, which we define to be a ZIP code's ratio of mortgage plus home equity debt to home values as of 2006. It is equivalently the loan-to-value (LTV) ratio of owner-occupied houses in a ZIP code. The median housing leverage ratio across ZIP codes is 0.54, with substantial cross-sectional variation. The 90th percentile has a leverage ratio of 0.90, while the ratio is only 0.36 at the 10th percentile. We use the leverage ratio specific to housing as a proxy for credit constraints given the evidence in Mian and Sufi (2011) that housing collateral is often used for borrowing.

Table VI tests whether the MPC varies across ZIP codes based on the housing leverage ratio. Given results in Table V that show the MPC differs by income, we want to make sure that any differences in the MPC by the housing leverage ratio are not driven by some underlying correlation between leverage and income. Columns (1) and (2) of Table VI report regressions relating the housing leverage ratio to net worth and household income, respectively. The results show that the housing leverage ratio in a ZIP code is orthogonal to both income and net worth. The lack of correlation between the housing leverage ratio and measures of wealth allows us to separately estimate whether MPC differs by leverage.[18]

Column (3) tests whether households in ZIP codes with a higher housing leverage ratio have a higher MPC out of housing wealth on autos. There is a strong and significant effect. Columns (4) and (5) include the level and interaction terms based on net worth and income, respectively. The MPC is higher for households with a higher housing leverage ratio, as well as for poorer households. Both high housing leverage and low net worth amplify the effect of the housing decline on spending, and these effects are independent of each other.

18. The lack of correlation between the housing leverage ratio and net worth per capita could easily be driven by the fact that poor households face higher costs of mortgage debt finance.

TABLE VI

HETEROGENEITY IN THE MPC: THE ROLE OF HOUSING DEBT

	(1)	(2)	(3)	(4)	(5)
	Dependent variable: Housing leverage ratio, 2006		*Dependent variable: ΔAuto spending ($000), 2006–9*		
Δ Home value, $000, 2006–9			0.006**	0.010**	0.011**
			(0.002)	(0.002)	(0.002)
Housing leverage ratio, 2006			−2.112**	−2.146**	−2.191**
			(0.228)	(0.232)	(0.230)
(Δ Home value)*(Housing leverage ratio, 2006)			0.021**	0.020**	0.020**
			(0.003)	(0.004)	(0.003)
Net worth, $millions, 2006	0.004			−0.153	
	(0.013)			(0.158)	
(Δ Home value)*(Net worth, 2006)				−0.005**	
				(0.001)	
Income per household, $ millions, 2006		0.327			0.022
		(0.233)			(1.627)
(Δ Home value)*(Income per household, 2006)					−0.059**
					(0.015)
Constant	0.595**	0.576**	−0.786**	−0.667**	−0.705**
	(0.011)	(0.016)	(0.150)	(0.150)	(0.157)
N	6,385	6,448	6,222	6,182	6,222
R^2	0.000	0.003	0.272	0.272	0.279

Notes. This table presents coefficients from regressions relating the change in household spending to the change in home value between 2006 and 2009. All regressions are at the ZIP code level. The housing leverage ratio is defined to be the ratio of mortgage and home equity debt to home value in a ZIP code as of 2006. Throughout, Δ signifies change in thousands of dollars. Standard errors are heteroskedasticity robust, clustered at the state level. All regressions are weighted by the number of households in a ZIP code. **,* Coefficient statistically different than 0 at the 1% and 5% confidence levels, respectively.

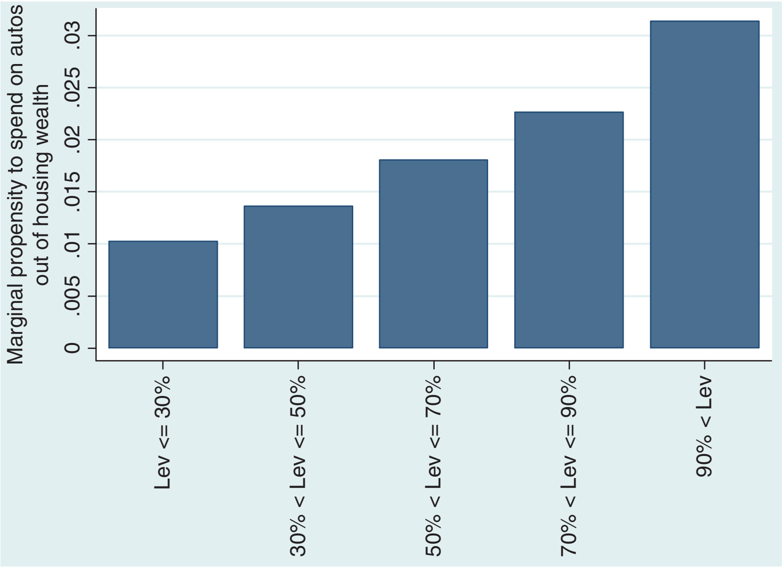

FIGURE VI

Marginal Propensity to Consume out of Housing across Housing Leverage Ratio Distribution

