Home Equity and Labor Income: The Role of Constrained Mobility

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Abstract

Using detailed data for homeowners in the U.S., we document a negative, non-linear relation between the loan-to-value ratio (*LTV*) of the primary residence and labor income. Consistent with constrained mobility for high *LTV* individuals, we find stronger effects among subprime, liquidity constrained individuals and those living in regions with limited local alternate employment opportunities and strict non-compete law enforcement. Though high *LTV* individuals are less likely to move residences across MSAs, they are more likely to change jobs without changing their residence. We find no effects among similar neighboring renters employed at the same firm with similar job tenure.

Keywords: Home Equity, Mortgage Debt, Income, Labor Mobility, Debt Overhang

JEL Classification Numbers: D10, G21, J30, J62, J61, R20

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Introduction

The great recession and the subsequent slow recovery in wages have heightened interest in understanding if and how mortgage debt and house price changes affect labor market outcomes. One potential channel that links the mortgage market to the labor market runs through labor mobility. If a homeowner's equity is negative, i.e. if the mortgage debt outstanding is more than the value of the house, it can adversely affect labor mobility.[1] Reduced mobility can in turn lead to a higher likelihood of unemployment[2] or affect labor income for the employed individuals by reducing their bargaining power and the quality of match between an employee and employer. In this paper, we use administrative wage data matched to the credit profiles of millions of homeowners in the U.S. to document the effect of home equity on labor income among employed individuals. Our data also allows us to examine the channels through which any such effects operate.

An underwater homeowner facing the prospect of moving to accept a better job opportunity can do one of three things. She can sell her house and compensate the lender for any possible shortfall between the sale price (net of transaction costs) and the mortgage outstanding. Her ability to do so will depend on her access to liquidity and the extent to which she is credit constrained. Alternatively, she can retain her house and possibly rent it. This will affect her ability to make a down payment on a new house. She may also perceive some costs originating either from rental market frictions or from her preference for homeownership. Finally, she has the option to walk away from her house and default on the mortgage. Each of these options has some cost associated with it and depending on the severity of credit constraints, a homeowner may be willing to give up some attractive (out-of-region) employment opportunities to remain in her current residence. This constrained mobility may in turn affect the homeowner's incentives

[1] For instance, low home equity may constrain labor mobility owing to credit constraints (Stein [1995], Ortalo-Magne and Rady [2006]) or nominal loss aversion (Genesove and Mayer [2001], Engelhardt [2003], Annenberg [2011]). See also Paul Krugman in New York Times 2010, https://krugman.blogs.nytimes.com/2010/07/29/beveridge-worries/.

[2] See Paul Krugman in New York Times 2010, https://krugman.blogs.nytimes.com/2010/07/29/beveridge-worries/.

to search for opportunities in the first place and consequently her bargaining power with her employer, thereby adversely affecting her labor income.[3]

In addition to mobility, home equity may also affect labor income through a debt overhang channel. If a large fraction of a homeowner's income goes towards servicing mortgage debt then she may not have incentives to increase labor supply and seek better opportunities (e.g. Bernstein [2019], Donaldson et al. [2019]). Alternatively, low home equity may increase labor income if it provides additional incentives for the homeowner to reduce debt and avoid the possibility of a costly default (Lazear et al. [2016]). We refer to this as the incentive channel. We evaluate the merits of these alternate channels that relate home equity and labor income.

We use anonymized data from Equifax Inc., one of the three credit bureaus that is involved in the collection and transmission of data on the credit histories and employment of individuals within the U.S. The credit data includes anonymized information on the credit histories of all individuals in the U.S., including historical information on all their credit accounts, credit scores, and zip codes of residence. The employment data covers over 30 million employees across the U.S. from over 5,000 firms and contains granular information including employee's wages, bonus, commissions, job tenure, and firm level details. This is one of the first papers to use such detailed credit and employment data about the U.S. population.

We use the intersection of the credit and employment data to obtain a panel over the 72 month period between Jan 2010-Dec 2015. We conduct our analysis on a random sample of 300,000 individuals from our data who have an active mortgage as of January 1, 2010. These mortgages were originated sometime before January 1, 2010. We observe the employer-reported incomes in the employment data. We measure home equity as the loan-to-value ratio (*LTV*) on the primary residence, where *LTV* is the ratio of total mortgage loan outstanding over the imputed market value of the house. Since we expect *LTV* to have a non-linear effect on income, our main independent variables are a set of dummy variables that identify individuals with *LTV*s in different buckets. The construction and choice of these buckets are described in section

[3]Even if the house is not underwater, high loan-to-value ratio *(LTV)* can reduce the amount of capital available to finance the down payment for a new home, thereby locking the individual to her current residence.

2.

The two main sources of variation in *LTV*, changes in the amount of loan outstanding and changes in home values are both problematic to identify our effects. Loan outstanding can change both due to normal loan repayment – a function of loan maturity – and also due to prepayments or delayed payments.[4] All of these may be correlated with an individual's income. To overcome this, we follow Bernstein [2019] and instrument *LTV* with a synthetic loan-to-value ratio (*SLTV*), that is constructed under the assumption of uniform maturity and interest rate for all borrowers and no pre payment or delayed payments.[5] The exclusion restriction in our instrumental variables (IV) specification is that *SLTV* – which varies based on purchase cohort and house price changes since mortgage origination – affects labor income only through its effect on *LTV*. We discuss the validity of this assumption in section 2.

We proxy for changes in home values using a zip code level house price indexes. We control for local economic conditions (and hence the local labor market conditions) using zip code specific time fixed effects. We are able to do this because *SLTV* varies across individuals within a zip code based on when they bought their house (i.e., their purchase cohort). We also include within purchase cohort time fixed effects to control for average nationwide cohort effects at a particular point in time. Finally one could argue that local industry specific shocks could differentially affect the labor market outcomes and house prices of individuals in a zipcode belonging to different purchase cohorts. To evaluate this, we conduct a parallel placebo analysis using a sample of 'renters' who reside in the same zip code as our homeowners, work for the same firm, are similar in age and have similar levels of income, non-mortgage debt and job tenure. Our assumption is that the 'renters' should be subject to similar labor market shocks as the homeowners.

We find a strong negative, non-linear relation between *LTV* and income. Our IV estimates show that individuals with *LTV* between 1 and 1.5 earn 352.1 dollars lower monthly income relative to individuals with *LTV* between 0.3 and 0.4, our base case. This effect is economically

[4] We treat refinancing as closing of one loan account and the opening of another.

[5] The use of a synthetic mortgage instrument to tackle endogeneity problems associated with home equity goes back to Cunningham and Reed [2013] and has been adopted in different ways in other papers like Palmer [2015], Bernstein and Struyven [2017], Guren [2016].

significant as it corresponds to 5.1% of the sample mean. We also document slower income growth among individuals with high *LTV* values. Specifically, individuals with *LTV* between 1 and 1.5 experience 3.4 percentage points lower income growth (relative to the beginning of the sample) when compared to the base case. Thus our results are consistent with both the mobility and the debt-overhang channels and inconsistent with the incentive channel. We also find that individuals with *LTV* values between 0.8 and 1 earn lower income and experience slower income growth relative to individuals in the base case. This suggests that the negative effects of high *LTV* begin even at values below 1. According to the mobility channel, this can happen if the transaction costs associated with selling a house reduce ones ability to make down payment for a new house (Stein [1995], Genesove and Mayer [1997]).

We find that *LTV* does not significantly affect the labor income and income growth for the individuals in our placebo sample. Thus, a renter who resides in the same zip code, works for the same firm, with similar age, levels of income, non-mortgage debt and job tenure as a homeowner with *LTV* greater than 1 does not experience lower income and income growth as compared to a renter who is similar (on the above dimensions) to a homeowner with *LTV* between 0.3 and 0.4. The insignificant result suggests that unobserved local labor market shocks have a limited effect on our results.

To test the validity of the the mobility channel, we examine the effect of *LTV* on labor mobility. We measure mobility as instances when the individual moves residence (with or without a change in employer) from one MSA to another. We find that individuals with high *LTV* values are less likely to move. For instance, individuals with *LTV* values between 1 and 1.5 are 0.1 percentage points less likely to move in a month relative to individuals in the base case. This effect is economically large when compared to the mean likelihood of moving of 0.13% in a given month. Using credit scores and access to liquidity at the beginning of our sample as alternate measures of credit constraints, we find that the negative effect of *LTV* on labor mobility is stronger for borrowers with below median credit scores and for those with below median undrawn credit limit relative to the mortgage outstanding. We also find that the negative effect of high *LTV* on labor income is larger in absolute magnitude for borrowers with below median credit scores and for those with below median undrawn credit limit relative to the mortgage outstanding.

Constraints on geographic mobility are likely to prove less detrimental to labor income if the local MSA provides alternate opportunities in the individual's line of work. For example, an information technology (IT) professional residing in the San Francisco bay area can more easily find alternate employment without moving residence, whereas a similar individual residing in St Louis may find it difficult to do so. To test this conjecture, we differentiate the MSAs in our sample based on the availability of jobs in an individual's industry. The intuition is that it will be easier for an individual to shift to jobs within her industry than outside. Consistent with our conjecture, we find that the negative effect of high *LTV* on income is stronger for individuals living in MSAs that have below median level of industry specific jobs, i.e. below median level of the ratio of the number of the MSA's residents employed in the specific industry identified using the 3-digit NAICS code to the total number of employed residents in the MSA as of Jan 2010.

Strict enforcement of non-compete laws by states can also affect an individual's ability to find alternate employment in her MSA. Consistent with that, we find the negative relation between *LTV* and income to be stronger for individuals residing in states where non-compete laws are strictly enforced.

We next evaluate the importance of the debt overhang channel. An individual subject to debt overhang is likely to limit her labor supply and her efforts to improve her income till there is a reduction in her debt load. On the other hand, if an individual's income is adversely affected by constrained geographic mobility, the individual may show greater inclination to change jobs without changing residence in an effort to improve her income and compensate for the loss of out-of-region opportunities. Consistent with the mobility channel but inconsistent with the debt overhang channel, we find that individuals with high *LTV* values are more likely to change jobs without changing their residence. This effect is stronger among credit constrained individuals whose inter-MSA mobility is relatively more constrained. We also find this effect to be stronger for individuals who reside in MSAs with greater industry specific job opportunities and those residing in states where non-compete laws are not strictly enforced. Employing the number of hours worked (for hourly wage employees) and the extent of variable pay (for salaried employees) as measures of labor supply we also relate *LTV* to labor supply. Inconsistent with the debt overhang channel we do not find a significant relationship between *LTV* and these measures of labor supply.

Constrained mobility due to high *LTV* can depress both the income in the current job and the raise an employee gets when she changes jobs. The former can happen if constrained mobility reduces employee search effort for alternate employment and consequently her bargaining power while the latter can occur because of a constrained opportunity set.[6] We find that both these contribute to our baseline estimates. When we limit our sample to the time before an individual changes jobs for the first time and repeat our analysis we find that individuals with *LTV* between 1 and 1.5 earn 351.6 dollars lower monthly income relative to individuals with *LTV* between 0.3 and 0.4. We also find that high LTV individuals experience a 2 percentage point lower wage increase when they change jobs.

A potential limitation of our analysis is that we do not observe the house price at mortgage origination and hence the amount of down payment at origination. This can potentially introduce noise in our estimates if the difference between the true *LTV* and our imputed *LTV* is correlated with income. A number of features of our analysis help overcome this problem. First, our use of multi-dimensional fixed effects helps control for a number of channels through which this potential measurement error could affect our estimates. For example our within cohort time effects will control for any tendency of individuals within a purchase cohort to put lower down-payment – say due to a credit boom – to buy their house. Second, the results of our placebo tests ensure that the measurement error has a limited effect on our estimates. If the measurement error were correlated with house prices and income trends for specific set of individuals within the main sample, one would expect it to also affect the income trends of the corresponding renters. This is because both sets of individuals reside in the same zip code and are employed at the

[6]For instance, high LTV individuals who are not willing to move will likely look for opportunities within the region of their residence.

same firm and hence should be subject to similar economic conditions. Third, our use of non-parametric piecewise function instead of a linear function of *LTV* helps minimize the impact of noise on our estimates. For instance, if the true *LTV* is 1.26 while our imputed *LTV* is 1.15, this will not induce an error in our estimation because we will correctly assign the homeowner to the [1,1.5) bucket. This noise may however lead to misclassification errors if it pushes individuals into an incorrect *LTV* bucket (e.g. Schulhofer-Wohl, 2012).

We conduct a number of tests to further ensure that our inability to observe house prices at origination and the resulting misclassification error does not bias our conclusions. First, we compare our *LTV* measure to two different distributions in Equifax's Credit Risk Insight Servicing McDash (CRISM) data from Gerardi et al., 2018 where *LTV* is calculated based on actual origination *LTV* values. We find our *LTV* distribution matches well with the data from CRISM. Second, we conduct a number of tests where we repeat our baseline analysis using specifications where misclassification is likely to be smaller. Specifically, we re-estimate our baseline test by dropping the observations that are close to the cut-offs for our bins, considering only one dummy variable as the independent variable which identifies observations with $LTV > 0.8$, and considering only one dummy variable while dropping observations on the neighborhood of the cut-off (i.e. those with $LTV \in [0.7, 0.9]$). Across all specifications, we find results similar to our baseline estimates. Third, potentially, actual down payments could be higher in zip codes where the homeowner expects house prices to increase. To the extent such expectations depend on time and geography, they should vary at the zipcode-purchase cohort level. To control for this, we repeat our estimates after including within zip code purchase cohort fixed effects and find our results to be unaffected. Fourth, we repeat our analysis for the sub-sample of zip codes that have more homogeneous house prices (i.e. those with low within zip code standard deviation in house prices). We expect the measurement error in *LTV* to be smaller in these zip codes and find similar estimates for this sub-sample. Finally, we repeat our analysis with alternate assumptions about the *LTV* at origination and obtain consistent results.

Our results provide strong support to the conjecture that steep declines in house prices in the presence of a large amount of mortgage debt is likely to worsen the match between employees and employers, and affect employee productivity. Given the decline in house prices during the great recession, our estimates imply that constrained mobility owing to high *LTV* values can explain up to a 2.3% decline in wages. The negative spillovers that we document is of relevance to both policy makers and companies. Our results will help policy makers identify the geographies and the sub-populations that will be most constrained by low home equity. This can be used to design targeted policy interventions. Our results are also of relevance to firms interested in hiring and developing human talent as they show that credit constraints may affect an employee's willingness to move to take up job opportunities. If firms can relax such constraints, that may enhance labor mobility and consequently productivity.

