# Financial Literacy and Mortgage Equity Withdrawals

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***Abstract***

The recent U.S. consumption boom and the subsequent surge in mortgage defaults have been linked to mortgage equity withdrawals (MEWs). MEWs are correlated with covariates consistent with a permanent income framework augmented for credit-constraints. Nevertheless, many households are financially illiterate. We assess the unexplored linkages between "active MEW" and measures of financial literacy using panel data from the Health and Retirement Study (HRS). Findings indicate that declines in mortgage interest rates encouraged MEWs. Nevertheless, financially illiterate households were significantly more likely to withdraw housing equity via traditional first or second mortgages (including cash-out mortgage refinancings but not home equity loans). We find that the financially less savvy are 3-5 percentage points more likely to engage in this type of MEW relative to those who answered financial literacy questions correctly. Also significant were state differences in debtor versus creditor interests in bankruptcy, with loan demand effects outweighing loan supply effects across states.

JEL Codes: E21, E32, E44, E51
Key Words: mortgage equity withdrawals, financial literacy, consumption, credit constraints

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Mortgage equity withdrawals (MEWs) have been linked to the UK consumption boom of the late 1980s (Miles, 1992, and Muellbauer and Murphy, 1997) and the U.S. consumption boom of the early 2000s (Aron et al., 2011, Greenspan and Kennedy, 2008, and Hurst and Stafford, 2004). At the macro-level, MEW has been linked to an increased sensitivity of consumption to housing wealth (Duca, 2006, and Carroll, Otsuka, and Slacelek, 2011). At the micro level, MEWs are correlated with liquidity constraints (Benito, 2009, and Browning et al., 2008, Hurst and Stafford, 2004), consistent with permanent-income models incorporating credit constraints, which imply that housing wealth influences consumption by providing collateral for loans to otherwise credit-constrained families (Englehardt, 1996, and Muellbauer and Lattimore, 1995).

However, the recent mortgage bust suggests that some households were not aware of the risks they took, consistent with evidence that many are not financially literate and that some withdrew housing equity via refinancing even when their mortgage rates rose. Using data on a subset of middle and older age households in the Health and Retirement Study (HRS), Lusardi and Mitchell (2007) document that many families incorrectly answered questions about compound interest, money illusion, and portfolio diversification. Furthermore, incorrect answers have been linked to sub-optimal saving for retirements (Lusardi and Mitchell, 2007) and over-borrowing (Lusardi and Tufano, 2009). Van Rooij, Lusardi and Alessi (2011) find that financial sophistication had a significant impact on stock market participation. Bernheim, Garrett and Maki (2000) found evidence of positive role of high school financial education mandates in enhancing financial knowledge and that financial literacy has an important effect on subsequent wealth accumulation. In another paper, Bernheim and Garrett (2003) found that employer-based financial education programs are effective in raising saving. In addition, there is also evidence that many home-owners do not choose the lowest cost home purchase mortgage because they may be confused by terms in the mortgage contract (Woodward and Hall, 2010). Evidence indicates that mistaken beliefs had a role in the consumption boom of the early- to mid-2000s. In particular, Agarwal (2007) found that households who overestimated the market values of their homes had higher consumption and lower savings than those who did not.

Nevertheless, the literature has not examined the links between financial literacy and MEW behavior which has implications for whether financial illiteracy may have helped fuel debt and consumption growth before the recent housing bust. This study addresses this gap by examining whether answers to financial literacy questions are linked to which homeowners withdraw housing equity. To control for non-literacy influences on MEW, we include a comprehensive set of households' economic and demographic characteristics, individual gains from refinancing, home price appreciation and aspects of state bankruptcy laws. Previous research has highlighted significant differences between the effects of variables related to cognitive ability, numerical ability and other measures of financial literacy (Cole and Shastry, 2009). To test for differential impact of various measures of financial literacy, we include three different measures of financial literacy in our estimated model of MEW—compound interest, money illusion, and portfolio diversification–and find that knowledge of the benefits of portfolio diversification has the most significant impact on the propensity to engage in MEW.

Our results indicate that the financially literate are significantly less likely to withdraw housing equity via increasing mortgage debt, although, we find no significant differences in MEW through tapping home equity lines of credit (HELOC). Results from our richest specification suggest that the financially literate are 5 percentage points less likely to withdraw equity from their homes. Consistent with the limited literature on MEWs, we also find that the propensity for withdrawing housing equity rises with house price appreciation and incentives to lower mortgage interest rates. In line with Lefgren and McIntyre's (2009) findings that state variation in legal codes affects bankruptcy rates, we also find that differences in debtor legal conditions across states are correlated with MEW behavior, suggesting that legal differences affect the cross-regional supply of and demand for consumer versus real-estate-secured debt. Moreover, in the presence of a variable controlling for legal differences across states, stronger effects of cross-state and cross-time differences in house price appreciation emerge.

We address the fact that financial literacy could be endogenous as it may be correlated with individual specific risk preferences which also affect the propensity to withdraw mortgage equity. We use HRS survey-based measures of risk aversion to account for systematic differences in risk preferences which may bias our estimates of the impact of financial literacy on mortgage equity withdrawal. The estimated impact of financial literacy on MEW propensity from our baseline model increases from -3 percentage points to -5 percentage points—when we control for risk aversion, suggesting that the more financially savvy are also less risk averse. This is consistent with recent findings that cognitive ability is inversely related to risk aversion (Dohmen et al., 2010). Our findings are robust to including year and state fixed effects and to the use of nonparametric matching methods to estimate average treatment effects.

Our results have implications for the effectiveness of financial education programs that could help households make better financial decisions. There is evidence that MEW is correlated with mortgage delinquencies as the housing bubble unraveled. Sufi and Mian (2010) find that the surge in MEW can account for as much 40 percent of new mortgage defaults between 2006 and 2008. Even in cases not resulting in default or delinquency, higher borrowing by the ill-informed can lower future consumption during a housing bust via lowering their net liquid assets (see Aron et al., 2011). Such over-borrowing also increases the probability that a negative house price shock could push a borrower into a negative net housing equity position, which, in turn, reduces its labor market mobility (see Ferreira et al., 2010). Thus a negative relationship between financial literacy and MEW propensity suggests that financial education programs might lower mortgage default rates and other negative consequences of high borrowing.

To present these findings, this study is organized as follows. Section II lays out the basic empirical specification which is based on theoretical factors affecting the propensity to withdraw housing equity. The third section presents the data and variables used. The fourth section provides estimation findings and some robustness checks, and the conclusion summarizes some possible implications for household behavior and public policy regarding consumer protection.

**II. Basic Model Specification and Estimation Details**

Let $MEW^*$ denote the unobservable gain to the household from refinancing to withdraw mortgage equity and let $MEW$ be an indicator variable equaling 1 if $MEW^* > 0$ and zero otherwise. We then model the probability of refinancing to withdraw mortgage equity as:

$$\begin{aligned} Prob(MEW_{\mathrm{ist}} = 1) = \Phi( &\beta_0 + \beta_1\, Dlit_i + \beta_2\, RefIncent_{it} \;+\; \beta_3\, \mathrm{HomeApprec}_{st} + \\ &\beta_4\, Garnish_s + \; Chapter13_s + \beta_6\, Unempd_{it} + \boldsymbol{X}'\boldsymbol{\gamma} + \alpha_i + e_{it}) \end{aligned} \tag{1}$$

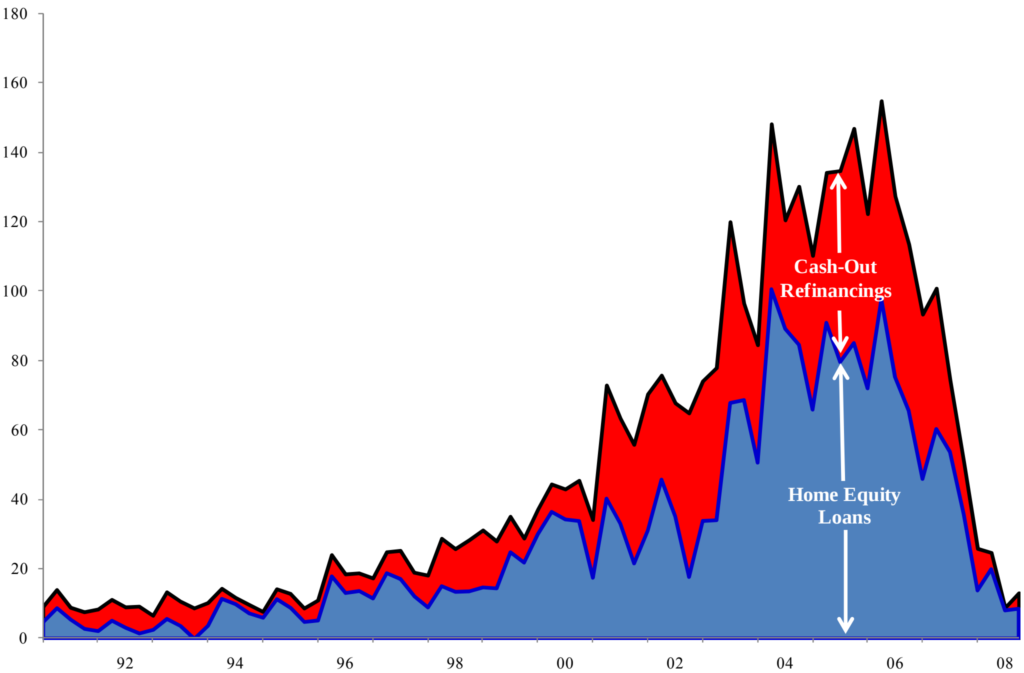
where $i$, $t$, and $s$ index households, year, and state of residence, respectively. $Dlit_i$ is a dummy variable for whether the respondent is financially literate. $RefIncent_{it}$ measures how much refinancing lowers the mortgage payment, equal to gap between new and the average existing mortgage interest rate multiplied by household i's mortgage principal. $\mathrm{HomeApprec}_{st}$ is the state level three year average annual price appreciation. $Garnish_s$ and $Chapter13_s$ are state level legal variables that may be correlated with the incentive to withdraw equity. *Unemp*, a 0-1 variable for whether the respondent was unemployed in the prior two years, allows us to account for the role of liquidity constraints on mortgage equity withdrawal. $\boldsymbol{X}$ is a vector of demographic variables such as age, sex, race, education, marital status, number of children in the household, permanent income, liquid wealth, loan-to-value ratio, and risk preferences, which may influence the propensity to withdraw mortgage equity. $\alpha_i$ is an individual-specific unobserved effect and $e_{it}$ is an error term that varies both with individuals and time, $\Phi(.)$ is the standard normal CDF. Assuming a standard normal distribution for the composite error term, $u_{it} = \alpha_i + e_{it}$, gives rise to the standard Probit model for estimating the probability of withdrawing mortgage equity.