The figure plots the estimated marginal propensity to spend on autos based on the 2006 housing leverage ratio of a ZIP code. The housing leverage ratio is defined to be total mortgage and home equity debt scaled by the total value of owner-occupied homes in a ZIP code. The MPC is estimated using ZIP code–level data and regressing the change in spending on automobile purchases between 2006 and 2009 on the change in home values over the same period. Each regression is run separately for ZIP codes in a given housing leverage ratio category and the resulting MPC coefficient is plotted.

The magnitude of the heterogeneity in MPC by leverage is seen in Figure VI. It estimates the MPC separately for various household leverage categories. ZIP codes with a housing leverage ratio below 30% cut spending on autos by \$0.01 for every \$1 decline in home value. However, the same effect is three times as large for ZIP codes with a housing leverage ratio of 90% or higher. The fact that levered ZIP codes cut back more on spending for the same dollar decline in home value is the essence of Fisher's (1933) "debt deflation" argument.[19]

19. Disney, Gatherhood, and Henley (2010) provide evidence from the United Kingdom that spending by underwater homeowners has a higher sensitivity to wealth shocks.

VI.C. Why Do Levered Households Have Higher MPCs?

The evidence shows that spending responds aggressively to a reduction in household net worth and that the response is much stronger for poorer households and households with higher housing leverage. As discussed earlier, the MPC response to a reduction in housing net worth may be driven by a pure wealth effect and/or tighter credit constraints given the role of housing collateral for borrowing.

Table VII provides some direct evidence on the role of credit constraints in driving the MPC response. In particular, we answer two questions: (1) do credit constraints become tighter when home values decline?, and (2) for a given dollar decline in home values, do credit constraints bind more for poorer and more levered households?

Our data set allows the construction of four different measures of the change in credit conditions experienced by households in a ZIP code between 2006 and 2009: the change in home equity limit, the change in credit card limit, the change in refinancing volume, and the change in percentage of population with a credit score below 660. In columns (1), (4), (7), and (10), we regress these four measures on the change in home value between 2006 and 2009.

We find that a decline in home value leads to tighter credit constraints. A lower home value leads to reduced home equity and credit card limits, a decline in refinancing volume, and an increase in the fraction of subprime borrowers in the ZIP code. The refinancing result is particularly interesting because mortgage interest rates plummeted from 2006 to 2009. ZIP codes with a sharp decline in home values were unable to take advantage of these lower rates (e.g., Boyce et al. 2012).[20]

In columns (2), (8), and (11), we find that the marginal effect of a decline in home value on tighter credit constraints is significantly larger for ZIP codes that have a high housing leverage ratio. In other words, constraints bind more for a given decline in home value when households in a ZIP code have little collateral left in their homes. In terms of magnitude, a ZIP code at the

20. We can also instrument the change in home value with housing supply elasticity. The results are similar in the IV specification, except for the result with change in home equity limit as the dependent variable. The coefficient in this specification is positive and insignificant, but not statistically different than our column (1) estimate.