1 Related Literature

A growing literature examines the relation between home equity and labor market outcomes. The most researched outcome is labor mobility. Theory predicts that lower home equity should constrain labor mobility, for instance owing to credit constraints (Stein [1995], Ortalo-Magne and Rady [2006]), nominal loss aversion (Genesove and Mayer [2001], Engelhardt [2003], Annenberg [2011]) or higher likelihood of defaults (Deng et al. [2000], Ghent and Kudlyak [2011], Molloy and Shan [2013]). However, there is mixed empirical evidence on the topic. For instance, while Henley [1998], Chan [2001], Ferreira et al. [2010, 2012], Goetz [2013], Kothari et al. [2013], Modestino and Dennett [2013] and Bernstein and Struyven [2017] document a positive relation between home equity and labor mobility, others find weak, null or opposite results (e.g. Aaronson and Davis [2011], Molloy et al. [2011], Schmitt and Warner [2011], Schulhofer-Wohl [2012], Farber [2012], Coulson and Grieco [2013], Mumford and Schultz [2013], Bricker and Bucks [2016], Demyanyk et al. [Forthcoming]).[7] Yet others document the effect of housing lock on unemployment

[7]Other related body of work studies different aspects of mobility and documents nuanced results. For instance, Donovan and Schnure [2011] find that negative equity reduces intra-county migration but leaves out-of-state migration unaffected, while Nenov [2012] document that negative equity reduces in-migration rates, but has no impact on out-migration. In addition, McCormick [1983], Head and Lloyd-Ellis [2012], Blanchflower and Oswald [2013]

(Karahan and Rhee [2013], Valletta [2013]), macroeconomic fluctuations (Sterk [2015]) and recovery from recession (Herkenhoff and Ohanian [2011]). Recent work by Brown and Matsa [2017] shows that individuals seeking employment and residing in areas with greater house price declines are more likely to apply for jobs within the region of their residence. We contribute to this literature by using detailed credit and income data for a large sample of U.S. residents to document the consequences of housing lock on homeowners' income. Precise employer-reported incomes allow us to document that employed, high *LTV* homeowners earn lower income. Importantly, the granularity of our data and the components of income that we observe (e.g. hourly workers, number of hours worked, variable pay etc) allow us to better evaluate and distinguish the economic mechanisms and identify the role of constrained mobility in reducing income for high *LTV* homeowners.

Closely related to our work, Cunningham and Reed [2013] use survey data from the American Housing Survey (AHS) to document a negative relation between *LTV* and income while Bernstein [2019] uses bank account data to infer income and document that negative home equity leads to reduced labor supply owing to debt overhang. In contrast, we use detailed employer reported data and do not find evidence of the debt overhang channel for the sample of employed individuals as we observe no change in labor supply either for hourly or salaried workers. Instead, our results suggest that individuals with negative home equity experience declines in income owing to constrained mobility likely because it reduces employee bargaining power or worsens the match between employees and employers thus affecting employee productivity.

Our paper also relates to the broader literature that investigates the effect of leverage on different aspects of household decision making and the economy. For instance, prior studies have examined the effect of extreme leverage on entrepreneurial activity (Adelino et al. [2015]), employment opportunities (Mian and Sufi [2014], Bos et al. [2015]), household consumption and investment decisions (Bhutta et al. [2010], Foote et al. [2008], Fuster and Willen [2013], Guiso

argue that even outside of home equity, homeownership could interfere with the labor market by reducing workers' mobility owing to transferring costs associated with transactions.

et al. [2013], Mian et al. [2013]), and the real economy (Mian and Sufi [2011], Mian et al. [2015]). Melzer [Forthcoming] finds that households with negative home equity reduce investments in their house, since they anticipate not to be residual claimants any more. Using administrative data from home affordable modification programs, Scharlemann and Shore [2016] find that individuals with negative home equity are more likely to default on their mortgage. We contribute to this literature by highlighting a new dimension of the consequences of the spillover effects of home equity on the labor market.

2 Empirical Methodology

To evaluate the effect of home equity on labor income, we begin by estimating variants of the following model:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \le LTV_{it-1} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt} \quad (1)$$

where the dependent variable y_{iczt} is a measure of income or mobility for individual i residing in zip code z during year-month t, and belonging to purchase cohort c based on when she bought her house. Our primary measures of income include the level of income in dollars, logarithm of income and percentage change in income relative to the income both at the beginning of the sample (i.e. income growth) and one year before. Our measure of mobility is the dummy variable *Mobility*, which takes a value of one in year-month t if the MSA associated with individual i's primary residence in month t is different from her MSA in month $t-1$.

The main independent variables in our analysis are the indicator functions $\left\{1_{\{l_k \le LTV_{it-1} < h_k\}}\right\}$ which equal one when individual i's loan-to-value ratio (LTV) at the end of year-month $t-1$ is between l_k and h_k - i.e., $LTV_{it-1} \in [l_k, h_k)$.[8] Before we describe the construction of the indicator functions, we describe our calculation of LTV for which we use the imputation method described in Bernstein [2019]. While we observe the exact loan amount outstanding at any point in time

[8]The loan-to-value ratio is computed on the individual's primary residence as reported in our credit data.

and changes in house prices at the zip code level, we do not observe individual home values at the time of initial purchase (or refinancing). Hence we make some simplifying assumptions to calculate *LTV*. Hereinafter we refer to the month of mortgage origination or refinance as the month of origination.

The *LTV* we calculate at the time of origination depends on the number of mortgages an individual originates. If an individual originates a single mortgage in a month, we assume the *LTV* at origination to be 0.8. On the other hand, if the individual originates multiple mortgages in a month, we assume the origination *LTV* on the largest mortgage to be 0.8 and calculate the total *LTV* based on the total amount borrowed on all of the mortgages. This assumption is based on the common industry practice to cap the *LTV* on the primary mortgage at 0.8 in order to comply with GSE (Fannie Mae/Freddie Mac) guidelines. In cases where the borrower requires more than 80% financing, the lender supplements the first mortgage with a second mortgage.

We assume that the percentage change in house price is the same for all houses within a zip code. Therefore, we calculate *LTV* at any point in time t as the ratio of the actual loan amount over changes in zip code level house price index:

$$LTV_{it} = LTV_0 \times \frac{(1 + \%\Delta Loan_{it})}{(1 + \%\Delta HPI_{zt})}, \tag{2}$$

where LTV_0 is the *LTV* at loan origination, $\%\Delta Loan_{it}$ is the percentage change in loan amount outstanding since origination, and $\%\Delta HPI_{zt}$ is the percentage change in the zip code level house price index since mortgage origination. Note that there are two possible sources of measurement error in our imputed *LTV* measure. First, the initial *LTV* may be different from what we assume it to be. Second, the actual change in the value of a home can be more or less than that based on the zip code level house price change. We discuss the ways in which we address these concerns in section 5.2.

We divide the range of *LTV*s in our sample into six non-overlapping buckets: $[0, 0.3)$, $[0.3, 0.4)$, $[0.4, 0.8)$, $[0.8, 1)$, $[1, 1.5)$, and (≥ 1.5). We include indicator functions to represent these buckets excluding the $[0.3, 0.4)$ bucket, the base case. The coefficient β_k in Equation 1 is a measure of the difference in the average outcome variable for individuals with LTV between l_k and h_k as compared to the base case. We employ dummy variables instead of a linear term in LTV to identify any possible non-linear effect of LTV on the outcome variables without imposing any functional form restriction, especially around $LTV = 1$. Our use of dummy variables will also greatly diminish any bias due to measurement error in *LTV*.

The choice of the buckets is based both on having the cut-offs at round numbers and our assessment as to where one might expect the relation between LTV and our outcome variables to structurally change. For instance, we are especially interested to know how labor income is affected when mortgages are underwater, i.e. when $LTV > 1$. However, we suspect that the effect of LTV on the outcome variables may change if the mortgage is deep underwater. In such cases, the individual may have an incentive to strategically default on her mortgage and this may affect her income and mobility. Hence, we use buckets $[1, 1.5)$ and (≥ 1.5) to separately examine the effect for underwater and deep underwater mortgages on income and mobility. Similarly, LTV may end up being a binding constraint for individuals looking to sell their house even if it is below 1. This can happen if diminished home equity constrains their ability to make a down-payment on a new home (Stein [1995], Genesove and Mayer [1997]). Furthermore, transaction costs associated with selling a property may also effectively make the mortgage underwater even if the LTV is below 1. We use the bucket $[0.8, 1)$ to evaluate the effect of LTV on income and mobility for such mortgages. To ensure that our results are not due to our choice of buckets, we repeat our baseline analysis with equal sized buckets with a spread of 0.1.

We include a robust set of controls in our specification. First, we include individual fixed effects (δ_i) to control for individual-level, time-invariant characteristics. Second, we include zip code specific time effects (δ_{zt}) to account for time-varying local economic conditions that could affect both LTV and labor income. For example, adverse local economic conditions may simultaneously decrease home values (thus increase LTVs) and labor income. Third, we include purchase cohort specific time effects (δ_{ct}) to control for time-varying life cycle and cohort effects.

For instance, individuals who just purchased a home in a new neighborhood may be in the early stages of their career and hence earn less income and also (mechanically) have higher *LTV*s. Together δ_{ct} and δ_{zt} control for the average level of the outcome variable within a purchase cohort and a zip code at a particular point in time respectively. Finally, we include a quadratic term in job tenure and age $(X_{i,t-1})$ to account for time-varying individual-level factors that could affect their income.

Two main factors drive the variation in LTV_{it} in our sample: the outstanding loan amount and the change in zip code level house price index since mortgage origination. These factors have a multiplicative effect, which ensures that we have variation in *LTV* across individuals within the same zip code as well as variation in *LTV* across individuals within the same purchase cohort.

Outstanding loan amounts can change either from scheduled loan repayments over time – a function of loan maturity – or from partial prepayments or delayed payments. All of these choices could be related to an individual's income. For example, an individual who experiences an increase in pay, may choose to use the windfall to partially pre-pay her mortgage. Similarly individuals that experience a negative shock to their income may be late in their mortgage payments and this could affect the loan outstanding and consequently *LTV*. To ensure such endogenous changes in loan amounts do not bias our conclusions, we isolate the variation in *LTV* due to changes in the regional house price index since mortgage origination.

Specifically, we follow Bernstein [2019] and instrument *LTV* with a synthetic loan-to-value ratio $(SLTV)$. We calculate *SLTV* by assuming a uniform loan maturity and interest rate across our sample. We assume the monthly loan payments to be equal to those that would arise under a 30-year mortgage with a fixed interest rate and no prepayment. The synthetic change in loan amount every month is given by:

$$\%\Delta SynthLoan_{ct} = -\frac{(1+r)^{t-c} - 1}{(1+r)^{360} - 1}, \tag{3}$$

where r is the mortgage interest rate. For our baseline estimates, we assume that the mortgage interest rate is 6.75% - the median mortgage interest rate in the small sub-sample for which we know the interest rates. Finally, using the synthetic loan amount, we calculate *SLTV* as:

$$SLTV_{it} = LTV_0 \times \frac{(1 + \%\Delta SynthLoan_{ct})}{(1 + \%\Delta HPI_{zct})} \tag{4}$$

We employ *SLTV* as an instrument for *LTV* in the following instrumental variables (IV) regression:

$$1_{\{l_k \leq LTV_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{c(i)t} + \sum_k \theta_k \times 1_{\{l_k \leq SLTV_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt} \quad \forall k$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{c(i)t} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{LTV_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}, \tag{5}$$

where we use the *SLTV* bucket indicator function, $1_{\{l_k \leq SLTV_{it-1} < h_k\}}$, as an instrument for the corresponding *LTV* bucket indicator function. The difference between *SLTV* and *LTV* is that *SLTV* only uses changes in loan amounts that are a function of the time since the house was purchased (or refinanced), i.e. the purchase cohort to which the individual belongs. Hence *SLTV* depends on the time since mortgage origination – which will affect the loan outstanding – and house price changes since origination, and varies at the purchase cohort x zipcode x time level. This allows us to include both purchase cohort x time and zipcode x time fixed effects to control for cohort effects and zipcode level economic conditions.

Our exclusion restriction requires that after controlling for time varying characteristics at the zipcode level, time varying cohort effects, individual level time invariant characteristics, age and job tenure, *SLTV* affects labor income only through it's effect on LTV. As mentioned before *SLTV* changes with house price since origination and time since origination. These generate variation at the zipcode x cohort x time level. Our exclusion restriction could be violated by a local shock that both differentially affects the labor income of individuals belonging to different purchase cohorts as well as house prices. We conduct a placebo test using a population of renters to rule out such omitted variables. We discuss this further in section 4.1.

3 Data

3.1 Sample Construction

Our empirical analysis leverages anonymized data on individual credit profiles and employment information from Equifax Inc., one of the three major credit bureaus. The anonymized credit data contains information on the credit histories for all individuals (with a credit history) in the U.S. for the period 2010-2015. This includes anonymous information on historical credit scores along with disaggregated individual credit-account level information such as account type (e.g. credit card, home loan, etc.), borrower location, account age, total borrowing, account balance, and any missed or late payments. The employment data covers millions of individuals from more than 5,000 employers in the U.S. and includes anonymous information on each employee's wages, salary, bonus, average hours worked, job tenure, firm level details, and whether the employee remains employed at the firm at a given point in time. Kalda [2019] provides details on the representativeness of the employment data. This is one of the first papers to use such detailed credit and employment data on the U.S. population.

We merge the two datasets to obtain a panel with credit and employment information over the 72 month period between 2010-2015. We restrict the panel to homeowners with an active mortgage loan as of January 1, 2010. Note that these mortgages were originated sometime before January 1, 2010. While the earliest mortgage in our sample was originated in 1976, most of the mortgages were originated during the boom years of 2002-06. To make the computations feasible, we draw a random sample of 300,000 individuals from this sample to conduct our analysis. We allow individuals to drop out of our sample if they are no more employed with a firm included in our employment data.[9]

We retain individuals in our sample until the first time they move their residence. Thus, if an individual changes the zip code of her residence for the first time in January 2012, with or without the closure of the corresponding mortgage account, she is dropped from the sample

[9] We discuss the implications of this sample selection for our analysis later in section 5.1.

starting January 2012. This is because once an individual changes residence, they internalize the cost of (high) *LTV* of their previous residence and are no longer affected by it. Also note that we include the first month's income after the move in our sample to make sure the pay differential is captured by our estimates. Refinancing is reflected in our data by the closing of one account and the opening of a new account. In such instances, we retain the old account up until the month before its closure and then switch to the new account with a beginning *LTV* calculated using the procedure detailed in the previous section.[10]

The zip code level house price data we use comes from Corelogic and covers the period 1976-2015. Specifically, we use Corelogic's monthly house price indices (HPI) to impute changes in home values at the zip code level. These indices are calculated using a weighted repeat sales methodology and are normalized by setting the index value as of January, 2010 to 100.