The gross time series MEW estimates of Greenspan and Kennedy (2008) have three major components: MEWs arising from the turnover of homes sold, the tapping of home equity lines or borrowing from a new second or third lien mortgage, and cash-out mortgage refinancing. Our binary measure of MEW is based on home-owners who have not moved, and essentially is based on the second and third MEW components for the households in our HRS sample. This gauge of "active MEW" is viewed as a deliberate form of borrowing that empirically is more closely linked to consumer spending (Greenspan and Kennedy, 2008).

Controlling for the incentives to refinance is important in our Probit model because, by the late 1990s, refinancing mortgages became a major component of active MEW (see Figure 1 and Greenspan and Kennedy, 2008). If mortgage interest rates fall enough to overcome fixed costs to warrant refinancing, then refinancing also offers one the ability to withdraw equity from housing at little marginal cost and at tax-favored and low-collateralized interest rates. In this sense, decisions to refinance or conduct an MEW can often be a linked decision for a household wishing to borrow or to draw down its portfolio stake in housing. One advantage of conducting a cash-out refinancing over borrowing through a home equity line, is that the former give homeowners the ability to lock in low, long-term interest rates on MEWs. By contrast, home equity loans either have a variable interest rate, or if they have a fixed interest rate, it is usually above the rate on first-lien mortgages owing to the lower value of collateral on second liens.

Because of these considerations, the incentives to refinance mortgages may affect the marginal decisions to conduct an MEW. Accordingly, we include $RefIncent_{it}$ in the Probit specification.

**Figure 1: Cash-Out Refinancings Have Been a Large Component of "Active MEW"**




Source: updated data based on Greenspan and Kennedy (2008). Free cash extracted from these two components is roughly equal to "Active MEW" with some minor differences.

## III. Data and Variables

### *Defining who withdrew mortgage equity*

Our main data source is the Health and Retirement Survey (HRS), a representative sample of U.S. population age 50 and over. We then use a random subsample of HRS respondents who were selected to answer an additional three financial literacy questions in 2004. From these, we focus on homeowners who remained in their 1998 homes across five semi-annual HRS surveys conducted between 1998 and 2006.[1] We defined a household as

[1] 1266 respondents were asked one of the financial literacy questions in 2004 yielding 4232 respondent years, after imputing the 2004 response for each respondent to all years the sample. 74% of these are homeowners and 92% of the homeowners did not buy or sell a house, leaving us with 2706 observations. After dropped observations due to any missing variables, we are left with 2433 observations in the baseline model in Model 3 of Table 2.

withdrawing equity from their homes if their reported outstanding mortgage debt rose from one survey to the next (if so *MEW* =1, and 0 otherwise).[2] Effectively, MEW activity can occur if the household borrowed during a two-year interval using a home equity loan, another type of second or third mortgage, or refinanced their old mortgage debt into a larger new mortgage (a "cash-out" mortgage refinancing). We restrict our measures of MEW to where mortgage debt rose without or with the use of home equity lines of credit (HELOC's) for reasons discussed later.

***Demographic and Educational Background Control Variables***

Each Probit model includes a baseline set of control variables. Demographic controls include the age of the respondent (*Age*), and 0-1 variables for whether the respondent is white (*White*), male (*Male*) and married (*Married*). If loan demand or acceptance of new financial products is declining in age, *Age* could have a negative sign. If households with older or white members face less binding credit constraints on consumer loans (e.g., Duca and Rosenthal, 1993), their demand to withdraw housing equity would be lower. We also include the number of children living in the household (*NumChildren*), which likely has a positive effect if larger families have higher debt demand or are more likely to face binding size limits on consumer loans. *Unemp* could have a positive sign if a consumption-smoothing boost to loan demand from unemployment outweighs any decline in loan supply to the unemployed. Because we assess the role of financial literacy rather than general educational background, we include a common set of 0-1 variables for whether the respondent only graduated from high school (*HSchoolGrad*), graduated from college (*CollegeGrad*) or graduated from high school but only attended college without graduating (*SomeCollege*). Summary statistics are in Table 1.

***Variables Controlling for Financial Condition***

[2] There was no difference in the sample if the threshold for an MEW were a $1 or $1,000 rise in mortgage debt.

Research has found that the liquidity constrained are more likely to tap home equity (Hurst and Stafford, 2004). Such households may also face differential costs of maintaining financial literacy. Therefore most models include two proxies for liquidity constraints: whether the respondent is unemployed (*Unemp*) and its lagged liquid assets (*LaggedLiquidAssets*). To account for permanent income, we calculate the average total household income over all the years in the sample (*MeanIncome*). Because MEW may depend on how much a respondent has already leveraged their home, we also include lagged loan-to-value ratio (*LaggedLTV*).

***Measuring Financial Literacy***

A key variable in equation (1) is $Dlit_i$, a measure of financial literacy of the respondent. To gauge financial literacy, we used several 0-1 variables measuring if a household correctly answered a financial literacy question (=1 if correct, 0 otherwise). One question (*LitCompound*) asked whether one would have more than, equal to, or less than $1.02 in a deposit account after three years if one originally deposited $1 and earned an annual deposit rate of 2 percent. Correct answers likely reflect numeracy. Another question (*LitMonIllus*) asked whether one could buy more of, the same, or less than a given basket of goods if one bought them today with $100, or if one waited a year, during which the inflation rate equaled 2 percent and the $100 were put in a bank deposit earning 1 percent annual interest. Correct answers likely reflect literacy in the sense of numeracy and understanding money illusion. The third question (*LitPortRisk*) asked whether it were safer to invest in a stock mutual fund or an individual company's stock. Correct answers to this question likely reflect a basic understanding of portfolio diversification. Only 34 percent correctly answered all three questions, with 69, 78, and 55 percent, correctly answering the compound interest, money illusion, and portfolio diversification questions, respectively. We classified those who did not answer a particular question with those who incorrectly answered as financially illiterate. Coefficient estimates were similar dropping those not answering from the sample, with standard errors larger owing to fewer degrees of freedom.

***Standard MEW Supply and Demand Variables and Control Variables***

Following Benito (2008) we control for standard MEW factors and other influences. Several reflect the reduced-form effects of loan supply and demand factors that work in the same (e.g., house price appreciation) or opposite (regional variation in the rights of debtors versus creditors) direction. If whites are less liquidity constrained from having more inherited wealth or face easier constraints for non-secured credit than nonwhites, the coefficient on *White* would reflect positively signed loan demand and loan supply effects. Other variables primarily reflect demand factors. Nevertheless, if some demographic variables and the unemployment dummy are also correlated with credit constraints, there may be some oppositely signed loan supply and demand effects. This implies that some estimated coefficients reflect the net effect of oppositely or ambiguously signed loan demand versus loan supply. For example, lower income from unemployment might lower loan demand or increase the desperation need to tap housing wealth to smooth consumption, whereas loan supply will likely be reduced. We find a positive, but statistically insignificant sign on the 0-1 variable for being unemployed over the prior two years.

***Mortgage Interest Rate Incentives to Refinance***

Homeowners who do not sell their homes can withdraw housing equity by taking out a second mortgage or refinancing their old mortgage with a larger loan. Owing to the transactions costs of refinancing, the incentive to withdraw housing equity is enhanced if borrowers benefit from refinancing mortgages at lower interest rates. To control for the latter, we include the product of an individual's mortgage debt in the prior survey and maximum quarterly interest rate gap, defined as the average interest rate on outstanding mortgages minus the interest rate on new mortgages, *RefIncent*.[3] The higher the ratio, the more advantageous it is to refinance a mortgage and to withdraw housing equity via mortgage refinancing. We interacted this variable with different measures of financial literacy to test whether the financially literate are more likely to withdraw housing equity when refinancing entailed switching to a lower mortgage interest rate.

Freddie Mac data reveal there are periods when the average refinancing homeowner replaces a lower interest rate mortgage with one having a higher interest rate and larger principal balance. This pattern suggests a role for credit constraints since households can usually borrow against home equity at a lower interest rate than on unsecured loans. So even if a mortgage refinancing raises the interest rate on the owner's prior mortgage balance, it may be their lowest interest rate option for new borrowing. Another rational interpretation of borrowers replacing lower with higher interest rate mortgages is that they may be switching from adjustable-rate mortgage to a higher, but more stable, fixed rate mortgages. An alternative explanation is that financial illiterate borrowers might mistake the lower mortgage payments from lengthening the maturity of mortgages for the true cost of the mortgage rather than the higher interest rate. A related possibility is that the financially illiterate may not adequately consider the higher cost of refinancing their mortgages when withdrawing housing equity. These last two alternatives imply that the financially illiterate are more likely than the literate to withdraw housing equity.