TABLE VII

WHY DO LEVERED ZIP CODES HAVE A HIGHER MPC OUT OF HOUSING WEALTH?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent variable: Δ Home equity limit, $000, 2006–9			*Dependent variable: Δ Credit card limit, $000, 2006–9*			*Dependent Variable: Δ Refinancing volume, $000, 2006–9*			*Dependent variable: Δ Fraction subprime borrowers, 2006–9*		
Δ Home value, $000, 2006–9	0.023**	−0.010*	0.040**	0.010**	0.009*	0.015**	0.119**	0.012	0.179**	−0.033**	−0.019**	−0.046**
	(0.002)	(0.004)	(0.006)	(0.001)	(0.004)	(0.003)	(0.007)	(0.015)	(0.019)	(0.002)	(0.003)	(0.002)
Housing leverage ratio, 2006		−1.234**			0.850*			13.378**			0.172	
		(0.429)			(0.390)			(0.857)			(0.267)	
(Δ Home value)*(Housing leverage ratio, 2006)		0.058**			0.002			0.179**			−0.024**	
		(0.007)			(0.007)			(0.019)			(0.004)	
Income per household, $ millions, 2006			17.295			−3.516			109.261**			0.543
			(11.630)			(2.224)			(40.775)			(1.677)
(Δ Home value)*(Income per household, 2006)			−0.187*			−0.070*			−0.536			0.165**
			(0.093)			(0.030)			(0.307)			(0.018)
Constant	0.216	0.972**	−0.603	−1.080**	−1.606**	−0.856**	4.077**	−4.058**	−1.306	−1.623**	−1.733**	−1.751**
	(0.114)	(0.273)	(0.610)	(0.094)	(0.224)	(0.134)	(0.318)	(0.620)	(2.098)	(0.083)	(0.202)	(0.129)
N	6,273	6,236	6,273	6,262	6,228	6,262	6,212	6,191	6,212	6,262	6,215	6,262
R^2	0.051	0.098	0.090	0.021	0.023	0.023	0.311	0.361	0.532	0.293	0.311	0.322

Notes. This table presents coefficients from regressions relating borrowing constraints to the change in home value between 2006 and 2009. All regressions are at the ZIP code level. The housing leverage ratio is defined to be the ratio of mortgage and home equity debt to home value in a ZIP code as of 2006. Throughout, Δ signifies change in thousands of dollars. Standard errors are heteroskedasticity robust, clustered at the state level. All regressions are weighted by the number of households in the ZIP code. **,* Coefficient statistically different than 0 at the 1% and 5% confidence levels, respectively.

10th percentile of the housing leverage ratio distribution (36% leverage) saw a decline in refinancing of (0.012 + 0.36*0.179 =) \$0.08 for every \$1 decline in home value. A ZIP code at the 90th percentile (90%) saw decline in refinancing of (0.012 + 0.9*0.179 =) \$0.17 for every \$1 decline in home value.

Column (5) shows that the cut in credit card limits is no larger for more levered ZIP codes for a given decline in home values. A decline in home values in a ZIP code leads to lower credit card limits, but not differentially so if the ZIP code has a higher housing LTV ratio. This result suggests that lending tied to housing collateral is the key channel through which the housing leverage ratio matters.

Finally, columns (3), (6), (9), and (12) show that the marginal effect of a decline in home value on tighter credit constraints is significantly larger for ZIP codes with lower income households. In other words, constraints bind more for a given decline in home value for poorer households. Given that income and leverage are orthogonal at the ZIP code level, the heterogeneity with respect to income is independent of the heterogeneity with respect to household leverage.[21]

VII. CONCLUSION

The Great Recession was characterized by a collapse in household spending. This article analyzes the role of the housing wealth shock in precipitating the collapse in consumption. An advantage of our empirical exercise is the availability of micro-level data on consumption combined with a natural experiment that generates large cross-sectional dispersion in housing net worth shocks across the country.

We find a large effect of housing net worth shocks on consumption, with a reduction in spending of 5–7 cents for every \$1 of housing wealth loss. Our most interesting finding is that the MPC differs significantly across ZIP codes by both income and leverage, and these two effects are independent of one another. These results suggest that the aggregate impact of wealth shocks depends not only on the total wealth lost but also on how these losses are distributed across the population.

21. The results are similar when we interact change in house value with net worth in 2006 instead of income. We do not report these results for brevity.

There is large-scale systematic evidence that a high level of private debt is associated with deeper and more prolonged recessions (Jordà, Schularick, and Taylor 2011). These recessions are also characterized by a deep collapse in consumption (IMF 2012) that can in turn throw an economy into a liquidity trap (Eggertsson and Krugman 2012). The most recent recession in the United States and Europe followed the same patterns (Glick and Lansing 2009).

Our article provides direct evidence on how leverage in combination with asset price shocks can translate into demand-driven recessions. Leverage not only amplifies asset price shocks for well-known reasons but has strong consequences for how the loss in wealth is distributed. The impact on consumption of the level and distribution of wealth losses has important consequences for the effectiveness and appropriate design of monetary and fiscal policy as well as our financial system overall. We look forward to research that tackles these issues.

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Supplementary Material

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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