We make note of two issues with our sample that may potentially bias our estimates. First, our sample is confined to the individuals in the intersection of the credit and employment data. Thus, our sample may not be representative of the population of mortgage borrowers in the U.S. as the employment data is not comprehensive and consists of individuals employed at the 5,000 firms that Equifax obtains data from. The firms in our sample are larger than the average firm in the U.S. with a median firm employing over 1,100 individuals. However, the income distribution is representative of the U.S. workforce. For instance, the median individual in the data is 41 years old with an annual salaried income of $41,015. This is comparable to the U.S. workforce where the median individual with full-time employment is 41.9 years of age, is salaried, and earns an income of $41,392. Second, our sample may be subject to a survivorship bias. Recall that we focus on individuals who are current on their mortgage as of January, 2010. Depending on when they bought their house, these individuals may have gone through the crisis without defaulting on their mortgage even if their house was underwater. Thus, on average, the individuals in our sample may have a lower propensity to default on their mortgage.

[10]Since the individual is likely to internalize the high *LTV* when she refinances her house, we conduct robustness tests after dropping individuals when they refinance their house. We present the results in Table IA1 of the Internet Appendix (IA)

3.2 Sample Description & Statistics

Figure 1 compares the distribution of individuals in our sample across states in the U.S. to the same distribution of the entire population (as of 2010) based on the location of an individual's residence. The numbers in the figure represent the percentage difference in this distribution, i.e. $[\frac{StatePopulation}{TotalPopulation}]_{Sample} - [\frac{StatePopulation}{TotalPopulation}]_{Census}$. The distribution of employees in our sample is comparable to the distribution of the U.S. population for most states. The residual differences arise from Nevada, Colorado, Nebraska, Missouri and Minnesota being over represented in our sample and Montana, Wyoming, Vermont and West Virginia being under-represented.

Table 1 reports summary statistics for the key variables that we use in our analysis. We have a total of 14,031,645 individual-month observations. The top panel reports summary statistics for our outcome variables. The mean monthly income in our sample is \$6,927 while the median is \$5,513. The mean increase in income is 10.1% relative to January of 2010. The mean probability that an individual moves MSAs in our sample is 0.13% per month.

The bottom panel of Table 1 summarizes our independent variables. The mean (median) loan size in our sample is \$192,400 (\$112,100). Loan size is right skewed and has a maximum value of over \$695,000. We impute the purchase price based on our calculation of LTV at origination. The average loan balance in our sample is \$161,600, approximately 84% of the original loan amount.

The mean (median) LTV and $SLTV$ in our sample are 0.7 (0.8) and 0.8 (0.8), respectively. The summary statistics for the indicator functions show that 89% of the observations in our sample have LTV between 0 and 1. Of the individuals with LTV between 0 and 1, 4% have LTV less than 0.3, 11% have LTV between 0.3 and 0.4, 54% have LTV between 0.4 and 0.8 while 20% have LTV between 0.8 and 1. About 11% of our observations have LTV greater than 1.

Figure 2 displays the density plot for the number of loan originations across time. Consistent with the spike in mortgage originations in the early 2000s, most individuals in our sample originate loans between 2002-2006. Hence, the individuals in our sample are likely to have experienced a decline in house prices during the Great Recession.

Panel A of Figure 3 plots the distribution of monthly house price changes between 2001-

2015. Most monthly house price changes at the zip code level are within the range of -2.5% to 2.5%. These changes, when accumulated over several months can amount to large differences in house prices. Panel B illustrates this idea by plotting the density of annual house price changes. Annual house price changes range from -20% to +20% between 2001-2015. Combined, these plots highlight the significant variation in house prices in our sample. This is likely to generate large variation in *LTV*s that will help identify our effects.

4 Empirical Results

4.1 Home Equity & Labor Income

We begin our empirical analysis by estimating Equation 1 and present the results in Table 2. The dependent variable in column (1) is the level of income measured in dollars (*Income ($)*). The positive and significant coefficient on $1_{\{0 \leq LTV < 0.3\}}$ in column (1) indicates that individuals with $LTV \in [0, 0.3)$ earn 66.5 dollars higher monthly income as compared to individuals with $LTV \in [0.3, 0.4)$, our base case. We find that individuals with $LTV \in [0.4, 0.8)$ earn similar income as the base case. However, individuals with *LTV* greater than 0.8 earn lower income than our base case individuals. Specifically, those with $LTV \in [0.8, 1)$ earn 83.2 dollars less. The magnitude of this effect increases with *LTV* values as individuals with $LTV \in [1, 1.5)$ earn 129.3 dollars less than the base case. This effect is also economically significant as it corresponds to 1.8% of the mean income in the sample. Interestingly, we find that individuals with *LTV* greater than 1.5 do not earn statistically different income than the base case. Overall the results indicate a negative relationship between *LTV* and income especially when *LTV* increases beyond 0.8.

In column (2) we repeat our analysis with logarithm of income as the dependent variable. The coefficients on $1_{\{0 \leq LTV < 0.3\}}$ and $1_{\{0.4 \leq LTV < 0.8\}}$ are both insignificant suggesting that the monthly income of individuals with $LTV \in [0, 0.3)$ and $LTV \in [0.4, 0.8)$ is not statistically different from those with $LTV \in [0.3, 0.4)$. However, similar to column (1) we find that individuals with $LTV \in [0.8, 1)$ and $LTV \in [1, 1.5)$ earn less income as compared to the base case. Specifically, individuals with $LTV \in [0.8, 1)$ earn 0.2 percentage points lower income while individuals with $LTV \in [1, 1.5)$ earn 0.8 percentage points lower income. In contrast to column (1), we find that individuals with *LTV* greater than 1.5 also earn 0.3 percentage points less income than the base case.

In columns (3) and (4) we repeat our tests with income growth as the outcome variable. Specifically, we model the percentage change in income relative to January of 2010 in column (3) and 12-month log change in income in column (4). Similar to column (2), we find that income growth of individuals with $LTV \in [0, 0.3)$ and $LTV \in [0.4, 0.8)$ is statistically indistinguishable from the income growth of individuals in the base case. However, based on estimates in column (3), individuals with $LTV \in [0.8, 1)$ experience 0.2 percentage points lower income growth while individuals with $LTV \in [1, 1.5)$ experience 1 percentage point lower income growth relative to the base case. These magnitudes are economically significant as they correspond to 2% and 10% respectively of the mean income growth in the sample. Here again we find that individuals with *LTV* values greater than 1.5 experience lower income growth. Estimates in column (4) paint a similar picture.

In columns (5) - (8) we repeat our analysis for the placebo sample which we construct as follows. For every homeowner in the main sample, we identify an individual who as of Jan 2010 resides in the same zip code, works for the same firm, is similar in age, and has similar levels of income, non-mortgage debt and tenure at the firm but does not have an open mortgage account. We refer to these individuals as renters. We then attribute the homeowner's *LTV* to the renter. The variation in *LTV* for the renters is driven by house price changes at the zip code level and any repayments and/or refinancing by the homeowner. Since the renters live in the same area and work for the same firm with similar tenure as the homeowner, their labor income should be subject to similar economic shocks. If our prior results are due to unobserved economic conditions affecting both house prices and labor income, or are driven by correlations between measurement error and income trends, then that should play out in the renter sample as well. Since we are not able to find a matched renter for every homeowner, our placebo sample is smaller than the main homeowner sample. We find that there is no significant relationship between *LTV* and labor income in the placebo renter sample. This assures us that correlated unobserved economic conditions or measurement error may have a limited effect on our results.

In Table 3, we present the results of the reduced form estimation wherein we include the indicator functions for *SLTV* instead of *LTV*. In column (1) we use the level of income as our outcome variable. Similar to the results in column (1) of Table 2, we find that individuals with $SLTV \in [0, 0.3)$ earn slightly higher income while individuals with $SLTV \in [0.4, 0.8)$ earn similar income as compared to individuals with $SLTV \in [0.3, 0.4)$. However, we find bigger effects for individuals with *SLTV* values greater than 0.8. Specifically, individuals with $SLTV \in [0.8, 1)$ earn 130.9 dollars (1.9% of the sample mean) less while individuals with $SLTV \in [1, 1.5)$ earn 189.6 dollars (2.7% of the sample mean) less as compared to individuals in the base case. We find consistent results when we employ logarithm of income (column (2)), growth in income (column (3)) and 12-month log change in income (column (4)) as the outcome variables. For instance, individuals with $SLTV \in [0.8, 1)$ experience 1.2 percentage points (12% of the sample mean) lower income growth while individuals with $SLTV \in [1, 1.5)$ experience 1.9 percentage points (19% of the sample mean) lower income growth when compared to the base case. In columns (5) - (8) we repeat our analysis for the placebo sample and find no significant relation between *SLTV* of the renters sample and labor income.

In Table 4 we present the results of the IV regression described in Equation 5. We have six first stage regressions, one for each *LTV* bucket indicator. In Panel A of Table 4 we provide the coefficients along with F-statistic for each of the first stage regressions. We find that all the instruments are strong with the F-statistics being significantly greater than the threshold of 10 (Bound et al. [1995], Staiger and Stock [1997]).

Panel B reports the coefficients for the second stage. The results in column (1) show that consistent with our OLS results, labor income decreases with home *LTV*. Comparing the magnitude of our coefficient estimates between the OLS and IV specifications, we find that our IV estimates are much larger than our OLS estimates. For example our IV estimates indicate that individuals with $LTV \in [0.8, 1)$ earn 263.7 dollars less income and those with $LTV \in [1, 1.5)$ earn 352.1 dollars less income as compared to individuals with $LTV \in [0.3, 0.4)$. Both these magnitudes are economically significant as they correspond to 3.8% and 5.1% of the sample mean respectively. The IV estimate for individuals with *LTV* values greater than 1.5 is also statistically significant and shows that these individuals earn 178.6 dollars less than individuals in the omitted category. Thus the endogeneity of loan amounts appears to generate a positive association between *LTV* and labor income.

In column (2) we focus on logarithm of income. Here again our IV estimates are larger than the corresponding OLS estimates and provide evidence of lower income among individuals with high *LTV*. Finally in columns (3) and (4) we focus on income growth and find that individuals with high *LTV* experience slower income growth as compared to individuals with $LTV \in [0.3, 0.4)$. Similar to the first two columns, our IV estimates in columns (3) and (4) are larger in absolute magnitude than the OLS estimates. For example from column (3) we find that individuals with $LTV \in [0.8, 1)$ experience 2.4 percentage points slower income growth while those with $LTV \in [1, 1.5)$ experience 3.4 percentage points slower income growth as compared to our base case. Finally, we find that individuals with *LTV* values greater than 1.5 experience 2.4 percentage points slower growth in income.

Possible downward bias in our OLS estimates could explain the difference between our IV and OLS estimates. There are three main factors that drive a wedge between *LTV* and *SLTV*: partial pre-payment of the mortgage, having a mortgage tenure less than 30 years (say 15 years) and late payments. While partial pre-payment and shorter tenure (hereinafter we refer to these combined as pre-payment) is likely to depress *LTV* relative to *SLTV*, late payments is likely to increase *LTV* relative to *SLTV*. Our summary statistics (see Table 1) indicate that *LTV* is on average lower than *SLTV* which indicates that pre-payments dominate late payments in our sample. We find a negative correlation between the likelihood of pre-payment and labor income (unreported) which may be driven by low income individuals chosing shorter term mortgages. This could contribute to the downward bias in our OLS estimation.

In columns (5) - (8) we repeat our IV analysis within the placebo sample of renters. We find no significant relationship between *LTV* and income and income growth in the renters sample. This provides strong evidence that unobserved labor market shocks and measurement error may have a limited effect on our results.

To ensure that our results are not sensitive to the specific *LTV* buckets we pick, we repeat our tests with dummies to indicate 16 different *LTV* buckets instead of the six we have in the Tables. We construct these buckets as follows. We divide the *LTV* values in our sample into 16 different buckets of which 15 buckets are of 0.1 width each. Since the number of observations with *LTV* greater than 1.5 are small, we combine all these observations into one bucket - $LTV \in [>= 1.5)$. As before, the omitted category is the bucket with $LTV \in (0.3, 0.4]$. Figure 4 plots the results with these sixteen *LTV* bucket indicators.

In Panel A of Figure 4 we model *Income ($)* and plot the coefficient estimates along with confidence intervals (CI) at 95% level. The estimates suggest that income for individuals with *LTV* less than 0.3, and *LTV* between 0.4 and 0.8 is not statistically different from income for individuals with $LTV \in (0.3, 0.4]$. However, the coefficients progressively go down with *LTV* and our results indicate that income for individuals with *LTV* values greater than 0.8 is lower than income for the base case. In Panel B, we model *Income ($)* for the placebo sample and find no significant relationship between income and *LTV*.

Our exclusion restriction may be violated by shocks that affect both house prices and the income of individuals that belong to different cohorts and reside in different regions in a differential manner. For example, regional (industry) booms that induce (migration and) home purchases in specific areas during specific time periods and differentially affect future income of new migrants and incumbents many years later may bias our estimates. A number of factors relating to our analysis help assuage this concern. First, if our results are driven by individuals moving to a particular zipcode in response to a regional boom then it should affect the individuals in our placebo sample as well. This is because the individuals in our placebo sample are at the similar stage in terms of life cycle (age) and career trajectory (tenure), as our main sample.