***House Price Appreciation***

House price appreciation raises loan supply, reflecting greater collateral, and loan demand, reflecting a greater ability to smooth consumption or rebalance asset portfolios. To control for house price appreciation, we included the annualized real appreciation rate of house prices over the three years preceding each HRS survey using state FHFA house price indexes deflated by the personal consumption expenditures deflator (*HomeApprec*).

[3] Not knowing the horizons of homeowners, we could not calculate present values, as in Hurst and Stafford (2004).

***Cross-State Differences in Bankruptcy and Default Laws***

Recent literature examines the links between cross-state variation in lending laws and loan quality. Based on variables used by Lehnert and Maki (2007) and Lefgren and McIntyre (2009), we control for differences in laws about what portion of a bankrupt borrower's (1) income is shielded from garnishment (*Garnish*) and (2) what percent of real estate assets are shielded by homestead exemptions from nonmortgage lenders (*Homestead*, scaled as a percent of median existing house prices, National Association of Realtors).[4] Using another data source,[5] we also control for whether (3) lenders need to file a lawsuit to start the foreclosure process (*Judicial* = 1 if only judicial proceedings allowed, .5 if nonjudicial and judicial are allowed, and 0 if only or predominantly nonjudicial) or (4) mortgage lenders have access to other borrower assets or income if there is a shortfall between the principal (plus fees) and the net value of real estate collateral collected on a repossessed home (*Deficiency*=1 if allowed, 0 if not or impractical).

In principle, such variables affect loan supply and loan demand, sometimes in opposite directions. For example, the higher the share of income exempt from garnishment (*Garnish*),[6] the more willing lenders are to supply real-estate secured loans relative to other forms of consumer credit. The reason is that unsecured consumer credit lenders have less recourse to a bankrupt borrower's future income, while mortgage lenders can repossess a home. This effect on the relative loan supply of loans could be offset if a higher share of income shielded from garnishment dissuades lenders from supplying credit to denizens of a state, resulting in a

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[4] We use data from Legal Consumer (http://www.legalconsumer.com/bankruptcy/laws/) on bankruptcy exemptions for nonfarm property for married or joint owners on standard residential homes (not mobile homes), excluding any extra exemptions for disabled, elderly, or mentally ill people. The exemption used also assumes that a family contains two minor children (minors affect the size of the bankruptcy exemption in Maine, Tennessee, and Virginia).

[5] Source: All Foreclosure, http://www.all-foreclosure.com/procedures.htm. For missing data on South Dakota, state laws indicated that deficiencies are allowed and that there is a mix of judicial and non-judicial proceedings.

[6] Most states follow federal laws making 25% of disposable income subject to garnishment. Some states set lower percentage limits. Where state guidelines exempt "living expenses," we multiply the share subject to garnishment by 50% to adjust for living expenses. In states shielding a nominal weekly amount of income, we annualize income and tdivide by 1999 state median family income downward by 25% to convert income into an after-tax equivalent.

negative effect of garnishment. The impacts of such considerations on loan demand are oppositely signed. Greater shielding from garnishment tends to boost loan demand, while giving borrowers more of an incentive to substitute unsecured loans for collateralized loans.

Withdrawing home equity should theoretically be increasing in the share of real estate assets shielded in bankruptcy from a nonmortgage lender (*Homestead*). The reason is that a nonmortgage lender has less recourse to a bankrupt's real estate assets, while mortgage lenders can still repossess a home. In theory, by raising the costs of collecting on delinquent mortgages, *Judicial* should be negatively related to lenders willingness to allow borrowers to withdraw housing equity. In contrast, by enabling mortgage lenders to collect more than collateral in the case of default, *Deficiency* should be positively (negatively) related with the propensity to make an MEW if loan supply effects outweigh (are outweighed by) loan demand effects. Variables like *Homestead*, *Judicial*, and *Deficiency* have been statistically insignificant in accounting for cross-state variation in loan quality, in contrast to variables accounting for garnishment or the relative use of chapter 13 versus chapter 7 bankruptcy (Lefgren and McIntyre, 2009).

State "legal cultures" can differ insofar as differences in legal precedents and formal legal restrictions and regulations favor the use of Chapter 13 bankruptcy over Chapter 7 bankruptcy. If a borrower files under Chapter 7, they allow all non-shielded assets (pensions and homestead-protected real estate are exempt) to be liquidated to settle their debts. If they file under Chapter 13, they commit to making negotiated loan payments over the next 3-5 years without having to liquidate unshielded assets. Garnishments (direct deductions from a borrower's paycheck to the lender) are still subject to state limits. If a borrower does not meet Chapter 13 commitments, the lender can start a new bankruptcy proceeding. Chapter 13 generally is seen as less advantageous to lenders and allows borrower attorneys to collect higher fees that lower net payouts to lenders.

Of these legal variables, Lefgren and McIntyre (2009) find that only the garnishment (*Garnish*) and the Chapter 13 share of bankruptcy filings (*Chap13Share*) were statistically significant, with both having a positive correlation with cross-state variation in the rate of bankruptcy filings, and *Garnish* explaining an economically significant portion of cross-state variation. Largely in line with this result, we find that the only significant legal variables are *Garnish* and *Chap13Share*.

## IV. Estimation Results

We estimate Probit models of nonHELOC MEWs that all include a basic set of demographic and background variables, but differ as to whether they include variables for financial literacy, controls for liquidity controls, permanent income, loan-to-value ratio, and legal differences across states, year and state fixed effects. Due to the lack of time variation in financial literacy we are not able to estimate models with individual specific fixed effects to control for any unobserved heterogeneity correlated with financial literacy or other variables as well as with MEW propensity. We start by assuming that, conditional on other covariates in equation (1), $Dlit_i$ and other right hand side variables are uncorrelated with both $\alpha_{\mathrm{i}}$ and $\mathrm{e}_{\mathrm{it}}$. Because errors may be correlated across years for the same unit owing to the presence of unobserved effects $\alpha_i$,, all standard errors are clustered at the respondent level. Table 2 reports findings from eight models.. The baseline model (Column 1) includes family demographic variables, key variables capturing the incentive to refinance i.e., *RefIncent*, and *HomeApprec*. The last two have statistically significant positive coefficients, implying that there was a greater propensity to tap housing equity via MEWs among those having greater interest rate incentives to refinance and who lived in states with faster house price appreciation. In the baseline model, there are only three other variables that are statistically significant, with older and white households having a significantly lower propensity to withdraw housing equity and with the number of children positively affecting that propensity. Loan demand is likely to be less among the first two of those three categories, while unsecured loan supply could be greater if whites face easier credit constraints, consistent with Duca and Rosenthal (1993). For these reasons, the coefficients on these variables are loosely consistent with the view that credit constrained households are more likely to withdraw housing equity because empirically younger, nonwhite, and larger families have a greater likelihood of being credit constrained.

To estimate the impact of financial literacy on MEW propensity, Column 2 adds the three financial literacy variables to Column 1. Of these, only *LitPortRisk* was statistically significant, indicating a lower MEW propensity for those having some portfolio literacy. The estimated marginal effect of -0.037 implies that those who answered *LitPortRisk* correctly are 3.7 percentage points less likely to withdraw mortgage equity relative to those who either answered incorrectly or did not know. The remaining columns, therefore, drop the two insignificant measures of financial literacy. Dropping the alternative measures of financial literacy in Column 3 has minimal impact on the estimated impact of *LitPortRisk* on propensity to withdraw equity.

To capture the impact of liquidity constraints on MEW, Column 4 controls for whether the respondent is unemployed (*Unemp*) and includes lagged liquid assets (*LaggedLiquidAssets*) The positive sign on the unemployment dummy suggests that the increased loan demand effects associated with smoothing consumption have positive credit constraint effects on the likelihood of conducting an MEW that outweigh any negative effects of loan supply or loan demand associated with job loss. The negative and significant sign on lagged liquid assets confirms that liquidity constraints are a key driver of propensity to withdraw equity. Column 5 enriches the model to include mean household income (*MeanIncome*) and lagged loan-to-value ratio (*LaggedLTV*), which does not qualitatively change the estimated impact of financial literacy.

Several legal variables were added to the model in Column 6, and a model selection procedure was used to progressively omit the most insignificant legal variable. In the end, only *Chap13Share* and *Garnish* were at least marginally significant. In particular, there was a statistically significant greater MEW propensity in states whose legal environment induced the use of Chapter 13 over Chapter 7 bankruptcy and a marginally significant higher propensity in states protecting a higher share of household income from garnishment. This suggests that the positive loan demand effects of legal codes favoring debtors outweighed the impact of their negative loan supply effects. The apparent weaker effects on loan supply may reflect that some lenders underestimated the downside risk of new mortgage products in the recent housing boom (Duca, Muellbauer, and Murphy, 2010). The only notable effect from including the legal variables is that the house price appreciation coefficient is larger and more significant. The estimated impact of financial literacy on MEW propensity is significant and remarkably stable across Columns 1-6 in Table 1, indicating that the financially literate are about 3 percentage points less likely to withdraw equity via refinancing or using traditional second mortgages.

The decision to withdraw equity using traditional first or second mortgages may operate differently from tapping home equity lines of credit (HELOC's). HELOC borrowers have been found to differ in many respects, tending to be wealthier and own more expensive homes. A possible reason is that HELOCs tend to be held in portfolio, giving lenders more incentive to use tighter credit standards than on traditional mortgages which are more often securitized. Therefore, in Column 7 of Table 2, we explore whether the financially literate exhibit any systematic differences in tapping HELOC's. We find no significant effect of financial knowledge on changes in HELOC debt. In Column 8, we use both forms of MEW, i.e., through the first and second mortgages and HELOC's. Not surprisingly, the sign on financial literacy, although negative, is insignificant. Much of the impact of financial literacy on MEW appears to operate through changes in first and second nonHELOC mortgages as apparent in Columns 1-6.

***Identification***

The estimates in Table 2 may be biased from three potential sources of endogeneity in self-reported financial literacy. First, the cross-sectional variation in financial literacy may correlate with underlying differences in risk preferences that affect the propensity to refinance, inducing correlation between financial literacy and unobserved heterogeneity, $\alpha_i$. Second, even after controlling for risk preferences, financial literacy may be correlated with the time varying error term, $e_{it}$, as households may learn from any experience with mortgage borrowing, leading to biased estimates. We address this concern by using an instrumental variables approach.

Even controlling for many covariates, regression-based estimates of financial literacy on MEW may be biased if the functional form is miss-specified or if significant differences in covariates exist between the financially literate and illiterate. Invoking a selection on observables argument from the program evaluation literature, we use propensity score and nonparametric matching methods to estimate the causal effect of financial literacy on MEW propensity.

***Controlling for Risk Preferences***

While time invariance of our financial literacy variable precludes eliminating $\alpha_i$ in eq. (1) using a traditional fixed effects approach, we attempt to deal with it using survey-based measures of risk tolerance as a proxy for unobserved heterogeneity and estimate the following model.

$$Prob(MEW = 1)_{ist} = 1\big(\beta_0 + \beta_1\, Dlit_i + \beta_2\, RefIncent_{it} + \beta_3\, \text{HomeApprec}_{st} + \beta_4\, Garnish_s + Chapter13_s + \beta_6\, Unemployed_{it} + \textstyle\sum_{j=2}^{6} Rrisk_{jit} + \boldsymbol{X'\gamma} + \alpha_i + e_{it} > 0\big), \tag{2}$$

where $Rrisk_{jit}$ are categorical dummies of risk tolerance with $Rrisk_{6it}$ denoting the most risk averse and $Rrisk_{1it}$, the least risk averse and the omitted group. Risk tolerance measures do not exist in most datasets, but a unique set of income gambling questions provides controls for this commonly omitted variable.[7] The key identifying assumption is that controlling for risk aversion and other characteristics, any remaining individual variation in financial literacy owes to exogenous factors unrelated to individual choice and unobserved determinants of mortgage debt.

We enhance the models in Table 2 by including 5 survey-based measures of risk aversion.[8] Across columns 1-6 of Table 3, which correspond to columns in Table 2, the financial literacy results are stronger and more significant. Price appreciation is no longer significant in most models in Table 3, perhaps reflecting that homeowners in states with more variable prices could be less risk averse than those elsewhere. This possibility suggests that the less risk averse sort into states with more volatile prices or that higher price appreciation affects risk preferences. The statistical significance and coefficient magnitude of the interest rate incentive gain variable are very similar to Table 2. Finally, *Garnish* is no longer significant in Column 6. Controlling for preference heterogeneity in Table 3 suggests that the financially literate are about 5 percentage points less likely to withdraw mortgage equity. In columns 7 and 8 of Table 3 MEW use is based on changes in debt balances on regular mortgages and HELOC's. The results are very similar to those in Table 2, i.e., almost all of the difference in MEW propensity between financially literate and the not so savvy operates through increases in first and second mortgages.

***Addressing Robustness***

We test the sensitivity of our results to including cohort effects, retirement status, health status of the respondent and spouse, year effects and state fixed effects. Table 4 includes the 5 categorical measures of risk aversion in each model. Since state legal variables are insignificant in Column 6 of Table 3, we focus on the robustness of our estimates in Column 5 of Table 3.

[7] These measures are based on a set of income gamble questions asking the respondents to choose between a job with guaranteed current income and an alternative job with a probability of earning twice their current income with an inverse probability of earning half that income. The probabilities of lower income are 1/10, 1/5, 1/3, 1/2, and 3/4. Responses are classified into six categories from the least to the most risk averse. Adding these variables reduced the sample size to 1,239 because only a subset of households in the HRS were asked these questions.
[8] Engelhardt and Kumar (2011) use survey measures of risk aversion to control for unobserved saving preferences.

Column 1 in Table 4 is identical to Column 5 in Table 3. Adding cohort effects in Column 2 does not notably alter the size or significance of the impact of portfolio literacy. Columns 3 and 4 add retirement and health status to control for any correlation they might have with financial literacy or MEW propensity. The estimated impact of financial literacy remains stable, although significant at the 10% level. Column 5 augments the model with year fixed effects. Since none of the cohort, retirement, and heath variables are significant (the p-value on their joint significance is 0.71), we drop them in Column 6 and include both year and state fixed effects. Including these fixed effects helps control for omitted factors correlated with financial literacy that vary across time and space, the latter of which might reflect self-selection effects arising from correlations of unobserved preference or other variables with financial literacy. Although year and state fixed effects are jointly significant, the qualitative and quantitative results are basically unchanged. As before, the estimated effect of financial literacy is significant at the 5 percent level and lowers MEW propensity by 5 percentage points, with little change in the coefficients across the models.[9]

***Robustness to Alternative Measures of Financial Literacy***

We have so far used the correct response of the portfolio risk question (*LitPortRisk*) to define financial literacy. To check the sensitivity of the estimated impact of financial literacy on MEW propensity to alternative measures of literacy, Table 5 presents results using different proxies for financial literacy as the dependent variable with the r.h.s. variables from the model in column 6 of Table 4. In column 1, *LitPortRisk* measures financial literacy. Column 2 and 3 use *LitCompound* and *LitMonIllus*, respectively. When entered individually, the marginal effect of *LitCompound* is very similar to *LitPortRisk*, while *LitMonIllus* in column 3 is insignificant. In column 4, we use the total number of correct answers to the three literacy questions, which

[9]Although our estimates may be sensitive to selection effects from analyzing only homeowners who did not move, Hurst and Stafford (2004) found no evidence of selection due to omitting movers in their refinancing model.

ranges from 0 to 3. This measure is significant but slightly less than *LitPortRisk*. Finally, in column 5, we instead use a financial literacy index (*Fin Lit Index*) similar to Van Rooij, *et. al* (2011) and Lusardi and Mitchell (2009), by extracting the common factor from all three variables. The estimated impact of financial literacy is robust to using this broad measure.

Despite robustness of our main result to controlling for a rich set of covariates including risk preferences, our estimated impact of financially literacy on home equity borrowing may still be biased if the individual choice of attaining financial literacy is correlated with other factors that are also correlated with borrowing. We use instrumental variable methods to purge our estimates of the impact of financial literacy of biases arising from any remaining endogeneity.

***Linear Instrumental Variables Estimates***

Table 6 presents instrumental variable results and to conserve space, shows results only using the financial literacy variable *LitPortRisk*. Panel A of Table 6 provides instrumental variable estimates of a linear probability model of MEW propensity. We use state level average high school graduation rates, and the educational attainment of parents as instruments. Although the point estimates are negative, we cannot reject the hypothesis that the impact of financial literacy on MEW is not different from zero. This result is not surprising, as the p-values on the significance of our instrument set in the first stage (see the bottom panel), are high, implying that the instruments are weak. For the baseline model in column 1, the instruments are significant in explaining financial literacy at 10 percent level. However, in richer specifications in columns 2-6, the instruments have little explanatory power in the first stage, although the overidentification test based on the Sargan statistic indicates that the overidentifying restrictions are valid. Given insignificant IV coefficients, a Hausman specification test for IV vs. OLS estimation of a linear probability model of MEW propensity, fails to reject the null hypothesis of no endogeneity.

***Hausman and Taylor Models for Panel Data***

To fully exploit the panel nature of our data, we use an alternative instrumental variables (IV) strategy to estimate eq. (1). Although, the time invariance of our key variable, $Dlit_i$, precludes estimation of fixed effects, we use the “HT” approach of Hausman and Taylor (1981). To implement this approach we partition the vector of covariates into four categories based on their correlation with the unobserved heterogeneity $\alpha_i$: endogenous and exogenous time-varying and endogenous and exogenous time constant. *RefIncent, LaggedLTV, Dlit, LaggedLiquidAssets,* and *risk tolerance indicators*, are considered correlated with $\alpha_i$, and hence endogenous. The HT strategy entails first estimating coefficients on time-varying covariates by first differencing. The coefficients on time constant variables such as financial literacy are then estimated with an IV approach by using means of exogenous time-varying variables and time-constant covariates as instruments. Results are in panel B of Table 6. Bootstrapped cluster-robust standard errors are in parentheses. The coefficients on financial literacy imply that the impact of financial literacy on MEW propensity is implausibly high, ranging between -0.42 and -1.29. Moreover estimates are highly imprecise as the HT approach is also susceptible to lack of identification due to a weak instruments problem plaguing the linear IV estimates.

***Bivariate Probit Model with Instrumental Variables***

Two concerns with the simple linear IV strategy and HT approach are that (1) both are estimated using linear probability models and (2) they are plagued with weak instruments problem. We now specify a Probit equation for financial literacy:

$$Prob(Dlit_i = 1)_{ist} = \Phi\big(\pi_0 + \pi_1\, Dlit_i + \pi_2\, RefIncent_{it} + \pi_3\, \text{HomeApprec}_{st} + \pi_4\, Garnish_s + \pi_5\, Chapter13_s + \pi_6\, Unemployed_{it} + \boldsymbol{X}'\boldsymbol{\delta} + \boldsymbol{Z}'\boldsymbol{\phi} + \epsilon_{it}\big), \tag{3}$$

where $\boldsymbol{Z}$ is the vector of instruments excluded from the MEW equation, consisting of state average high school graduation rates, father’s educational attainment, and mother’s educational attainment.

We assume that composite error terms $u_{it}\ and\ \epsilon_{it}$ in (1) and (3), respectively have a joint bivariate normal distribution each with mean zero, variance of one and unknown correlation $\rho$.