However, we find an insignificant relationship between *LTV* and income in our placebo sample. Second, we re-estimate our baseline tests by including MSA x cohort x time fixed effects along with zipcode x time fixed effects. The former are likely to control for MSA level timevarying economic shocks. Table IA2 reports these results where we find similar estimates to our baseline. Finally, we also estimate our test by including zipcode x industry x time fixed effects along with cohort x time fixed effects. We define industry at the 3-digit NAICS code level. These additional fixed effects should control for local industry shocks. We again find our results to be robust to this specification. The results of these tests are discussed in detail in section 5.4. Overall, these results lend support to our exclusion restriction and suggest that specific regional shocks likely don't drive our estimates.Home Equity & Labor Mobility

In this section, we examine the effect of *LTV* on labor mobility and how this effect interacts with an individual's credit constraints. The results are reported in Table 5. The dependent variable in this analysis is *Mobility* which is a dummy variable that takes a value of one in year-month t if the MSA associated with individual i's primary residence in month t is different from their MSA in month $t-1$. Although we include the full set of *LTV* indicator variables, we only report the coefficients on $1_{\{0.8 \leq LTV_{it-1} < 1\}}$ and $1_{\{1 \leq LTV_{it-1} < 1.5\}}$ so as to report all the cross-sectional results. In column (1), we find that individuals with *LTV* values between 0.8 and 1.5 are less likely to move than individuals with $LTV \in [0.3, 0.4)$. Specifically, individuals with both $LTV \in [0.8, 1)$ and $LTV \in [1, 1.5)$ are 0.1 percentage points less likely to move in a month. These effects are economically large when compared to the mean likelihood of moving in a month of 0.13% in our sample.

In columns (2) and (3) we examine the role of credit constraints on the effect of *LTV* on mobility. We use two measures of credit constrains. In column (2) we differentiate borrowers based on their access to liquidity measured using the aggregate amount of undrawn credit limits in their card accounts. We classify borrowers with above (below) median undrawn limits as a proportion of mortgage outstanding as of Jan 2010 as having *Above* (*Below*) median access to liquidity. We expect *LTV* to especially affect the mobility of borrowers with less access to liquidity. We perform our cross-sectional tests by including interaction terms between the indicator variables that identify *LTV* buckets, and *Above* and *Below* dummy variables. Here again we only report coefficients on the interaction terms with $1_{\{0.8 \leq LTV < 1\}}$ and $1_{\{1 \leq LTV < 1.5\}}$ for brevity. We find that while higher *LTV* lowers mobility for borrowers with *Below* median access to liquidity, it does not affect the mobility of borrowers with *Above* median access to liquidity. The differences between estimates for interaction terms with *Above* and *Below* are statistically significant and economically meaningful when compared to the mean mobility of 0.13% in the sample.

In column (3) we classify borrowers as having *Above* (*Below*) median levels of credit score as of Jan 2010. We expect borrowers with below median credit scores to face greater credit constraints as compared to borrowers with better credit scores. If *LTV* affects labor mobility because of credit constraints then we expect this effect to be stronger for individuals with below median credit scores. Consistent with this, we find that the effect of *LTV* on labor mobility is indeed greater for individuals with below median credit scores. As before, the coefficients on the interaction terms involving *Above* are statistically different from those involving *Below*.

In columns (4) - (6) we examine the effect of *LTV* on mobility for the placebo renters sample. We find no significant relationship between *LTV* and mobility in the sample of renters. This provides assurance that the relationship between *LTV* and mobility that we unearth may not be due to unobserved local economic conditions or measurement error.

As before, to alleviate the concern that our results may be sensitive to the specific *LTV* buckets we use, we implement a specification wherein we include dummies to indicate 16 *LTV* buckets. We construct these buckets in the same manner as in Figure 4. In Panel A of Figure 5 we model *Mobility* for our main sample and present the coefficient estimates along with the 95% confidence intervals (CI). The estimates suggest that mobility of individuals with *LTV* less than 0.8 is not statistically different from that for individuals with $LTV \in (0.3, 0.4]$. However, the coefficients progressively go down with *LTV*, and show that individuals with *LTV* greater than 0.8 are less likely to move than individuals in the base case. In Panel B, we plot the estimates for the placebo sample and find no significant relationship between *LTV* and mobility.

4.2 Home Equity & Labor Income : the Mobility Channel

In this section, we evaluate the merits of the 'mobility' channel by testing for possible heterogeneity in the relation between *LTV* and labor income.

4.2.1 Heterogeneity by Credit Constraints

In the previous section, we find that high *LTV* is associated with a decline in mobility and this effect is stronger among credit constrained individuals. If this stifled mobility drives the negative relation between high *LTV* and income, we expect to see a larger decline in income for credit constrained individuals with high *LTV*. We evaluate this in Table 6.

In Panel A we classify homeowners with above (below) median undrawn limits as a proportion of mortgage outstanding as of Jan 2010 as having *Above* (*Below*) median access to liquidity. The outcome variable in column (1) is the level of income measured in dollars (*Income ($)*). Although we include interaction terms with the full set of *LTV* indicator variables and *Above* and *Below*, we only report the coefficients on the interaction terms with $1_{\{0.8 \leq LTV < 1\}}$, $1_{\{1 \leq LTV < 1.5\}}$ and $1_{\{LTV \geq 1.5\}}$ for brevity. We find that the negative effect of *LTV* on labor income is stronger for individuals with below median access to liquidity. We find that the coefficients on the interaction terms involving *Below* are statistically different from those involving *Above*. In column (2) we model logarithm of income and find similar results. In column (3) we focus on income growth and again find that *LTV* has a larger effect on income growth for individuals with below median access to liquidity.

In Panel B of Table 6 we differentiate borrowers based on their credit scores as of Jan 2010. From column (1) we find that while higher *LTV* lowers income for borrowers with *Below* median credit score, it does not affect the income of borrowers with *Above* median credit score. The coefficients are also statistically different across interaction terms involving *Above* and *Below*. In column (2) we model logarithm of income and find similar results. In column (3) we focus on income growth and again find that *LTV* has a statistically stronger effect on income growth for individuals with below median credit score as compared to individuals with above median credit score.

In columns (4) - (6) of both Panels A and B we examine the heterogeneity in the relation between *LTV* and income for the placebo sample. We find no significant effect of *LTV* on income for individuals with different levels of credit constraints among the sample of renters.

4.2.2 Heterogeneity by Market Conditions & Non-Compete Laws

If the negative association between *LTV* and labor income that we uncover is due to constrained mobility, then it should be especially stronger for individuals who reside in areas with fewer alternate job opportunities. Individuals who reside in areas with more job opportunities are more likely to be able to change jobs without changing residence. For such individuals the negative association should be muted. We evaluate this hypothesis by examining the heterogeneity in the relation between *LTV* and income based on the industry-specific opportunities available in the MSA of the individual's residence. The intuition behind using industry-specific opportunities is that individuals may find it easier to transition to new jobs within the same industry as they can more easily leverage their skills in such jobs. For instance, an IT professional looking for growth opportunities is more likely to search for opportunities within her sector. Such a professional residing in the San Francisco bay area may find it easier to change jobs without changing residence than if she were living in St Louis. Similarly an engineer working in the automobile industry is more likely to find alternate employment opportunities in Detroit than in say, Miami.

We measure the industry-specific opportunities available in a region as the ratio of the number of residents of a MSA employed in the specific industry based on 3-digit NAICS code to the total number of employed residents in the same MSA as of Jan 2010. Thus a higher number indicates that a larger fraction of jobs in that region are from the industry of an individual's employment. This is likely to indicate the industry specialization of the local area. We differentiate individuals into those residing (and working) in MSAs with *Above* and *Below* median levels of industry-specific jobs and repeat our tests and report the results in Table 7. We expect *LTV* to especially affect the income of individuals residing in MSAs with below median levels of industry-specific jobs. As before, we only report the coefficients on the interaction terms with $1_{\{0.8 \leq LTV < 1\}}$, $1_{\{1 \leq LTV < 1.5\}}$ and $1_{\{LTV \geq 1.5\}}$ for brevity.

The outcome variable in column (1) is the level of income measured in dollars (*Income ($)*). We find that the effect of *LTV* on labor income is greater for individuals who reside in MSAs with fewer industry-specific jobs. The difference between coefficients on the interaction terms involving *Above* and *Below* are statistically significant. In column (2) we model logarithm of income and find similar results. Finally in column (3) we focus on income growth and again find that *LTV* has a stronger effect on income growth for individuals who reside in MSAs with below median levels of industry-specific jobs relative to individuals residing in MSAs with above median levels of industry-specific jobs.

In a similar vein, if the negative association between *LTV* and labor income is driven by the mobility channel, high *LTV* values should have a stronger effect for individuals who are restricted from taking up other job opportunities in the same or related industry within the region of their residence. We evaluate the merits of this prediction by exploiting the differences in non-compete laws at the state-level. Among other things, non-compete laws prohibit employees from using the skills and knowledge gained from their existing employer for a set period of time after their employment, either by working for a competitor within a reasonable geographic area or by recruiting business from current clients. Hence, individuals residing in states where non-compete laws are strictly enforced may not be able to take up other nearby job opportunities in the same or related industry as their current job even if such opportunities exist. On the other hand, in states where non-compete laws are not enforced, individuals have the freedom to search for job opportunities in any industry within the same region.

We estimate the heterogeneity in the relation between *LTV* and labor income based on the enforcement of non-compete laws in the state of residence in Table 8. We use the non-compete enforcement index developed in Garmaise [2011] to measure the enforcement of non-compete laws. The index is based on twelve different dimensions of non-compete enforcement and takes a value from zero to twelve based on whether or not a state's enforcement exceeds a certain threshold on each of the twelve dimensions. The list of states with index values can be found in Appendix Table IA3. We classify individuals residing in states where non-compete laws are either prohibited (index value of zero) or enforcement is weak (index value less than three) as being in weak non-compete law states *(Below)* and those residing in states with enforcement index greater than three as being in strict non-compete law states (*Above)*. In columns (1) and (2) we find that the effect of *LTV* on labor income is greater for individuals who reside in states with strict non-compete laws. In column (3) we focus on income growth and find that *LTV* has a stronger effect on income growth for individuals who reside in states where non-compete laws are strictly enforced relative to individuals who reside in states where either non-compete laws are prohibited or enforcement is weak.

In columns (4) - (6) of both Tables 7 and 8, we examine the heterogeneity in the relation between *LTV* and income for the placebo sample. We find no significant effect of *LTV* on income for individuals with *LTV* values greater than 0.8 across categories of individuals residing in regions with different levels of industry-specific jobs and non-compete law enforcement.

4.3 Home Equity & Labor Income : Wage gains during job change

If high *LTV* imposes costs when an individual moves residence, then she is likely to move only if she receives an attractive job opportunity that more than compensates for the additional imposed costs. Thus when we focus on inter-MSA job changes, we expect high *LTV* individuals to experience a larger wage gain as compared to low *LTV* individuals. We test this in Table 9. For this analysis, we include one observation per inter-MSA job change and exclude individual and within cohort time effects because there is not enough variation within the same individual or cohort-time. The outcome variable in column (1) is the log change in income at the new job relative to the old job. We find that individuals with *LTV* between 1 and 1.5 experience a 0.9 percentage point *higher* wage growth when they change jobs across MSAs as compared to individuals with *LTV* between 0.3 and 0.4. In column (2) we repeat our tests with the percentage change in income that accompanies a job change and again find that high *LTV* is associated with a larger increase in pay for inter-MSA job changes.[11] These results are consistent with individuals with high *LTV* perceiving a cost to moving across MSAs.

4.4 Home Equity & Labor Income : Mobility vs Debt Overhang Channels

To distinguish the mobility channel from the debt overhang channel, we focus on job change not involving residential mobility. According to the mobility channel, if the individual's income growth is depressed due to constraints on moving residence then she may try to change jobs without moving residence to improve her prospects. The individual's ability to do so will be enhanced if the local labor market offers alternate job opportunities. On the other hand, if the individual's income is depressed due to debt-overhang, then such individuals will show no inclination to change jobs even within the region. Such individuals will attempt to improve their labor supply only if there is a reduction in their debt load. We use this contrasting prediction to distinguish the mobility channel from the debt-overhang channel.

In Table 10 we evaluate the effect of *LTV* on job change without an accompanying change in residence. While the mobility channel will predict that households with high *LTV* will exhibit greater tendency to change jobs without moving residence, the debt overhang channel will have no equivalent prediction.

We construct an indicator variable, *Job change*, that captures job change in the absence of geographic mobility. It turns on in month t if the individual changes employer while residing in the same address till month t+6.[12] Panel A of Table 10 reports results for the effect of *LTV* on job change not involving geographic mobility.[13] Although we include the full set of *LTV* indicator variables, we only report the coefficients on $1_{\{0.8 \leq \text{LTV} < 1\}}$ and $1_{\{1 \leq \text{LTV} < 1.5\}}$ for brevity. In column (1) we present the baseline IV estimates. We find that individuals with $LTV \in [1, 1.5)$ are 0.2

[11] If high LTV individuals begin with a low income, these results may be driven by mean reversion in income. To ensure this is not the case, we re-estimate our results after controlling for income in the previous job in Table IA4 and find similar results.

[12] We do a robustness test in Table IA5 where we define this variable as a dummy variable that takes a value of one in month t if the individual changes employer while residing in the same address in months t and $t-1$.

[13] We find similar results if we define this variable as individuals changing employers without changing the zip code of their residence.

percentage points more likely to change jobs without moving residence than individuals with *LTV* $\in [0.3, 0.4)$. This result is economically significant when compared to the mean likelihood of an individual changing jobs without moving residence of 1.7% in our sample.

If individuals are changing jobs without moving residence to mitigate the loss of out-of-region job opportunities, we would expect this to be higher for individuals with low access to liquidity and credit as we find that their inter-MSA mobility is especially constrained by high *LTV* values. In column (2) we differentiate borrowers based on their access to liquidity measured as the aggregate amount of undrawn credit limits in their card accounts. As before, we classify borrowers with above (below) median undrawn limits as a proportion of mortgage outstanding as of Jan 2010 as having *Above* (*Below*) median access to liquidity. Although consistent with our conjecture, the magnitude of the coefficients on the interaction terms involving *Below* are larger than the ones involving *Above*, we find that they are not statistically significant. In column (3) we repeat our tests differentiating borrowers based on their credit score as of Jan 2010. We find that while *LTV* increases the likelihood of *Job change* for borrowers with *Below* median credit scores, it does not affect the likelihood of *Job change* for borrowers with *Above* median credit scores. The difference between the coefficients on the interaction terms involving *Above* and *Below* are statistically significant and economically large.

In column (4) of Panel A we differentiate individuals based on the availability of industry-specific jobs in their MSA to examine the effect of *LTV* on *Job change*. We find that high *LTV* individuals who reside in MSAs with *Above* median levels of industry-specific jobs are more likely to change jobs without moving residence relative to high *LTV* individuals who reside in MSAs with *Below* median levels of industry-specific jobs. Finally in column (5) we examine the effect of state-level non-compete law enforcement on the effect of *LTV* on the likelihood of *Job change*. Although we find that the positive association between *LTV* and the likelihood of *Job change* is stronger for individuals who reside in states where either non-compete laws are prohibited or enforcement is weak, we find that the coefficient on the interaction terms are not significantly different across the sub-samples.