Jointly estimating the MEW and financial literacy equations (1 and 3, respectively) in a bivariate Probit model may aide identification. Intuitively, in addition to the instruments in the financial literacy equation, the nonlinear functional form of such models is an important source of identification. Unlike univariate Probit models, bivariate models yield multiple types of marginal effects on joint, marginal and conditional probabilities. Given our focus on the impact of financial literacy on MEW propensity, we present the marginal effects of financial literacy on the joint probability of $MEW = 1$ and $Dlit_i = 0$. Standard errors reported are robust and have been clustered at the respondent level. The estimated marginal effect on financial literacy on MEW, evaluated at the mean of covariates, is negative, ranging from -0.03 to -0.06 across the specifications. Unlike the linear IV specifications in panels A and B, the estimated marginal effects are significant across most models and are similar in size to the univariate Probit models.

***Evidence from Matching Estimators***

As is well-known from the treatment effect/program evaluation literature (Heckman, Ichimura, and Todd, 1997; Heckman, Lalonde, and Smith, 2000; Imbens, 2004; Abadie and Imbens, 2006; Imbens and Wooldridge, 2009), a primary concern with estimating the causal impact of financial literacy on MEW propensity is that the linear regression form assumed in specifications (1) and (2) does not effectively adjust for significant differences in observed characteristics between the financially literate and the illiterate as is apparent from Table 1. Further, the linear regression framework is highly sensitive to functional form misspecifications that could cloud the estimated impact of financial literacy on MEW propensity. We are primarily interested in estimating an overall impact of financial literacy or the average treatment effect

($ATE$) and the impact of financial literacy on $MEW$ for those who are literate, i.e., average treatment effect on the treated ($ATT$). An important focus of our paper is the impact of exposing the financially illiterate to financial education programs, i.e., the average treatment effect on the untreated ($ATU$). We estimate the three key parameters nonparametrically, using two recently adopted types of matching estimators: propensity score matching (see Dahejia and Wahba, 2002; and Leuven and Sianesi, 2003) and nearest-neighbor matching (Anadie and Imbens, 2006).

Without getting into too much technical detail, the qualitative and quantitative results reported earlier are robust to using matching estimators. As discussed in Appendix A, which refers to Table 7, the estimated effects of financial literacy on MEW propensity across all specifications are negative and statistically similar to the results from univariate and bivariate Probit models. The results are somewhat sensitive to changes in specification but are statistically not different across specifications. In general financial literacy leads to a decline in MEW propensity, particularly for the financially illiterate by an average of about 5 percentage points.

Despite robustness and statistical significance of the estimated impact of financial literacy, there are some caveats. First, the lack of time variation in financial literacy precludes comprehensively accounting for unobserved heterogeneity in refinancing. Although we control for such heterogeneity with survey-based measures of risk aversion, our estimates could still be biased if there is any remaining correlation between financial literacy and unobserved taste for mortgage debt. Second, our measure of the increase in the sum of first and second mortgages is an imperfect measure of propensity to withdraw housing equity via a mortgage refinancing.

## V. Interpretation and Conclusion

During the U.S. mortgage borrowing boom of 1998-2005, middle- to older age households who have more children, are younger, are nonwhite, and are financially illiterate about portfolio risk were more likely to have "actively" withdrawn housing equity via cash-out mortgage refinancing or borrowing against traditional second mortgages. Our estimates suggest that financially illiterate were about 3 to 5 percentage points more likely to withdraw mortgage equity using these means. The results regarding literacy accord with those of Lusardi and Mitchell, who find that literacy with respect to portfolio risk was more linked to suboptimal retirement preparation than literacy with respect to numeracy and money illusion based on the 2002 Health and Retirement Study. Our results also are consistent with findings from the UK (Miles, 2004) and U.S. (Bucks and Pence, 2008) that many households do not fully understand important characteristics of their mortgages. Our results loosely accord with those of Lusardi and Tufano (2009) and Stango and Zinman (2008), who find that illiteracy is linked to over-borrowing and under-accumulation of wealth.

Nevertheless, those latter studies defined literacy with respect to numeracy. We find that literacy based on computational questions involving compound interest or money illusion was insignificantly related to MEW activity when all three measures of financial sophistication were included in the specification. However, literacy in terms of understanding basic portfolio diversification was significant. Aside from our use of a smaller sample, there is a plausible explanation for this apparent difference in findings with respect to Lusardi and Tufano (2008) and Stango and Zinman (2008). First, they assess financial behavior in quantitative terms, where computational literacy would, a priori, seem to matter. In contrast, our probit models assess whether or not a household withdraws any housing equity at all. For such a binary decision, basic financial sense rather than numeracy could plausibly matter more. Although our data do not allow us to examine the propensity to refinance (we only observe the change in the amount of mortgage debt, not the interest rate or date of origination), our findings illustrate that mortgage borrowing is affected by illiteracy. In this loose sense, our results are not *in*consistent with Campbell's (2006) hypothesis that financial literacy contributed to his finding that many people did not refinance their mortgages when lower interest rates could have saved on borrowing costs.

We also find that households are more likely to withdraw housing equity in states where the legal code and culture make lenders less able to collect from bankrupt borrowers, consistent with Lefgren and McIntyre's (2009) emphasis that legal differences across states can help explain borrowing behavior. This suggests that MEW activity differs across states not only due to differences in house price appreciation rates, but also to differences in bankruptcy codes.

This study's findings also have at least two public policy implications. First, during the U.S. mortgage boom of the late 1990s and early 2000's, financial illiteracy contributed to mortgage equity withdrawals that increased household debt. Given the macro implications of mortgage equity withdrawals for consumption during the recent boom and bust, as well as the micro implications for optimal behavior for individual households, this finding suggests a possible role for public policy to improve financial literacy and make mortgage information disclosure more understandable and accessible to the general public. Second, although redressing mathematical illiteracy among adults is difficult, the stronger link of MEW behavior with portfolio literacy than with numeracy offers hope that financial education might help prevent suboptimal borrowing. Nevertheless, designing effective education programs entails dealing with a number of factors (e.g., more intensive lender screening and even cognitive decline over the life-cycle) as stressed by Agarwal et al. (2009) and Agarwal et al. (2010).

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## Appendix A: Evidence from Matching Estimators

We focus on estimating the overall impact of financial literacy or the average treatment effect ($ATE$) and the impact of financial literacy on $MEW$ for those who are literate, i.e., average treatment effect on the treated ($ATT$ ). An important goal of our paper is gauging the impact of exposing financially illiterate to financial education programs, i.e., the average treatment effect on the untreated ($ATU$). We estimate the three key parameters nonparametrically, using two recently adopted types of matching estimators: propensity score matching (Dahejia and Wahba, 2002, Leuven and Sianesi, 2003) and nearest-neighbor matching (Anadie and Imbens, 2006).

Denoting the MEW propensity of a financially literate respondent $i$ as $MEW_{i1}$and that of a financially illiterate respondent as $MEW_{i0}$, and denoting $E(.)$, the expectation operator, we estimate $\mathrm{ATE}, \mathrm{ATT}$, and $ATU$ conditional on other covariates $X = x$, using the following:

$$ATE(x): E(MEW_{i1}|X = x) - E(MEW_{i0}|X = x) \tag{4}$$

$$ATT(x): E(MEW_{i1}|Dlit_i = 1, X = x) - E(MEW_{i0}|Dlit_i = 1, X = x) \tag{5}$$

$$ATU(x): E(MEW_{i1}|Dlit_i = 0, X = x) - E(MEW_{i0}|Dlit_i = 0, X = x) \tag{6}$$

Averaging over all values ofX, gives an estimate of $\mathrm{ATE}, \mathrm{ATT}$, and $ATU$. Noting that the missing counterfactual, $\mathrm{E(MEW_{i0}|Dlit_i = 1, X)}$ in (5), is unobserved for a financially literate respondent, we find the closest match among the financially literate based on covariates. An analogous matching strategy is used to construct the missing counterfactual $\mathrm{E(MEW_{i1}|Dlit_i = 0, X)}$ in (6). Comparing the MEW propensity between a respondent and the closest match from the opposite treatment and averaging over the sample provides consistent estimates of ATT and ATU.

As explained in Abadie and Imbens (2006), two assumptions are required for consistency of $ATE$: unconfoundedness and overlap. Unconfoundedness, also known as selection on observables or conditional independence, requires that the treatment, i.e., financial literacy $Dlit_i$, is independent of the two potential outcomes $MEW_{i1}$ and $MEW_{i0}$, conditional on covariates $X$.

The unconfoundedness condition is weaker for the identification of $ATT$ as the only requirement is that treatment $Dlit_i$ be independent of the no treatment outcome $MEW_{i0}$. The overlap condition requires that $0 < Prob(Dlit_i = 1|X) < 1$ ,i.e., there must be financially literate as well illiterate at all possible values of covariates vector $X$ so that an appropriate match for the financially literate can be constructed at each value $x$ of $X$ from the control group, i.e., the illiterate and vice-versa. For identifying $ATT$, this condition weakens to $Prob(Dlit_i = 1|X) < 1$.

We first construct an appropriate match using the nearest neighbor approach proposed in Abadie and Imbens (2006). Let $M$ be the number of respondents in the control group forming a set of possible candidates, $S_{Mi}$, for being closest, in covariates $X_i$ based on an appropriate distance measure, to the $i_{th}$ member of the treatment group. Denote $N, N_1, N_0$ as the numbers of the overall sample, the treatment group and the control group, respectively.[10] An estimate $\widehat{MEW}_{i0}$ for the financially literate is constructed from the observed indicator $\mathrm{MEW_{ij}}$ as follows:

$$\widehat{MEW}_{i0} = \frac{1}{\mathrm{M}} \sum_{\mathrm{j \epsilon S_{Mi}}} \mathrm{MEW_{ij}} \ \text{ if } \mathrm{Dlit_i} = 1 \tag{7}$$

Analogously, the counterfactual $\widehat{MEW}_{i1}$ for the financially illiterate is constructed as:

$$\widehat{MEW}_{i1} = \frac{1}{\mathrm{M}} \sum_{\mathrm{j \epsilon S_{Mi}}} \mathrm{MEW_{ij}} \ \text{ if } \mathrm{Dlit_i} = 0 \tag{8}$$

Then as shown in Abadie and Imbens (2006), an estimator of $ATE$ can be written as:

$$\widehat{ATE} = \frac{1}{N}\sum_{i-1}^{N}(\widehat{MEW}_{i1} - \widehat{MEW}_{i0}) = \frac{1}{N}\sum_{i=1}^{N}(2 \times Dlit_i - 1)(1 + K_{Mi})\, MEW_i \tag{9}$$

$K_{Mi}$ is the frequency respondent $i$ is chosen as a match. Analogously, an estimator for $ATT$ is:

[10] The distance metric used to match a unit $i$ with covariate vector $X_i$ with another with vector $X_j$ is the vector norm $\left\|X_j - X_i\right\|_V$, where $\left\|x\right\|_v = (x'Vx)^{0.5}$.

$$\widehat{ATT} = \frac{1}{N_1}\sum_{i-1}^{N_1}(MEW_i - \widehat{MEW}_{i0}) = \frac{1}{N_1}\sum_{i=1}^{N_1}(Dlit_i - (1 - Dlit_i)K_{Mi})MEW_i \tag{10}$$

An estimate of $ATU$ is obtained exactly analogously to the $ATT$:

$$\widehat{ATU} = \frac{1}{N_0}\sum_{i-1}^{N_0}(\widehat{MEW}_{i1} - MEW_i) = \frac{1}{N_0}\sum_{i=1}^{N_0}(Dlit_i K_{Mi} - (1 - Dlit_i))MEW_i \tag{11}$$

As shown in Abadie and Imbens (2006), the estimators $\widehat{ATE}, \widehat{ATT}, \widehat{ATU}$ are unbiased only if matching is exact, which is plausible in case the covariates are discrete. The larger the number of continuous covariates, the more difficult it is to find exact matches and the estimators are biased with bias of the order $O(\frac{1}{N^k})$ where $k$ is the number of continuous covariates.

Rosenbaum and Rubin (1983) showed that if unconfoundedness holds, adjusting for the propensity score $p(x) = Prob(Dlit_i = 1|X = x)$ rather than an entire set of covariates $X$, is sufficient. This motivates a matching estimator that can be constructed by matching on a single variable (i.e., the propensity score, $p(x)$), which reduces both the "curse of dimensionality" from matching on an entire vector $X$ and the asymptotic bias. Since the true propensity score of financial literacy is unknown, it is estimated from a standard Probit or Logit model. Using estimated propensity scores, counterfactuals in (7) and (8) can be constructed based on the nearest neighbor method by comparing the propensity scores and then $\widehat{ATE}, \widehat{ATT}, \widehat{ATU}$ can be estimated as in (9), (10), and (11), with the standard errors corrected for first stage estimation.

We use both propensity score matching and nearest neighbor matching to estimate the impact of financial literacy on MEW propensity. We report cluster-bootstrapped standard errors for the propensity score-based matching estimates. We calculate the bias-adjusted estimates and appropriate standard errors for nearest-neighbor matching based on entire set of covariates $X$ using the methods suggested in Abadie and Imbens (2006). Results are presented in Table 7. Columns 1-3 present results from propensity score matching while columns 4-6 contain results from nearest neighbor matching using covariates. All estimated effects in the table are calculated by using one closest match to estimate the missing counterfactual for each respondent in the sample. Estimates based on just one match, reduce bias but can be imprecise. Therefore we also estimated all effects using two, three and four closest matches. The results were very similar and therefore we present results using just one match. The estimated effects across all specifications are negative and statistically similar to the results from univariate and bivariate Probit models. The results are somewhat sensitive to changes in specification but are statistically not different across specifications. Comparing estimates in column 1 and column 4, which are from the baseline specification, reveals that the ATE and ATU are significant when propensity score matching is used. However, when risk aversion categories are included in the covariate set, in column 2 and 4, ATE and ATU, are significant when nearest neighbor matching is used, but insignificant with propensity score matching. The estimates are neither statistically different across matching methods nor across specifications. In the richest specification in columns 3 and 6, most effects are insignificant except the ATU in column 6. Table 7 suggests that the impact of financial literacy on the MEW behavior of the control group, i.e., those who are financially illiterate, are larger than the overall ATE or the ATT which applies to the finically literate. The nearest-neighbor estimates in column 6 indicates that financial literacy among the those who are financially illiterate would reduce the likelihood of MEW by 10 percentage points, although across specifications this effect ranges from 3 to 10 percentage points. The precision of the estimates is affected by smaller sample size in richer specifications. However, the overall evidence from Table 7 is unmistakable; in general financial literacy leads to a decline in MEW propensity, particularly for the financially illiterate by an average of about 5 percentage points.

**Table 1: Summary Statistics**

| | LitPortRisk=0 | LitPortRisk=1 | Overall |
|---|---|---|---|
| Whether Wihdrew Equity | .171 | .159 | .166 |
| MEW Amount | 7622.496 | 8518.272 | 8209.268 |
| | (33571.98) | (33728.3) | (33601.28) |
| | [0] | [0] | [0] |
| RefIncent | .085 | .154 | .125 |
| | (.277) | (1.012) | (.804) |
| | [0] | [0] | [0] |
| HomeApprec | .234 | .234 | .235 |
| | (.155) | (.157) | (.156) |
| | [.177] | [.183] | [.179] |
| Garnish | .777 | .76 | .767 |
| | (.202) | (.214) | (.209) |
| | [.75] | [.75] | [.75] |
| Judicial | .496 | .517 | .505 |
| | (.462) | (.461) | (.462) |
| | [.5] | [.5] | [.5] |
| Deficiency | .728 | .715 | .716 |
| | (.445) | (.451) | (.451) |
| | [1] | [1] | [1] |
| Chap13Share | .283 | .272 | .276 |
| | (.138) | (.138) | (.137) |
| | [.28] | [.25] | [.28] |
| Unemp | .007 | .014 | .011 |
| Age | 65.446 | 62.213 | 63.523 |
| | (10.939) | (9.634) | (10.276) |
| | [64] | [60] | [62] |
| HSchoolGrad | .355 | .316 | .329 |
| SomeCollege | .252 | .247 | .247 |
| CollegeGrad | .178 | .345 | .281 |
| Male | .357 | .474 | .431 |
| White | .859 | .923 | .897 |
| NumChildren | 3.304 | 2.716 | 2.956 |
| | (2.107) | (1.749) | (1.924) |
| | [3] | [2] | [3] |

Note: Only means are presented for dummy variables. Standard errors in parentheses; median in square brackets. Estimates have been weighted by HRS household weights.

**Table 2: Marginal Effects from Probit Models of Whether Households Withdrew Housing Equity**

| MEW Channel | (1) Mortgage | (2) Mortgage | (3) Mortgage | (4) Mortgage | (5) Mortgage | (6) Mortgage | (7) HELOC | (8) Mortgage+HELOC |
|---|---|---|---|---|---|---|---|---|
| LitPortRisk | | -0.037** | -0.032** | -0.029* | -0.029** | -0.026* | 0.012 | -0.016 |
| | | (0.016) | (0.016) | (0.015) | (0.014) | (0.014) | (0.011) | (0.017) |
| LitCompound | | 0.001 | | | | | | |
| | | (0.017) | | | | | | |
| LitMonIllus | | 0.022 | | | | | | |
| | | (0.019) | | | | | | |
| RefIncent | 0.093** | 0.093** | 0.096** | 0.091** | 0.041** | 0.041** | -0.000 | 0.056** |
| | (0.020) | (0.019) | (0.020) | (0.019) | (0.019) | (0.018) | (0.002) | (0.023) |
| HomeApprec | 0.084* | 0.069 | 0.080* | 0.077* | 0.080** | 0.101** | 0.026 | 0.103** |
| | (0.043) | (0.043) | (0.043) | (0.040) | (0.038) | (0.039) | (0.031) | (0.049) |
| Age | -0.007** | -0.007** | -0.007** | -0.006** | -0.004** | -0.004** | -0.002** | -0.006** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| HSchoolGrad | 0.028 | 0.035 | 0.032 | 0.032 | 0.032 | 0.038* | 0.039** | 0.053** |
| | (0.024) | (0.024) | (0.024) | (0.023) | (0.021) | (0.021) | (0.019) | (0.026) |
| SomeCollege | 0.029 | 0.038 | 0.033 | 0.037 | 0.030 | 0.034 | 0.041* | 0.056* |
| | (0.029) | (0.030) | (0.029) | (0.028) | (0.026) | (0.026) | (0.024) | (0.032) |
| CollegeGrad | 0.016 | 0.026 | 0.022 | 0.038 | 0.036 | 0.040 | 0.058** | 0.063** |
| | (0.027) | (0.028) | (0.027) | (0.028) | (0.027) | (0.027) | (0.028) | (0.034) |
| Male | 0.022 | 0.021 | 0.021 | 0.019 | 0.020 | 0.020 | 0.004 | 0.012 |
| | (0.016) | (0.016) | (0.015) | (0.015) | (0.014) | (0.013) | (0.011) | (0.017) |
| White | -0.083** | -0.082** | -0.075** | -0.062** | -0.056** | -0.047** | -0.004 | -0.043* |
| | (0.026) | (0.027) | (0.026) | (0.025) | (0.023) | (0.022) | (0.017) | (0.026) |
| NumChildren | 0.016** | 0.014** | 0.015** | 0.013** | 0.011** | 0.011** | 0.002 | 0.015** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.004) |
| Married | -0.015 | -0.014 | -0.014 | -0.009 | -0.001 | 0.002 | 0.011 | 0.006 |
| | (0.020) | (0.019) | (0.019) | (0.018) | (0.016) | (0.016) | (0.013) | (0.020) |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Unemp | | | | 0.071 | 0.071 | 0.078 | -0.004 | 0.086 |
| | | | | (0.080) | (0.083) | (0.084) | (0.046) | (0.100) |
| LaggedLiquidAssets | | | | -0.000** | -0.000** | -0.000** | -0.000 | -0.000 |
| | | | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| LaggedLTV | | | | | 0.167** | 0.164** | 0.031* | 0.165** |
| | | | | | (0.028) | (0.027) | (0.017) | (0.033) |
| MeanIncome | | | | | -0.000 | -0.000 | 0.000* | 0.000 |
| | | | | | (0.000) | (0.000) | (0.000) | (0.000) |
| Garnish | | | | | | 0.051 | 0.016 | 0.066 |
| | | | | | | (0.035) | (0.029) | (0.043) |
| Chap13Share | | | | | | 0.116** | -0.092** | 0.028 |
| | | | | | | (0.051) | (0.040) | (0.063) |
| Observations | 2447 | 2400 | 2433 | 2433 | 2432 | 2432 | 2425 | 2425 |
| Pseudo-R-Sq | 0.08 | 0.09 | 0.09 | 0.09 | 0.12 | 0.13 | 0.06 | 0.10 |