In Panel B of Table 10 we estimate the relation between *LTV* and the likelihood of *Job change* for the placebo sample. We find no significant relation between *LTV* and the likelihood of *Job change* for this renters sample.

In addition, our data allows us to directly test for the debt overhang channel by evaluating changes in labor supply on the intensive margin. Our sample includes both hourly wage and salaried workers. We conduct separate tests for these employees to evaluate changes in labor supply. First, for hourly workers, we relate the hours worked with home *LTV* using our baseline specification. Panel A of Table 11 presents these results where we don't find any significant relation. Second, for salaried workers, we relate the fraction of variable pay to *LTV*. Under the assumption that labor supply adjustments are more likely to affect variable pay, the debt-overhang hypothesis would predict a decrease in the fraction of variable pay for individuals with high *LTV*. Here again, we find an insigificant relation between the fraction of variable pay and *LTV* as reported in Panel B of Table 11. If anything, the fraction of variable pay may be larger for very highly levered individuals though the economic magnitudes are small. These tests do not offer support for individuals adjusting their labor supply on the intensive margin in response to high home *LTV*.[14]

4.5 Home Equity & Labor Income : Current job versus job change

Constrained mobility due to high *LTV* can depress both the income in the current job and the wage gain an employee experiences when she changes jobs. The former can happen if constrained mobility reduces employee search effort for alternate employment and consequently her bargaining power. The latter can happen due to the constrained opportunity set the employee faces when she searches for a job. For instance, high *LTV* individuals who are not willing to move will likely look for opportunities within the region of their residence. Given the way we construct our sample, both these will contribute to our baseline estimates. In Tables 12 and IA6

[14]Our results may also be driven by default institutions as in Herkenhoff and Ohanian, 2019. However, in Table IA7 we re-estimate our baseline tests after dropping delinquent individuals from our sample and find similar estimates as our baseline coefficients. This suggests that defaults are not the main channel that drive our results.

we evaluate the importance of each in turn.

In Table 12, we evaluate the baseline effect by limiting the sample up until the first month an individual changes jobs. Thus we do not include any observations from the new job. Within this constrained sample, we repeat our baseline tests. The dependent variable in column (1) is the level of income, *Income ($)*. We find that individuals with $LTV \in [1, 1.5)$ earn 351.6 dollars less than the base case. This effect is very similar to our baseline estimates of 352.1 dollars. In column (2), we repeat our analysis with logarithm of income as the dependent variable while in column (3) we employ income growth as the outcome variable. In both columns we find that individuals with *LTV* greater than 0.8 experience lower income growth as compared to our base case. Thus individuals with high *LTV* endure a wage discount in their job and this contributes significantly to our baseline estimates.

In Table IA6 we evaluate the extent to which the wage change that accompanies a job change is related to home *LTV*. For this analysis, we include one observation per job change, and exclude individual and within cohort time effects because there is not enough variation within the same individual or cohort-time. The outcome variable in column (1) is the log change in income at the new job relative to the old job. We find that individuals with *LTV* between 1 and 1.5 experience a 2 percentage point lower wage increase when they change jobs as compared to individuals with *LTV* between 0.3 and 0.4. In column (2) we repeat our tests with the percentage change in income as the outcome variable and obtain similar results. Thus constrained mobility adversely affects the wage change the individual experiences when she changes jobs.

Note that the average change in income accompanying a job change is a function of the fraction of job changes with and without a change in residence and the income changes that accompany these. Our results indicate that individuals with high *LTV* experience fewer inter-MSA job changes (those that are accompanied by a change in residence), experience a higher income gain when they do experience an inter-MSA job change and experience a lower income gain on average with a job change. These combined indicate that these individuals should experience a smaller income gain with an intra-MSA job change.

5 Robustness

5.1 Sample Selection

A potential concern with our analysis is that individuals may drop out of our sample if they stop being employed within the firms covered in the Equifax data. This sample selection may potentially bias our estimates, especially if the probability of attrition from the sample correlates with *LTV*. We conduct a number of tests to evaluate this potential bias. We begin by plotting the attrition rate in our sample through time in Panel A of Figure IA1. The plot shows that on average we lose about less than 1% of our sample every month and this attrition rate seems to be relatively constant during our sample period. To evaluate the extent to which our estimates may be driven by this selection, we re-estimate our baseline results on a sub-sample that only includes individuals who we are able to observe throughout our sample period. Table IA8 reports the results for this subsample and we find them to be very similar to our baseline estimates.

The next set of tests examine different characteristics of individuals who drop out of our sample and potential factors that may drive this attrition. First, in Panels B through D of Figure IA1, we compare the evolution of credit profile of individuals who drop out of our sample at some point (the attrition sample) to that of individuals who remain in our sample (the remain sample). Specifically, we plot average values of credit scores, mortgage and non-mortgage debt balances through our sample period for both samples. We find that while there are some differences in these characteristics across the two samples during the first half of the sample period, the differences are eliminated during the latter period. In the earlier part of the sample, the individuals in the attrition sample have slighly higher credit scores and mortgage balances. There is no systematic pattern in the non-mortgage balances. Second, we evaluate whether the attrition rate relates to different individual characteristics. Figure 6 reports these results wherein we plot the average probability of attrition across different levels of age, credit score, total debt, mortgage debt, *LTV* and *SLTV*. We find no systematic patterns between attrition rate and these characteristics though there seems to be some differences across individuals with different *SLTV* buckets. Finally and most crutially, we test to see if the probability of attrition is related to *LTV* using our baseline specification. Table 13 reports these results where we find no significant relation between our *LTV* buckets and the probability of attrition. Further, the last panel of Figure 6 shows a similar non-significant relation between *LTV* buckets and attrition rate with more granular *LTV* buckets. Overall, these results indicate that sample selection is unlikely to significantly bias our results.

5.2 Measurement and Misclassification Error Problem

Since we do not observe the house price at the time of mortgage origination, our origination *LTV* could be different from the actual *LTV*. If the homeowner provides more down payment than what we assume, then our origination *LTV* will be larger than the actual *LTV*, whereas if the actual down payment is lesser, then our *LTV* will be smaller. Thus, the difference between actual down payment and the down payment we assume will drive a wedge between the *LTV* we calculate and the actual *LTV*. In addition, individual house prices can change at a faster or slower rate than the zip code level house prices. The measurement error induced by these factors may potentially bias our estimates but a number of factors help assuage these concerns. First, our use of multi-dimensional fixed effects will help control for some sources of this measurement error. For example the cohort specific time effects we include will control for the tendency of individuals within specific purchase cohorts to put down more or less downpayment. Second, the insignificant estimates from our placebo analysis suggests that the measurement error likely doesn't affect our estimates. If the measurement error were correlated with income trends for individuals within the main sample, one would expect it to be correlated with income trends for renters as well because both group of individuals are subject to similar economic conditions as they are of same age, reside in the same zip code and are employed at the same firm with similar tenure. Third, our use of non-parametric piecewise function instead of a linear function of *LTV* helps minimize the effect of noise in our *LTV*. For instance, if the true *LTV* is 1.26 while our imputed *LTV* is 1.15, this will not induce an error in our estimation because we will correctly assign the homeowner to the [1,1.5) bucket. This noise may however lead to misclassification errors if we assign individuals to incorrect *LTV* buckets. (e.g. Schulhofer-Wohl, 2012).

Notwithstanding these arguments, we do a number of tests to ensure that this measurement problem and the resulting misclassification don't drive our results. We begin by evaluating how our *LTV* measure compares to two different *LTV* distributions reported in Gerardi et al., 2018 which are based on actual origination *LTV* values. The first *LTV* distribution is constructed using a sample of active, owner-occupied, first-lien mortgages that are not in foreclosure.[15] We call this distribution CRISM 1. The second *LTV* distribution is constructed using a more restricted sample of single-family, prime-age (homeowner ages 24 to 65) mortgages with *LTV* ratios below 2.5 and positive mortgage balances.[16] We call this distribution CRISM 2. The sample requirements used in our paper are more similar to those used to construct CRISM 2 than CRISM 1. Table IA9 compares the percentage of our sample that belongs to different *LTV* buckets to the percentage of CRISM samples belonging to same buckets for years 2011 and 2013.[17] Overall, our sample *LTV* distribution matches the CRISM 2 distribution well. Although our *LTV* distribution contains a slightly greater (lower) proportion of mortgages in the "low" ("high") *LTV* group during both years, the magnitude of these differences is economically small. For example, in 2013, 65.4% (7.3%) of our sample belongs to the "low" ("high") *LTV* group while 62.2% (7.5%) of the CRISM 2 sample belongs to the "low" ("high") *LTV* group. The time trends in our *LTV* distribution also match the time trends in the CRISM 2 distribution well. For example, the proportion of mortgages in our sample that belongs to the "low" ("high") *LTV* group increases (decreases) from 47.5% to 65.4% (18.3% to 7.3%) between 2011 and 2013. For the CRISM 2 sample, this proportion increases (decreases) from 45.1% to 62.2% (21.7% to 7.5%) over the same period.

We also conduct a number of tests where we repeat our baseline analysis using specifications where misclassification is likely to play a smaller role. First, we re-estimate our baseline test by dropping the observations that are close to the cut-offs for our bins. Specifically, for all cut-off

[15] See pages 1104-1106, Appendix page 21, and Tables 2 and A.14 of Gerardi et al. (2018).

[16] This sample also imposes the requirements of the first sample: active, first-lien, owner-occupied mortgages that are not in foreclosure. See Appendix page 24 and Table A.15 of Gerardi et al. (2018). This is also the distribution Gerardi et al. (2018) uses to provide sample weights located at https://sites.google.com/site/kyleherkenhoff/research.

[17] We use years 2011 and 2013 because those are the only years for which LTV distribution is available from Gerardi et al., 2018.

points, we drop observations with $LTV \in [c - 0.02, c + 0.02]$ where c is the cut-off value. For example, for the bin that identifies observations with $LTV \in [0.8, 1)$, we drop observations with $LTV \in [0.78, 0.82]$ and $LTV \in [0.98, 1.02]$. To the extent misclassification is more likely to occur around the cut-offs, this will reduce the error. Table 14 reports results for these tests where we find similar results to our baseline. Second, we re-estimate our analysis by including only one dummy variable as the independent variable. This dummy identifies observations with $LTV > 0.8$ (i.e. we split the sample into only two bins). This will reduce the misclassification as it is likely to occur around only one cut-off point of 0.8. Finally, we re-estimate our analysis using a combination of the above two tests, i.e. we employ one dummy variable that identifies observations with $LTV > 0.8$ and drop observations on the neighborhood of the cut-off (i.e. those with $LTV \in [0.7, 0.9]$). Not including observations in this broader range of LTV values around the cut-off value is likely to reduce misclassification error further. Table IA10 reports results for these tests with only one dummy as the independent variable. While Panel A estimates our results for the entire sample, Panel B drops observations with $LTV \in [0.7, 0.9]$. Across both samples, we find consistent estimates to our baseline.

Another factor that may be of concern in our setting are home improvements as they may be correlated with both income and difference between actual house prices and zip code level house prices. To the extent home improvements are correlated with the purchase cohort, our cohort time fixed effects will control for those. On the other hand, if older cohorts that live in zip codes where house prices are expected to increase are more likely to make such improvements, then our fixed effects will not control for these. To the extent that households that perform home improvements have a differential labor income as compared to households that do not perform home improvement, this can bias our estimates. We perform two sets of tests to evaluate this concern. First, in Table IA11 we repeat our estimates within the sub-sample of individuals with below median income. To the extent such individuals are less likely to engage in home improvement, the bias should be less in this sub-sample. We find our results to be unaffected. Further in Table IA12 we repeat our estimates after dropping all individuals that originate a home equity line of credit, secured home improvement and other home equity loans sometime after moving into a residence. Such lines could likely be used to perform home improvements. Here again, we find our estimates to be similar.

Finally, we conduct additional robustness tests to ensure that our results are not unduly influenced by the assumptions we make about the origination *LTV*. In Panel A of Table IA13 in the IA, we repeat our estimates by assuming the origination *LTV* to alternatively be 0.75 and 0.85 for individuals who originate one mortgage during a month. In the case of multiple mortgage originations, we continue to assume the *LTV* of the larger mortgage to be 0.8. We find our results to be unaffected.

In Panel B of Table IA13 in the IA, we recalculate *LTV* at origination using zip code level median house price. We obtain information on zip code level median house price from the Corelogic data set which tracks the transactions that occur at a given zip code in a month and reports the median sales price. In order to minimize the error in this calculation, we restrict the sample to the most homogeneous zip codes, i.e. those where the standard deviation in transaction prices is at the bottom decile. Since an overwhelming majority of the mortgages in our sample were originated during the 2001 through 2006 period, we evaluate historical price deviations at the zip code level using data from the period between 1990 and 2000. As before, our dependent variables include levels of income, logarithm of income and percentage change in income. We find our results to be unaffected by this change.

Overall, these results suggest that the measurement error, if any, and the resulting potential misclasification likely don't have a significant effect on our estimates.

5.3 Instrument Construction & Alternative Instrument

To ensure that our results are not sensitive to our choice of interest rate and loan maturity that we employ to construct our instrument, we repeat our estimation with alternate assumptions. Instead of a constant interest rate for all mortgages in our sample, we use a time varying interest rate based on the national average interest rate on all mortgages issued during the month of origination. Panel A of Table IA14 in the IA reports coefficients for this estimation. Across different measures of income and income growth, we find similar results to our baseline estimates. For instance, we find that individuals with $LTV \in [1, 1.5)$ earn 381.6 dollars lower income and experience a 3.3 percentage points lower income growth than individuals with $LTV \in [0.3, 0.4)$.

In Panel B of Table IA14 of the IA we use interest rates and loan maturities that vary by region and time, i.e. we use state level averages for these variables for month of mortgage origination. For instance, if the mortgage was originated in Jan 2002 in California, we use average value of interest rate and maturity for all mortgages originated in California during the month of Jan 2002 to construct the instrument. As before, our dependent variables include levels of income, logarithm of income and percentage change in income. We find similar results as our baseline estimates.

In addition, we also use an alternative instrument from Bernstein and Struyven [2017] and follow their specification to verify our results. This instrument constructs synthetic *LTV* (i.e. *SLTV*) that is only a function of house price change since origination and keeps the loan outstanding constant during the sample period. Specifically, the only difference between our instrument and theirs is that they assume no amortization on the loans. Table IA15 reports results for this specification where Panel A reports the first stage and Panel B reports the IV estimates. As before, we find that the F-statistics across all columns in Panel A is much greater than the threhold value of 10 suggesting that the instrument doesn't suffer from the weak instrument problem. In Panel B, we obtain results similar to our baseline estimates suggesting that our results are likely not driven by specific assumptions we make to construct our baseline instrument.

5.4 Unobserved economic conditions

The biggest threat to our identification are potential unobserved local economic shocks that are correlated with both zip code level house price changes and the purchase cohort. For example, these could be shocks that differentially affect older versus younger households. Our parallel analysis with the sub-sample of renters is designed to overcome this concern. Since renters and homeowners in our sample live in the same neighbourhood, work for the same firm with similar age and job tenure, they should be subject to similar labor market shocks. To this extent the differential response of their labor income to *LTV* that we observe, is unlikely to be due to unobserved economic shocks that are correlated with both labor income and *LTV*.

One candidate economic shock that could be correlated with both our instrument and labor income could be industry-level shocks that differentially affect individuals residing within the same zip code. For example, homeowners that belong to older purchase cohorts in Detroit may work in the auto industry while those in the younger cohort may work for a home mortgage firm such as Quicken Loan. The decline in home prices in Detroit could be correlated with the decline in fortunes of the auto industry. Thus the older cohorts may experience different income trajectories as compared to the younger cohorts. While our renter analysis is likely to address this concern as our placebo sample includes individuals working in the same industry, we additionally repeat our estimates after including within zip code industry-time effects. Table IA16 reports the results of this analysis. Across different measures of income and income growth, we find similar results to our baseline estimates. In addition, as mentioned before, Table IA2 reports additional results of tests that control for regional shocks. Overall, these results lend support to our exclusion restriction and suggest that specific regional shocks likely don't drive our estimates.

5.5 Alternate definitions of mobility

In Table IA17 of the IA we repeat our analysis defining mobility at the zip code level instead of the MSA level. *Mobility* now takes a value of one in year-month t if the zip code associated with individual i's primary residence in month $t-1$ is different from their zip code in month t. Consistent with our baseline results, we find that individuals with high *LTV* values are less likely to move relative to individuals in the omitted category. Specifically, individuals with $LTV \in [0.8, 1)$ and $LTV \in [1, 1.5)$ are 0.9 and 1 percentage points less likely to move in a given month relative to our base case. From column (2) we find that the effect of *LTV* on mobility is stronger for individuals with below median levels of access to liquidity. Similarly, in column (3) we examine the heterogeneity in the relation between *LTV* and mobility based on individual's credit score as of Jan 2010. We find that the relation is significantly stronger for individuals with below median levels of credit score. This shows that our results are not sensitive to our definition of mobility.

6 Economic implication

In this section, we quantify the economic implication of our estimates. To do this, we use our estimates from Table 4 and the distribution of *LTV*s in our sample. Our estimates imply that a 1% fall in nationwide average house price will result in a 0.1% decline in monthly wages. This happens because of an increase in the proportion of individuals with high *LTV*s. This effect is significant because Figure 3 shows that annual house prices have fallen by more than 13% in over 5% of the zip code-years in our sample. This implies that house price changes can lead to 1.3% decline in monthly wages in over 5% of our sample zip code-years. Another way to provide context to these estimates is to note that from Jan 2007 to Dec 2010, average U.S. house prices declined by 23.03%.[18] Our estimates imply that such a decline can result in a 2.3% decline in wages due to constrained mobility.

We can also quantify our estimates in terms of the aggregate loss in total wages due to constrained mobility. Assuming that the distribution of *LTV* among *all* individuals with an open mortgage in the credit data is the same as the distribution of *LTV* in our sample, our estimates imply an aggregate loss of $452.27 billion in wages over our six year sample period due to high *LTV* constraining mobility.[19]

[18] Based on the S&P/Case-Shiller U.S. home price index available at https://fred.stlouisfed.org/series/CSUSHPINSA

[19] It is worth noting that there maybe some general equilibrium indirect effects on low LTV individuals. However, we argue that the direction of these indirect effects is not obvious. For example, if firms do not create new or relocate existing jobs, then low LTV homeowners may benefit from additional bargaining power (and hence receive higher wages). But if firms do create or relocate jobs, this bargaining advantage may not exist. This force, along with others, makes the indirect effect on income ambiguous. Therefore, we focus our discussion on the direct effects of LTV on labor income.

7 Conclusion

This paper uses detailed credit and employment data for millions of individuals in the U.S. to estimate the effect of home equity on labor income and explore the mechanisms through which this effect operates. We document a strong negative relation between *LTV* of an individual's primary residence and income for a sample of employed individuals. As compared to individuals with *LTV* between 0.3 and 0.4, individuals with *LTV* between 1 and 1.5 earn 352.1 dollars (5.1%) less monthly and experience slower income growth. We also document a negative relation between *LTV* and labor mobility especially among liquidity and credit constrained individuals.

Consistent with constrained mobility affecting labor income, we find that high *LTV* individuals who are liquidity and credit constrained experience greater declines in income. However, high *LTV* individuals residing in MSAs with greater employment opportunities and in regions with lax non-compete law enforcement experience relatively smaller income declines as they are able to move jobs without changing their residence to compensate for the loss of out-of-region opportunities.

Given the house price declines during the Great Recession, our estimates imply a 2.3% decline in monthly wages due to constrained mobility. Our results are of relevance to both policy makers and companies. Our results will help policy makers identify the geographies and the sub-populations that will be most constrained by low home equity. This can be used to design targeted policy interventions. Our results are also of relevance to firms interested in hiring and developing human talent. Our results show that credit constraints may affect an employee's willingness to move to take up job opportunities. If firms can relax such constraints, that may enhance labor mobility and consequently productivity.

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Table 1: Summary Statistics

This table reports the summary statistics of the variables we use in the analysis grouped into dependent and independent variables.

	N	Mean	St. Dev.	Min	Median	Max
Dependent variables						
Income ('000s $)	14,031,645	6.9	6.0	0.8	5.5	65.0
$\%\Delta Income$	13,506,434	10.1	30.7	-50.0	3.2	220.3
Mobility (%)	14,031,645	0.13	3.6	0	0	100
Job Change (%)	14,031,645	1.7	12.7	0	0	100
Independent variables						
Original loan amount ('000s $)	14,031,645	192.4	112.1	35.0	163.6	695.1
Purchase price ('000s $)	14,031,645	240.4	140.1	43.7	204.0	868.7
Loan balance ('000s $)	14,031,645	161.6	111.1	2.1	144.7	695.1
LTV	14,031,645	0.7	0.2	0.09	0.8	1.6
SLTV	14,031,645	0.8	0.2	0	0.8	2.3
$\mathbf{1}_{\{0\leq \text{LTV}<0.3\}}$	14,031,645	0.04	0.2	0	0	1
$\mathbf{1}_{\{0.3\leq \text{LTV}<0.4\}}$	14,031,645	0.11	0.2	0	0	1
$\mathbf{1}_{\{0.4\leq \text{LTV}<0.8\}}$	14,031,645	0.54	0.5	0	1	1
$\mathbf{1}_{\{0.8\leq \text{LTV}<1\}}$	14,031,645	0.2	0.4	0	0	1
$\mathbf{1}_{\{1\leq \text{LTV}<1.5\}}$	14,031,645	0.1	0.3	0	0	1
$\mathbf{1}_{\{1.5\leq \text{LTV}\}}$	14,031,645	0.01	0.1	0	0	1
$\mathbf{1}_{\{0\leq S\text{LTV}<0.3\}}$	14,031,645	0.01	0.1	0	0	1
$\mathbf{1}_{\{0.3\leq S\text{LTV}<0.4\}}$	14,031,645	0.08	0.1	0	0	1
$\mathbf{1}_{\{0.4\leq S\text{LTV}<0.8\}}$	14,031,645	0.56	0.5	0	1	1
$\mathbf{1}_{\{0.8\leq S\text{LTV}<1\}}$	14,031,645	0.24	0.5	0	0	1
$\mathbf{1}_{\{1\leq S\text{LTV}<1.5\}}$	14,031,645	0.1	0.3	0	0	1
$\mathbf{1}_{\{1.5\leq S\text{LTV}\}}$	14,031,645	0.01	0.1	0	0	1

Table 2: Home Equity & Labor Income : OLS

This table reports the coefficient estimates from the following OLS regressions that estimate the effect of LTV on labor income:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ indicate different LTV value buckets and take a value of one when the loan-to-value ratio (LTV) of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income, percentage change in income relative to the beginning of the sample and 12-month log changes in income. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Columns (1) through (3) report results for our main sample while the remaining columns report results for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). Coefficients and standard errors on log of income and log change in income are scaled by 100 for ease of interpretation.

	Main Sample				Placebo			
	Income ($)	Log(Income)	$\%\Delta Income$	$Log(\frac{Income_t}{Income_{t-12}})$	Income ($)	Log(Income)	$\%\Delta Income$	$Log(\frac{Income_t}{Income_{t-12}})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbf{1}_{\{0 \leq \text{LTV} < 0.3\}}$	66.5*	0.1	-0.1	0.03	-54.9***	-0.2**	-0.1	-0.1
	(37.3)	(0.1)	(0.1)	(0.05)	(10.1)	(0.1)	(0.1)	(0.1)
$\mathbf{1}_{\{0.4 \leq \text{LTV} < 0.8\}}$	-21.1	0.4	0.6	-0.1	8.2	0.1	0.05	-0.1
	(17.6)	(0.3)	(0.5)	(0.1)	(7.6)	(0.1)	(0.1)	(0.1)
$\mathbf{1}_{\{0.8 \leq \text{LTV} < 1\}}$	-83.2***	-0.2**	-0.2**	-0.3***	-6.7	-0.1	-0.4***	-0.1
	(12.7)	(0.1)	(0.1)	(0.04)	(8.5)	(0.1)	(0.1)	(0.1)
$\mathbf{1}_{\{1 \leq \text{LTV} < 1.5\}}$	-129.3***	-0.8***	-1.0***	-0.4***	14.6	0.01	-0.1	-0.1
	(14.6)	(0.1)	(0.1)	(0.04)	(10.4)	(0.1)	(0.1)	(0.1)
$\mathbf{1}_{\{1.5 \leq \text{LTV}\}}$	-30.8	-0.3**	-0.4**	-0.2***	30.4*	0.1	0.3	-0.1
	(26.6)	(0.1)	(0.2)	(0.04)	(15.4)	(0.2)	(0.3)	(0.3)
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	10,471,648	11,527,432	11,527,432	11,112,709	9,547,495
R^2	0.926	0.958	0.763	0.842	0.942	0.95	0.733	0.286

Table 3: Home Equity & Labor Income : Reduced Form

This table reports the coefficient estimates from the following reduced form regressions that estimate the effect of SLTV on labor income:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$ indicate different $SLTV$ value buckets which take a value of one when the synthetic loan-to-value ratio ($SLTV$) of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $SLTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. 2 Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Columns (1) through (3) report results for our main sample while the remaining columns report results for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). Coefficients and standard errors on log of income and log change in income are scaled by 100 for ease of interpretation.

	Main Sample				Placebo			
	Income ($)	Log(Income)	$\%\Delta Income$	$Log(\frac{Income_t}{Income_{t-12}})$	Income ($)	Log(Income)	$\%\Delta Income$	$Log(\frac{Income_t}{Income_{t-12}})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbf{1}_{\{0 \leq SLTV < 0.3\}}$	68.4*	0.4	0.8*	0.2	20.4	0.1	0.2	0.3
	(37.3)	(0.4)	(0.4)	(0.1)	(26.9)	(0.4)	(0.4)	(0.3)
$\mathbf{1}_{\{0.4 \leq SLTV < 0.8\}}$	42.6	0.3	0.3	-0.03	21.9	0.2	0.1	0.1
	(35.1)	(0.4)	(0.4)	(0.1)	(15.4)	(0.3)	(0.3)	(0.2)
$\mathbf{1}_{\{0.8 \leq SLTV < 1\}}$	-130.9***	-0.8***	-1.2***	-0.2*	-8.4	0.3	-0.3	0.2
	(26.5)	(0.3)	(0.3)	(0.1)	(15.4)	(0.3)	(0.3)	(0.2)
$\mathbf{1}_{\{1 \leq SLTV < 1.5\}}$	-189.6***	-1.2***	-1.9***	-0.4***	18.3	0.6**	-0.1	-0.1
	(27.3)	(0.3)	(0.3)	(0.1)	(16.0)	(0.3)	(0.3)	(0.2)
$\mathbf{1}_{\{1.5 \leq SLTV\}}$	-73.9**	-0.7**	-1.3***	-0.3***	24.4	0.1	0.2	-0.1
	(28.8)	(0.3)	(0.4)	(0.1)	(20.2)	(0.4)	(0.4)	(0.3)
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	10,471,648	11,527,432	11,527,432	11,112,709	9,547,495
R^2	0.926	0.958	0.762	0.842	0.942	0.95	0.733	0.286

Table 4: Home Equity & Labor Income : IV Regression

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_{zt} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

$$y_{iczt} = \delta_{zt} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. 2 Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Panel A reports results on the first stage while Panel B reports findings from the second stage. Columns (1) through (3) report results for our main sample while the remaining columns report results for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). Coefficients and standard errors on log of income and log change in income are scaled by 100 for ease of interpretation.