Note: The dependent variable is whether the household withdrew housing equity. The Standard errors presented in parentheses are based on robust standard errors clustered by households. * p<0.10, ** p<0.05

**Table 3: Marginal Effects from Probit Models of Whether Households Withdrew Housing Equity (Controlling for Risk Aversion)**

| MEW Channel | (1) Mortgage | (2) Mortgage | (3) Mortgage | (4) Mortgage | (5) Mortgage | (6) Mortgage | (7) HELOC | (8) Mortgage+HELOC |
|---|---|---|---|---|---|---|---|---|
| LitPortRisk | | -0.056** | -0.060** | -0.060** | -0.059** | -0.054** | 0.012 | -0.040 |
| | | (0.024) | (0.024) | (0.024) | (0.023) | (0.023) | (0.018) | (0.028) |
| LitCompound | | -0.024 | | | | | | |
| | | (0.028) | | | | | | |
| LitMonIllus | | 0.010 | | | | | | |
| | | (0.029) | | | | | | |
| RefIncent | 0.104** | 0.106** | 0.104** | 0.103** | 0.048* | 0.049* | -0.003 | 0.061* |
| | (0.029) | (0.029) | (0.029) | (0.029) | (0.028) | (0.028) | (0.003) | (0.036) |
| HomeApprec | 0.074 | 0.061 | 0.065 | 0.067 | 0.072 | 0.106* | 0.039 | 0.085 |
| | (0.063) | (0.062) | (0.062) | (0.061) | (0.060) | (0.062) | (0.048) | (0.073) |
| Age | -0.007** | -0.007** | -0.007** | -0.006** | -0.005** | -0.005** | -0.002** | -0.008** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) |
| HSchoolGrad | 0.029 | 0.031 | 0.039 | 0.040 | 0.034 | 0.038 | 0.048* | 0.048 |
| | (0.037) | (0.036) | (0.036) | (0.036) | (0.034) | (0.035) | (0.030) | (0.040) |
| SomeCollege | 0.048 | 0.048 | 0.052 | 0.053 | 0.042 | 0.041 | 0.021 | 0.032 |
| | (0.047) | (0.046) | (0.046) | (0.046) | (0.043) | (0.043) | (0.033) | (0.049) |
| CollegeGrad | 0.010 | 0.022 | 0.024 | 0.032 | 0.020 | 0.020 | 0.045 | 0.025 |
| | (0.040) | (0.040) | (0.040) | (0.041) | (0.039) | (0.039) | (0.036) | (0.046) |
| Male | 0.037 | 0.044* | 0.039* | 0.037* | 0.034 | 0.034 | -0.006 | 0.021 |
| | (0.023) | (0.023) | (0.023) | (0.023) | (0.022) | (0.021) | (0.016) | (0.027) |
| White | -0.092** | -0.080** | -0.083** | -0.078** | -0.078** | -0.065** | 0.010 | -0.036 |
| | (0.036) | (0.036) | (0.035) | (0.034) | (0.034) | (0.033) | (0.024) | (0.038) |
| NumChildren | 0.022** | 0.020** | 0.021** | 0.020** | 0.017** | 0.017** | 0.004 | 0.021** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.005) | (0.005) | (0.004) | (0.007) |
| Married | 0.019 | 0.021 | 0.028 | 0.031 | 0.030 | 0.032 | 0.027 | 0.038 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (0.028) | (0.027) | (0.027) | (0.026) | (0.026) | (0.025) | (0.020) | (0.031) |
| Rrisk2 | 0.039 | 0.038 | 0.038 | 0.042 | 0.026 | 0.017 | -0.062* | -0.023 |
| | (0.066) | (0.066) | (0.067) | (0.068) | (0.064) | (0.064) | (0.022) | (0.066) |
| Rrisk4 | -0.086* | -0.097** | -0.099** | -0.098** | -0.096** | -0.099** | -0.037 | -0.113* |
| | (0.037) | (0.032) | (0.033) | (0.033) | (0.032) | (0.032) | (0.033) | (0.047) |
| Rrisk4 | -0.034 | -0.037 | -0.041 | -0.041 | -0.044 | -0.048 | -0.039 | -0.051 |
| | (0.043) | (0.043) | (0.043) | (0.042) | (0.041) | (0.042) | (0.031) | (0.054) |
| Rrisk5 | -0.053 | -0.062 | -0.060 | -0.060 | -0.059 | -0.066 | -0.043 | -0.080 |
| | (0.042) | (0.040) | (0.041) | (0.041) | (0.040) | (0.040) | (0.031) | (0.051) |
| Rrisk6 | -0.055 | -0.067 | -0.068 | -0.067 | -0.070 | -0.075 | -0.077** | -0.105* |
| | (0.045) | (0.044) | (0.045) | (0.044) | (0.044) | (0.046) | (0.036) | (0.054) |
| Unemp | | | | 0.022 | 0.021 | 0.027 | 0.015 | 0.044 |
| | | | | (0.087) | (0.086) | (0.085) | (0.072) | (0.109) |
| LaggedLiquidAssets | | | | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 |
| | | | | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| LaggedLTV | | | | | 0.139** | 0.133** | 0.027 | 0.115** |
| | | | | | (0.038) | (0.038) | (0.027) | (0.048) |
| MeanIncome | | | | | 0.000 | 0.000 | 0.000* | 0.000 |
| | | | | | (0.000) | (0.000) | (0.000) | (0.000) |
| Garnish | | | | | | 0.027 | 0.024 | 0.052 |
| | | | | | | (0.057) | (0.050) | (0.071) |
| Chap13Share | | | | | | 0.179** | -0.119* | 0.054 |
| | | | | | | (0.087) | (0.067) | (0.107) |
| Observations | 1245 | 1235 | 1240 | 1240 | 1239 | 1239 | 1237 | 1237 |
| Pseudo-R-Sq | 0.09 | 0.10 | 0.10 | 0.10 | 0.11 | 0.11 | 0.06 | 0.09 |

The dependent variable is whether the household withdrew housing equity. The Standard errors presented in parentheses are based on robust standard errors clustered by households. The variables *rrisk2-rrisk6* are the five categories of risk aversion with increasing degree of risk aversion; *rrisk1*, i.e., least risk averse, is the omitted category. * p<0.10, ** p<0.05

**Table 4: Marginal Effects from Probit Models of Whether Households Withdrew Housing Equity (Robustness to Covariates)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| LitPortRisk | -0.059** | -0.058** | -0.058** | -0.049* | -0.047* | -0.059** |
| | (0.023) | (0.023) | (0.023) | (0.028) | (0.028) | (0.023) |
| RefIncent | 0.048* | 0.045 | 0.045 | 0.035 | 0.014** | 0.017 |
| | (0.028) | (0.028) | (0.028) | (0.030) | (0.004) | (0.012) |
| HomeApprec | 0.072 | 0.067 | 0.068 | 0.115* | 0.111 | 0.138 |
| | (0.060) | (0.060) | (0.060) | (0.069) | (0.070) | (0.120) |
| Age | -0.005** | -0.003 | -0.003 | -0.002 | -0.004 | -0.005** |
| | (0.001) | (0.002) | (0.002) | (0.002) | (0.003) | (0.001) |
| HSchoolGrad | 0.034 | 0.032 | 0.031 | 0.043 | 0.044 | 0.029 |
| | (0.034) | (0.034) | (0.034) | (0.041) | (0.040) | (0.035) |
| SomeCollege | 0.042 | 0.035 | 0.034 | 0.029 | 0.030 | 0.033 |
| | (0.043) | (0.043) | (0.043) | (0.048) | (0.048) | (0.043) |
| CollegeGrad | 0.020 | 0.015 | 0.015 | -0.007 | -0.003 | 0.018 |
| | (0.039) | (0.039) | (0.039) | (0.043) | (0.043) | (0.041) |
| Male | 0.034 | 0.034 | 0.033 | 0.053** | 0.056** | 0.029 |
| | (0.022) | (0.022) | (0.021) | (0.025) | (0.025) | (0.022) |
| White | -0.078** | -0.082** | -0.083** | -0.084** | -0.084** | -0.065** |
| | (0.034) | (0.034) | (0.034) | (0.042) | (0.041) | (0.033) |
| NumChildren | 0.017** | 0.017** | 0.017** | 0.023** | 0.022** | 0.019** |
| | (0.005) | (0.005) | (0.005) | (0.007) | (0.007) | (0.006) |
| Married | 0.030 | 0.031 | 0.031 | 0.072 | 0.076 | 0.030 |
| | (0.026) | (0.025) | (0.025) | (0.039) | (0.037) | (0.026) |
| Unemp | 0.021 | 0.016 | 0.013 | -0.067 | -0.067 | 0.038 |
| | (0.086) | (0.084) | (0.083) | (0.067) | (0.065) | (0.094) |
| LaggedLiquidAssets | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |

| | | | | | | |
|---|---|---|---|---|---|---|
| LaggedLTV | 0.139** | 0.137** | 0.136** | 0.136** | 0.148** | 0.148** |
| | (0.038) | (0.038) | (0.038) | (0.045) | (0.041) | (0.037) |
| MeanIncome | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Rrisk2 | 0.026 | 0.026 | 0.027 | -0.021 | -0.022 | 0.002 |
| | (0.064) | (0.063) | (0.063) | (0.051) | (0.050) | (0.061) |
| Rrisk4 | -0.096** | -0.097** | -0.096** | -0.126** | -0.123** | -0.101** |
| | (0.032) | (0.031) | (0.032) | (0.029) | (0.028) | (0.031) |
| Rrisk4 | -0.044 | -0.046 | -0.046 | -0.087** | -0.086** | -0.060 |
| | (0.041) | (0.040) | (0.040) | (0.036) | (0.035) | (0.039) |
| Rrisk5 | -0.059 | -0.059 | -0.060 | -0.096** | -0.096** | -0.074* |
| | (0.040) | (0.039) | (0.039) | (0.036) | (0.035) | (0.038) |
| Rrisk6 | -0.070 | -0.072 | -0.071 | -0.123** | -0.122** | -0.090* |
| | (0.044) | (0.043) | (0.043) | (0.042) | (0.041) | (0.044) |
| year effects | No | No | No | No | Yes | Yes |
| state effects | No | No | No | No | No | Yes |
| cohort effects | No | Yes | Yes | Yes | Yes | No |
| retired status | No | No | Yes | Yes | Yes | No |
| health status | No | No | No | Yes | Yes | No |
| Observations | 1239 | 1239 | 1239 | 971 | 971 | 1203 |
| Pseudo-R-Sq | 0.11 | 0.11 | 0.11 | 0.11 | 0.12 | 0.13 |

Note: The dependent variable is whether the household withdrew housing equity as measured by whether or not mortgage debt increased. The Standard errors presented in parentheses are based on robust standard errors clustered by households. Refer to Table 2 and 3 for risk aversion categories. * p<0.10, ** p<0.05

**Table 5: Marginal Effects from Probit Models of Whether Households Withdrew Housing Equity (Robustness to Measures of Financial Literacy)**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Measure of Financial Literacy | LitPortRisk | LitCompound | LitMonIllus | # Correct | Fin Lit Index |
| Effect of Financial Literacy | -0.059** | -0.053* | -0.006 | -0.032** | -0.037** |
| | (0.023) | (0.029) | (0.030) | (0.014) | (0.019) |
| RefIncent | 0.017 | 0.018 | 0.015** | 0.016** | 0.015** |
| | (0.012) | (0.015) | (0.007) | (0.008) | (0.007) |
| HomeApprec | 0.138 | 0.122 | 0.100 | 0.118 | 0.115 |
| | (0.120) | (0.121) | (0.120) | (0.120) | (0.120) |
| Age | -0.005** | -0.006** | -0.006** | -0.006** | -0.006** |
| | (0.001) | (0.002) | (0.002) | (0.001) | (0.002) |
| HSchoolGrad | 0.029 | 0.012 | 0.015 | 0.016 | 0.014 |
| | (0.035) | (0.034) | (0.035) | (0.034) | (0.034) |
| SomeCollege | 0.033 | 0.023 | 0.023 | 0.030 | 0.028 |
| | (0.043) | (0.042) | (0.042) | (0.042) | (0.042) |
| CollegeGrad | 0.018 | 0.005 | 0.007 | 0.014 | 0.010 |
| | (0.041) | (0.040) | (0.040) | (0.040) | (0.040) |
| Male | 0.029 | 0.037 | 0.032 | 0.035 | 0.033 |
| | (0.022) | (0.023) | (0.023) | (0.022) | (0.022) |
| White | -0.065** | -0.060* | -0.072** | -0.056* | -0.060* |
| | (0.033) | (0.035) | (0.036) | (0.035) | (0.035) |
| NumChildren | 0.019** | 0.020** | 0.020** | 0.019** | 0.019** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| Married | 0.030 | 0.011 | 0.012 | 0.023 | 0.023 |
| | (0.026) | (0.029) | (0.028) | (0.027) | (0.027) |
| Unemp | 0.038 | 0.016 | 0.024 | 0.019 | 0.017 |
| | (0.094) | (0.084) | (0.088) | (0.086) | (0.085) |

| | | | | | |
|---|---|---|---|---|---|
| LaggedLiquidAssets | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| LaggedLTV | 0.148** | 0.158** | 0.157** | 0.153** | 0.153** |
| | (0.037) | (0.036) | (0.035) | (0.037) | (0.037) |
| MeanIncome | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Risk Aversion | Yes | Yes | Yes | Yes | Yes |
| year effects | Yes | Yes | Yes | Yes | Yes |
| state effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 1203 | 1206 | 1203 | 1198 | 1198 |
| Pseudo-R-Sq | 0.13 | 0.13 | 0.13 | 0.14 | 0.13 |

Note: The dependent variable is whether the household withdrew housing equity as measured by whether or not mortgage debt increased. The Standard errors presented in parentheses are based on robust standard errors clustered by households.

**Table 6: Instrumental Variable Estimates of Impact of Financial Literacy on Mortgage Equity Withdrawal (MEW) Propensity**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *A. Linear Instrumental Variables* | | | | | | |
| *Coefficient on LitPortRisk* | -0.002 | -0.369 | -0.392 | -0.398 | -0.359 | -0.361 |
| | (0.113) | (0.261) | (0.267) | (0.275) | (0.279) | (0.277) |
| *B. Hausman and Taylor Model* | | | | | | |
| *Coefficient LitPortRisk* | -0.821 | -1.082 | -1.292 | -0.912 | -0.429 | -0.181 |
| | (1.313) | (1.228) | (0.876) | (0.644) | (0.371) | (0.355) |
| *C. Marginal Effects from Bivariate Probit with Instruments* | | | | | | |
| *On Prob(LitPortRisk=1,MEW=1)* | -0.036 | -0.062 | -0.062 | -0.062 | -0.055 | -0.052 |
| | (0.048) | (0.008) | (0.008) | (0.0080) | (0.010) | (0.009) |
| *Controls* | | | | | | |
| Risk Aversion | No | Yes | Yes | Yes | Yes | Yes |
| year effects | No | No | No | No | No | Yes |
| cohort effects | No | No | Yes | Yes | Yes | Yes |
| retired status | No | No | No | Yes | Yes | Yes |
| health status | No | No | No | No | Yes | Yes |
| Observations | 2432 | 1239 | 1239 | 1239 | 971 | 971 |
| P-value on Overid Test | 0.066 | 0.215 | 0.269 | 0.283 | 0.148 | 0.152 |
| P-value on IV in first stage | 0.074 | 0.646 | 0.613 | 0.634 | 0.722 | 0.722 |
| P-value on Hausman Test | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |

Note: The dependent variable for linear IV and Hausman and Taylor models is a dummy for whether the household withdrew housing equity as measured by whether or not mortgage debt increased. Average state level high school graduation rate and parent's education categories were used as instruments in the Linear IV and Bivariate probit models. In the Hausman and Taylor means of other exogenous variables were used as instruments in addition to Average state level high school graduation rate and parent's education categories. In Bivariate Probit, the two equations estimated jointly are for financial literacy indicator and whether the household withdrew housing equity as measured by whether or not mortgage debt increased. The standard errors presented in parentheses are based on robust standard errors clustered by households.

**Table 7: Estimates of the impact of financial literacy on Mortgage Equity Withdrawal (MEW) propensity based on matching estimation methods**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| ATE | -0.040 | -0.019 | -0.006 | -0.023 | -0.055 | -0.023 |
| | (0.018) | (0.026) | (0.032) | (0.017) | (0.025) | (0.026) |
| ATT | -0.034 | 0.004 | 0.019 | -0.013 | -0.039 | 0.025 |
| | (0.022) | (0.033) | (0.041) | (0.019) | (0.029) | (0.028) |
| ATU | -0.047 | -0.053 | -0.048 | -0.034 | -0.080 | -0.11 |
| | (0.025) | (0.036) | (0.043) | (0.022) | (0.029) | (0.031) |
| *Controls* | | | | | | |
| Risk Aversion | No | Yes | Yes | Yes | Yes | Yes |
| Year effects | No | No | No | No | No | Yes |
| Cohort effects | No | No | Yes | Yes | Yes | Yes |
| Retired status | No | No | No | Yes | Yes | Yes |
| Health status | No | No | No | No | Yes | Yes |
| *N* | 2432 | 1239 | 971 | 2432 | 1239 | 971 |
| *Matching Method* | PS | PS | PS | NN | NN | NN |

Note: The numbers in the table refer to the estimates of the impact of financial literacy on mortgage equity withdrawal (MEW) propensity as measured by whether or not mortgage debt increased. ATE is overall average treatment effect of financial literacy. ATT is the impact of financial literacy on MEW of those who are literate. ATU is the impact of financial literacy on MEW of those who are financially illiterate. PS stands for propensity score matching. Standard errors presented in parenthesis are corrected for estimated propensity score by calculating bootstrapped standard errors clustered by respondents based on 100 replications. NN denotes nearest neighbor matching on full set of covariates using the bias-adjusted estimation method proposed in Abadie and Imbens (2006). All estimated effects in the table were obtained by computing the missing counterfactual using one nearest neighbor. Estimates using two, three, and four neighbors were very similar.