Panel A: First stage regression					
	$\mathbf{1}_{\{0 \leq \text{LTV} < 0.3\}}$ (1)	$\mathbf{1}_{\{0.4 \leq \text{LTV} < 0.8\}}$ (2)	$\mathbf{1}_{\{0.8 \leq \text{LTV} < 1\}}$ (3)	$\mathbf{1}_{\{1 \leq \text{LTV} < 1.5\}}$ (4)	$\mathbf{1}_{\{1.5 \leq \text{LTV}\}}$ (5)
$\mathbf{1}_{\{0 \leq SLTV < 0.3\}}$	0.50***	-0.01	-0.01***	-0.00	0.00
	(0.008)	(0.005)	(0.002)	(0.001)	(0.001)
$\mathbf{1}_{\{0.4 \leq SLTV < 0.8\}}$	-0.04***	0.45***	0.01***	0.00***	0.00
	(0.005)	(0.007)	(0.002)	(0.001)	(0.001)
$\mathbf{1}_{\{0.8 \leq SLTV < 1\}}$	-0.04***	-0.19***	0.66***	0.01***	-0.00
	(0.005)	(0.007)	(0.002)	(0.001)	(0.000)
$\mathbf{1}_{\{1 \leq SLTV < 1.5\}}$	-0.02***	-0.33***	0.09***	0.71***	0.00
	(0.005)	(0.007)	(0.003)	(0.002)	(0.001)
$\mathbf{1}_{\{1.5 \leq SLTV\}}$	-0.01	-0.40***	-0.00	0.29***	0.57***
	(0.007)	(0.008)	(0.004)	(0.01)	(0.007)
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	14,031,645	14,031,645	14,031,645
F-Statistic	75.36	116.9	106.8	195.8	61.47

Table 4 (contd)

Panel B: Second stage regression	Main Sample				Placebo			
	Income ($)	Log(Income)	$\%\Delta Income$	$Log(\frac{Income_t}{Income_{t-12}})$	Income ($)	Log(Income)	$\%\Delta Income$	$Log(\frac{Income_t}{Income_{t-12}})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbf{1}_{\widehat{\{0\leq \mathrm{LTV}<0.3\}}}$	81.2	1.2	1.5	0.3	36.1	-0.1	0.3	0.4
	(72.6)	(0.8)	(1.0)	(0.2)	(52.8)	(0.8)	(0.8)	(0.5)
$\mathbf{1}_{\widehat{\{0.4\leq \mathrm{LTV}<0.8\}}}$	78.8	0.2	-0.03	-0.1	58.1	0.1	-0.7	0.3
	(68.9)	(0.7)	(0.9)	(0.1)	(43.3)	(0.6)	(0.7)	(0.5)
$\mathbf{1}_{\widehat{\{0.8\leq \mathrm{LTV}<1\}}}$	-263.7***	-1.5***	-2.4***	-0.3**	10.4	0.7	-0.1	0.2
	(57.2)	(0.6)	(0.7)	(0.1)	(42.8)	(0.6)	(0.7)	(0.4)
$\mathbf{1}_{\widehat{\{1\leq \mathrm{LTV}<1.5\}}}$	-352.1***	-2.2***	-3.4***	-0.5***	57.8	0.1	-1	0.2
	(57.1)	(0.6)	(0.7)	(0.1)	(43.6)	(0.6)	(0.7)	(0.4)
$\mathbf{1}_{\widehat{\{1.5\leq \mathrm{LTV}\}}}$	-178.6***	-1.4**	-2.4***	-0.4*	76.6	0.2	0.3	-0.9
	(58.6)	(0.6)	(0.7)	(0.2)	(52.0)	(0.7)	(0.8)	(0.6)
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	10,471,648	11,527,432	11,527,432	11,112,709	9,547,495
R^2	0.926	0.958	0.762	0.842	0.942	0.95	0.731	0.286

Table 5: Home Equity & Mobility

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor mobility and heterogeneity in this effect based on access to liquidity and credit:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1} < h_k}\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \text{LTV}_{\widehat{ir-1} < h_k}\}} \times Above + \sum_k \beta_k \times 1_{\{l_k \leq \text{LTV}_{\widehat{ir-1} < h_k}\}} \times Below + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ indicate different LTV value buckets which take a value of one when the loan-to-value ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variable y_{iczt} is *Mobility* which is defined as a dummy variable that takes a value of one for month t if the MSA of an individual's residence changes in month t. *Above* (*Below*) is a dummy variable that takes a value of one for individuals with above (below) median levels of access to liquidity based on the cross-sectional measures reported in different columns. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Columns (1) through (3) report results for our main sample while the remaining columns report results for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). All coefficients and standard errors are scaled by 100 for the ease of interpretation.

	Main Sample			Placebo		
	Mobility	Mobility	Mobility	Mobility	Mobility	Mobility
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-0.1***			-0.04		
	(0.04)			(0.04)		
$\mathbf{1}_{\{1.0 \leq \widehat{\text{LTV}} < 1.5\}}$	-0.1***			-0.02		
	(0.04)			(0.09)		
$1_{\{0.8 \leq \widehat{\text{LTV} < 1}\}} \times Above$		0.04	-0.04		-0.3	-0.2
		(0.06)	(0.1)		(0.3)	(0.5)
$1_{\{1 \leq \widehat{\text{LTV} < 1.5}\}} \times Above$		0.02	-0.02		-0.2	-0.1
		(0.06)	(0.08)		(0.4)	(0.6)
$1_{\{0.8 \leq \widehat{\text{LTV} < 1}\}} \times Below$		-0.1***	-0.1**		0.2	0.2
		(0.04)	(0.05)		(0.6)	(0.4)
$1_{\{1 \leq \widehat{\text{LTV} < 1.5}\}} \times Below$		-0.1***	-0.2***		0.1	0.1
		(0.04)	(0.06)		(0.4)	(0.5)
Cross-Sectional Variable		Unused Credit	Credit Score		Unused Credit	Credit Score
$1_{\{0.8 \leq \text{LTV} < 1\}} \widehat{\times [Above - Below]}$		0.14**	0.06		-0.5	0.0
$1_{\{1 \leq \text{LTV} < 1.5\}} \widehat{\times [Above - Below]}$		0.12*	0.18*		-0.3	0.0
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	14,031,645	11,527,432	11,527,432	11,527,432
R^2	0.340	0.340	0.339	0.588	0.588	0.588

Table 6: Home Equity & Income: Heterogeneity by Liquidity and Credit Constraints

This table reports the coefficient estimates from the following IV regressions that estimate the heterogeneous effect of LTV on labor income based on individual's access to liquidity and credit:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{ir-1} < h_k}\}} \times Above + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{ir-1} < h_k}\}} \times Below + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ indicate different LTV value buckets which take a value of one when the loan-to-value ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. *Above* (*Below*) is a dummy variable that takes a value of one for individuals with above (below) median levels of access to liquidity. Panel A (Panel B) reports results on the heterogenous effect based on different levels of unused credit (credit score). Columns (1) through (3) of Panel B report results for our main sample while the remaining columns report results for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

Panel A: Heterogeneity based on Unused Credit

	Main Sample			Placebo		
	Income ($)	Log(Income)	$\%\Delta Income$	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)	(4)	(5)	(6)
$1_{\{0.8 \leq \widehat{\text{LTV} < 1}\}} \times Above$	-123.7***	-1.1*	-1.4**	42.8	0.2	0.2
	(54.3)	(0.6)	(0.7)	(94.8)	(0.5)	(0.6)
$1_{\{1 \leq \widehat{\text{LTV} < 1.5}\}} \times Above$	-241.6***	-1.1*	-1.7**	42.0	0.4	0.4
	(53.1)	(0.6)	(0.7)	(111.8)	(0.6)	(0.6)
$1_{\{1.5 \leq \widehat{\text{LTV}}\}} \times Above$	-41.1	-0.6	-0.9	74.6	0.3	0.8
	(53.8)	(0.6)	(0.7)	(134.6)	(1.2)	(1.1)
$1_{\{0.8 \leq \widehat{\text{LTV} < 1}\}} \times Below$	-359.4***	-1.4***	-4.9***	21.7	-0.1	-0.02
	(87.8)	(0.5)	(1.5)	(384.0)	(0.5)	(0.5)
$1_{\{1 \leq \widehat{\text{LTV} < 1.5}\}} \times Below$	-429.2***	-3.3***	-5.1***	93.1	0.3	0.4
	(90.2)	(0.8)	(1.5)	(389.2)	(0.5)	(0.6)
$1_{\{1.5 \leq \widehat{\text{LTV}}\}} \times Below$	-247.3***	-2.2*	-4.1*	112.5	0.9	1.3*
	(89.7)	(1.2)	(2.2)	(391.1)	(0.6)	(0.7)
$1_{\{0.8 \leq \widehat{\text{LTV} < 1}\}} \times [Above - Below]$	235.7**	0.3	3.5**	21.1	0.3	0.2
$1_{\{1 \leq \widehat{\text{LTV} < 1.5}\}} \times [Above - Below]$	187.6*	2.2**	3.4**	-51.2	0.1	0.0
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	11,527,432	11,527,432	11,112,709
R^2	0.926	0.958	0.762	0.942	0.95	0.731

Table 6 contd

Panel B: Heterogeneity based on Credit Score						
	Main Sample			Placebo		
	Income ($)	Log(Income)	%$\Delta Income$	Income ($)	Log(Income)	%$\Delta Income$
	(1)	(2)	(3)	(4)	(5)	(6)
$1_{\widehat{\{0.8 \leq \text{LTV} < 1\}}} \times Above$	-62.7	-0.7	-1.5*	28.3	1.0	0.2
	(60.2)	(0.7)	(0.8)	(122.4)	(1.1)	(1.0)
$1_{\widehat{\{1 \leq \text{LTV} < 1.5\}}} \times Above$	-39.6	-1.2	-2.3***	54.8	1.0	0.2
	(59.5)	(0.8)	(0.9)	(125.7)	(1.1)	(1.0)
$1_{\widehat{\{1.5 \leq \text{LTV}\}}} \times Above$	99.2	-0.6	-1.6	96.5	1.8	1.3
	(81.6)	(0.8)	(0.9)	(146.0)	(1.4)	(1.4)
$1_{\widehat{\{0.8 \leq \text{LTV} < 1\}}} \times Below$	-464.2***	-2.7***	-3.5***	-21.3	-0.2	-0.1
	(94.0)	(0.8)	(1.0)	(89.0)	(0.6)	(0.7)
$1_{\widehat{\{1 \leq \text{LTV} < 1.5\}}} \times Below$	-677.6***	-3.6***	-4.7***	15.0	-0.2	-0.8
	(90.6)	(0.8)	(1.0)	(105.5)	(0.6)	(0.7)
$1_{\widehat{\{1.5 \leq \text{LTV}\}}} \times Below$	-428.9***	-2.5***	-3.1***	11.5	-0.2	-0.3
	(92.2)	(0.8)	(1.0)	(123.4)	(0.9)	(1.0)
$1_{\widehat{\{0.8 \leq \text{LTV} < 1\}}} \times [Above - Below]$	401.5***	2.0*	2.0	49.6	1.2	0.3
$1_{\widehat{\{1 \leq \text{LTV} < 1.5\}}} \times [Above - Below]$	638.0***	2.4**	2.4**	39.9	1.2	1.0
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	11,527,432	11,527,432	11,112,709
R^2	0.926	0.958	0.762	0.942	0.95	0.731

Table 7: Home Equity & Income: Heterogeneity by Local Job Opportunities

This table reports the coefficient estimates from the following IV regressions that estimate the heterogeneous effect of LTV on labor income based on the concentration of same industry jobs in the MSA of individual's residence:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\widehat{\{l_k \leq \text{LTV}_{ir-1} < h_k\}}} \times Above + \sum_k \beta_k \times 1_{\widehat{\{l_k \leq \text{LTV}_{ir-1} < h_k\}}} \times Below + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ indicate different LTV value buckets which take a value of one when the loan-to-value ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. *Above* (*Below*) is a dummy variable that takes a value of one for individuals who reside in MSAs with above (below) median levels of percentage of individuals employed in the same industry as them as of January 2010. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Main Sample			Placebo		
	Income ($)	Log(Income)	%$\Delta Income$	Income ($)	Log(Income)	%$\Delta Income$
	(1)	(2)	(3)	(4)	(5)	(6)
$1_{\widehat{\{0.8 \leq \text{LTV} < 1\}}} \times Above$	-79.2	1.9	1.6	23.5	0.6	1.1
	(68.1)	(1.7)	(1.9)	(95.2)	(0.6)	(0.8)
$1_{\widehat{\{1 \leq \text{LTV} < 1.5\}}} \times Above$	-157.3***	0.6	-0.4	36.3	0.1	0.2
	(62.7)	(0.7)	(0.7)	(111.2)	(0.6)	(0.8)
$1_{\widehat{\{1.5 \leq \text{LTV}\}}} \times Above$	-72.5	1.3	0.5	30.8	0.9	1.0
	(69.6)	(1.8)	(0.8)	(131.2)	(0.9)	(1.0)
$1_{\widehat{\{0.8 \leq \text{LTV} < 1\}}} \times Below$	-446.9***	-5.3***	-6.6***	58.3	0.3	-0.6
	(73.4)	(0.7)	(0.8)	(100.2)	(0.5)	(0.7)
$1_{\widehat{\{1 \leq \text{LTV} < 1.5\}}} \times Below$	-399.1***	-5.2***	-7.4***	64.1	0.03	0.4
	(71.2)	(0.7)	(0.9)	(116.9)	(0.5)	(0.7)
$1_{\widehat{\{1.5 \leq \text{LTV}\}}} \times Below$	-239.4***	-4.2***	-5.9***	73.5	0.1	-0.04
	(69.1)	(0.8)	(0.8)	(146.7)	(1.2)	(1.2)
$1_{\widehat{\{0.8 \leq \text{LTV} < 1\}}} \times [Above - Below]$	367.7***	7.2***	8.2***	-34.8	0.3	1.7
$1_{\widehat{\{1 \leq \text{LTV} < 1.5\}}} \times [Above - Below]$	241.8***	5.8***	7.0***	-27.7	0.1	-0.2
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	11,527,432	11,527,432	11,112,709
R^2	0.926	0.958	0.762	0.942	0.95	0.731

Table 8: Home Equity & Income: Heterogeneity by Non-Compete Laws

This table reports the coefficient estimates from the following IV regressions that estimate the heterogeneous effect of LTV on labor income based on non-compete enforceability in the state of individual's residence:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\mathrm{LTV}_{ir-1} < h_k}\}} \times Above + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\mathrm{LTV}_{ir-1} < h_k}\}} \times Below + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ indicate different LTV value buckets which take a value of one when the loan-to-value ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. *Above* (*Below*) is a dummy variable that takes a value of one for individuals who reside in states with high (low) levels of non-compete enforceability. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Main Sample			Placebo		
	Income ($)	Log(Income)	$\%\Delta Income$	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)	(4)	(5)	(6)
$1_{\{0.8 \leq \widehat{\mathrm{LTV}<1}\}} \times Above$	-269.8***	-1.4**	-2.5***	51.3	0.2	-0.3
	(61.2)	(0.6)	(0.8)	(67.7)	(0.6)	(0.6)
$1_{\{1 \leq \widehat{\mathrm{LTV}<1.5}\}} \times Above$	-384.4***	-2.3***	-3.6***	72.5	0.5	-0.3
	(67.4)	(0.6)	(0.7)	(83.1)	(0.7)	(0.6)
$1_{\{1.5 \leq \widehat{\mathrm{LTV}}\}} \times Above$	-185.6***	-1.5***	-2.9***	-35.6	0.8	-0.3
	(71.3)	(0.7)	(0.6)	(104.0)	(0.9)	(0.8)
$1_{\{0.8 \leq \widehat{\mathrm{LTV}<1}\}} \times Below$	-181.4***	-0.1	-1.4	-70.1	0.3	0.2
	(76.9)	(0.8)	(0.9)	(171.8)	(0.9)	(0.9)
$1_{\{1 \leq \widehat{\mathrm{LTV}<1.5}\}} \times Below$	-179.2***	-0.1	-1.1	87.4	0.3	0.3
	(64.8)	(0.8)	(0.9)	(63.3)	(0.9)	(0.9)
$1_{\{1.5 \leq \widehat{\mathrm{LTV}}\}} \times Below$	-53.4	-0.5	-0.1	207.1	0.5	1.0
	(81.5)	(0.8)	(0.9)	(185.7)	(1.1)	(1.1)
$1_{\{0.8 \leq \widehat{\mathrm{LTV}<1\}} \times [Above - Below]}$	-88.4	-1.3	-1.1	121.4	-0.1	-0.5
$1_{\{1 \leq \widehat{\mathrm{LTV}<1.5\}} \times [Above - Below]}$	-205.2**	-2.2**	-2.5**	-14.9	0.2	-0.6
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	11,527,432	11,527,432	11,112,709
R^2	0.926	0.958	0.762	0.942	0.95	0.731

Table 9: Home Equity and Income gains with inter-MSA job change

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on income gains associated with changing jobs across different MSAs:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_{zt} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_{zt} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_{zt} are zipcode×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} include quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the log-changes and percentage changes in income for the new job relative to the old job. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors are scaled by 100 for the ease of interpretation.

	$Log(\frac{Income_t}{Income_{t-1}})$	$\%\Delta Income$
	(1)	(2)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	0.3	0.4
	(0.6)	(0.6)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	0.5	0.5
	(0.4)	(0.4)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	0.6	0.8**
	(0.4)	(0.4)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	0.9**	1.1***
	(0.4)	(0.4)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	1.9***	2.5***
	(0.6)	(0.7)
Sample	Job Changes outside MSA	
Tenure and Age Controls	Yes	Yes
Zipcode×Month FE	Yes	Yes
Observations	63,393	63,393
R^2	0.009	0.009

Table 10: Home Equity & Income : Debt Overhang vs Mobility Channel

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on the likelihood that individuals change the firm they are employed at but not their residence for the six months following job change, and heterogeneity in this effect based on access to liquidity and market conditions:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1} < h_k}\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \text{LTV}\widehat{_{ir-1} < h_k}\}} \times Above + \sum_k \beta_k \times 1_{\{l_k \leq \text{LTV}\widehat{_{ir-1} < h_k}\}} \times Below + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ indicate different LTV value buckets which take a value of one when the loan-to-value ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variable y_{iczt} is $JobChange$ which is defined as a dummy variable that takes a value of one for months when the firm of individual's employment changes but her residence remains the same. *Above* (*Below*) is a dummy variable that takes a value of one for individuals with above (below) median levels of access to liquidity and market conditions based on the cross-sectional measures reported in different columns. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Panel A reports results for our main sample while Panel B reports results for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). All coefficients and standard errors are scaled by 100 for the ease of interpretation.

Panel A: Main Sample					
			Job Change		
	(1)	(2)	(3)	(4)	(5)
$\mathbf{1}_{\{\mathbf{0.8} \leq \widehat{\text{LTV}} < \mathbf{1}\}}$	0.1				
	(0.2)				
$\mathbf{1}_{\{\mathbf{1.0} \leq \widehat{\text{LTV}} < \mathbf{1.5}\}}$	0.2**				
	(0.1)				
$1_{\{0.8 \leq \widehat{\text{LTV}} < 1\}} \times Above$		-0.8	-0.4	0.8***	-0.1
		(1.7)	(0.3)	(0.3)	(0.3)
$1_{\{1 \leq \widehat{\text{LTV}} < 1.5\}} \times Above$		-0.9	-0.3	0.8***	-0.2
		(1.7)	(0.3)	(0.3)	(0.3)
$1_{\{0.8 \leq \widehat{\text{LTV}} < 1\}} \times Below$		0.1	0.4*	-0.4	0.1
		(0.2)	(0.2)	(0.3)	(0.2)
$1_{\{1 \leq \widehat{\text{LTV}} < 1.5\}} \times Below$		0.02	0.4**	-0.4	0.1
		(0.2)	(0.2)	(0.3)	(0.2)
Cross-Sectional Variable					
$1_{\{0.8 \leq \widehat{\text{LTV}} < 1\}} \times [Above - Below]$		-0.9	-0.8*	1.2**	-0.2
$1_{\{1 \leq \widehat{\text{LTV}} < 1.5\}} \times [Above - Below]$		-0.9	-0.7*	1.1*	-0.3
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	14,031,645	14,031,645	14,031,645
R^2	0.35	0.35	0.35	0.344	0.35

Table 10 contd

Panel B: Placebo					
	Job Change				
	(1)	(2)	(3)	(4)	(5)
$\mathbf{1}_{\widehat{\{0.8\leq \text{LTV}<1\}}}$	0.2				
	(0.3)				
$\mathbf{1}_{\widehat{\{1.0\leq \text{LTV}<1.5\}}}$	-0.1				
	(0.3)				
$1_{\widehat{\{0.8\leq \text{LTV}<1\}}} \times Above$		0.4	0.1	-0.2	0.4
		(0.5)	(0.5)	(0.5)	(0.4)
$1_{\widehat{\{1\leq \text{LTV}<1.5\}}} \times Above$		0.3	0.1	-0.3	-0.3
		(0.3)	(0.4)	(0.8)	(0.6)
$1_{\widehat{\{0.8\leq \text{LTV}<1\}}} \times Below$		-0.2	0.1	0.1	0.3
		(0.8)	(0.4)	(0.5)	(0.4)
$1_{\widehat{\{1\leq \text{LTV}<1.5\}}} \times Below$		-0.2	0.3	-0.1	-0.2
		(0.7)	(0.7)	(0.7)	(0.7)
Cross-Sectional Variable		Unused Credit	Credit Score	Industry Jobs	Non-Compete
$1_{\widehat{\{0.8\leq \text{LTV}<1\}}} \times [Above - Below]$		0.6	0.0	-0.3	0.1
$1_{\widehat{\{1\leq \text{LTV}<1.5\}}} \times [Above - Below]$		0.5	-0.2	-0.2	-0.1
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes
Observations	11,527,432	11,527,432	11,527,432	11,527,432	11,527,432
R^2	0.521	0.521	0.521	0.521	0.521

Table 11: Home Equity & Income : Debt Overhang Channel

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on the number of hours worked for hourly wage workers (Panel A) and the fraction of variable pay for salaried workers (Panel B):

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode$\times$month fixed effects, δ_{ct} are purchase cohort$\times$month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables include number of hours worked in Panel A and percentage of variable pay (i.e. sum of bonus, commissions and overtime as a fraction of total pay) in Panel B. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

Panel A: Hourly Wage Workers		
	Number of Hours Worked	
	(1)	(2)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	0.49	0.68
	(0.72)	(0.68)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	-0.06	-0.05
	(0.52)	(0.51)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	0.02	0.02
	(0.52)	(0.51)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-0.17	-0.24
	(0.54)	(0.53)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	0.30	0.35
	(0.93)	(0.93)
Tenure and Age Controls	Yes	Yes
Individual FE	Yes	Yes
Zipcode$\times$Month FE	Yes	Yes
Purchase cohort$\times$Month FE	Yes	Yes
Observations	1,521,496	1,479,958
R^2	0.94	0.944

Table 11 contd

Panel B: Salaried Workers				
	Percentage of Variable Pay			
	(1)	(2)	(3)	(4)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	0.3	0.3	0.3	0.4
	(0.2)	(0.2)	(0.2)	(0.3)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	0.1	0.1	0.03	0.1
	(0.1)	(0.2)	(0.1)	(0.2)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-0.1	-0.1	-0.1	-0.2
	(0.1)	(0.2)	(0.1)	(0.2)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	0.2	0.3	0.2	0.2
	(0.1)	(0.2)	(0.1)	(0.2)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	0.4**	0.9***	0.4**	0.9***
	(0.2)	(0.2)	(0.2)	(0.2)
Tenure and Age Controls	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes
Observations	12,379,574	7,785,707	11,720,948	7,345,223
R^2	0.671	0.713	0.683	0.723

Table 12: Home equity and labor income in current job

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income where we use a sample which drops individuals after they change their jobs:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	87.3	1.1	1.3
	(75.5)	(1.0)	(0.8)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	58.4	0.7	0.9
	(70.1)	(0.7)	(0.8)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-267.2***	-1.3**	-2.0***
	(58.2)	(0.6)	(0.7)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-351.6***	-1.9***	-2.8***
	(58.1)	(0.6)	(0.7)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	-171.6***	-1.2**	-1.8***
	(59.7)	(0.6)	(0.7)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434
R^2	0.926	0.958	0.762

Table 13: Home equity and probability of attrition

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on probability of attrition:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variable includes a dummy variable that takes a value of one for the last month that we can observe the individual in the data before she drops out because of not being employed within the employers in our data. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Attrition (1)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	-0.002
	(0.002)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	-0.001
	(0.001)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-0.002
	(0.002)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	0.001
	(0.001)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	0.003
	(0.003)
Tenure and Age Controls	Yes
Individual FE	Yes
Zipcode×Month FE	Yes
Purchase cohort×Month FE	Yes
Observations	16,248,320
R^2	0.089

Table 14: Robustness : Dropping observations in the neighborhood of LTV bucket cut-offs

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income after dropping LTV values that are in the neighborhood of LTV bucket cut-offs:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	92.1	0.7	1.1
	(83.1)	(0.9)	(1.0)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	87.3	1.0	-0.1
	(73.5)	(0.8)	(0.9)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-160.3**	0.04	-2.4***
	(62.9)	(0.7)	(0.8)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-255.4***	-1.4**	-3.7***
	(62.7)	(0.7)	(0.8)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	-170.4**	-0.9	-2.6***
	(64.1)	(0.7)	(0.8)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	11,840,432	11,840,432	11,378,794
R^2	0.893	0.92	0.749

Figure 1: Distribution of Individuals Across States as of Jan, 2010

This figure compares the distribution of individuals in our sample across states in the U.S. to the same distribution of entire population (as of 2010) based on location of residence. The numbers in the figure represent percentage difference in this distribution, i.e. $\frac{StatePopulation_s}{TotalPopulation_s} - \frac{StatePopulation}{TotalPopulation}$.

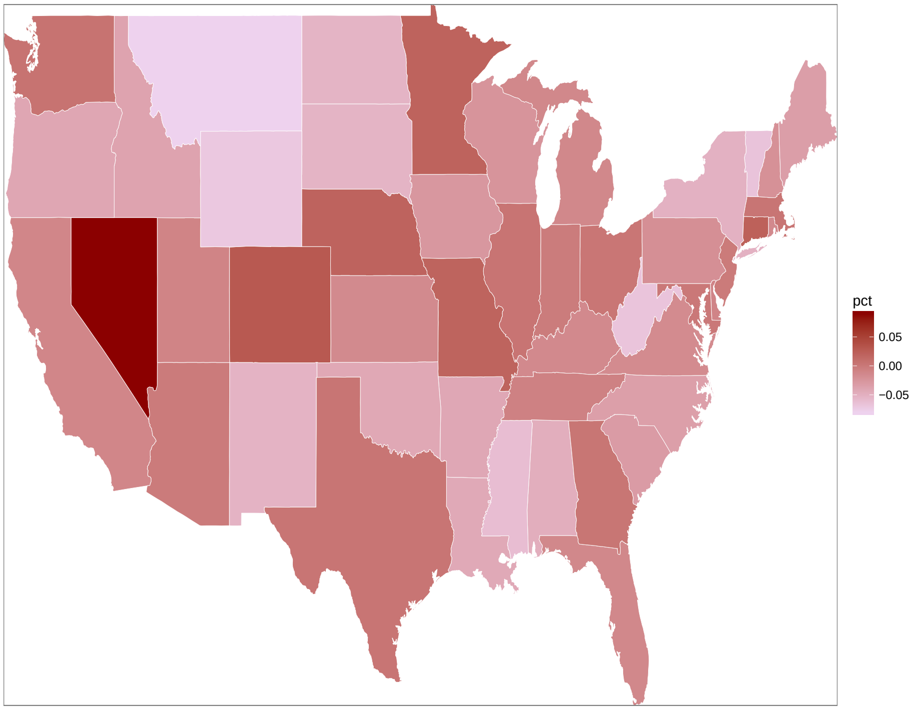


Figure 2: Purchase Year Distribution

This figure illustrates the distribution of purchase year in our sample. The horizontal axis represents year while the vertical axis represents the number of purchases.

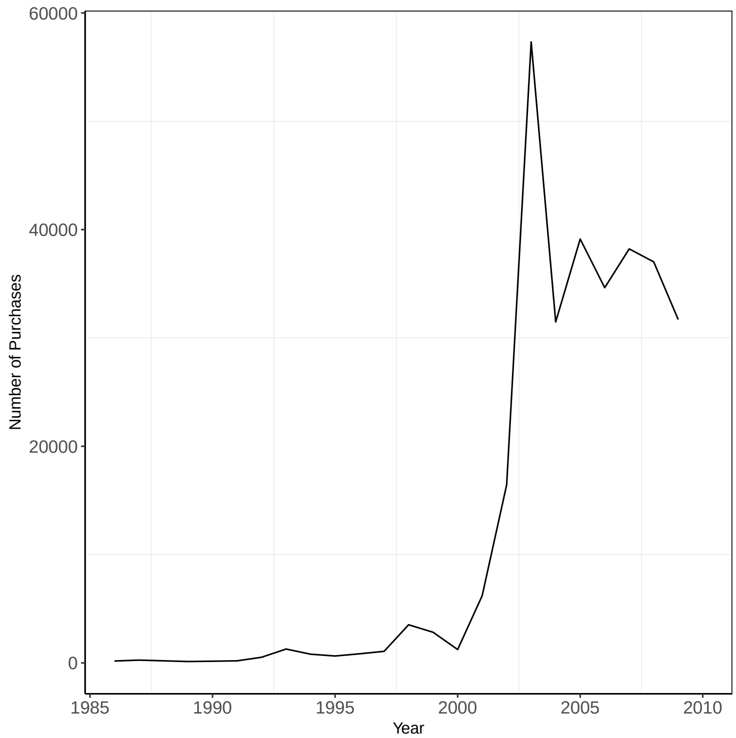
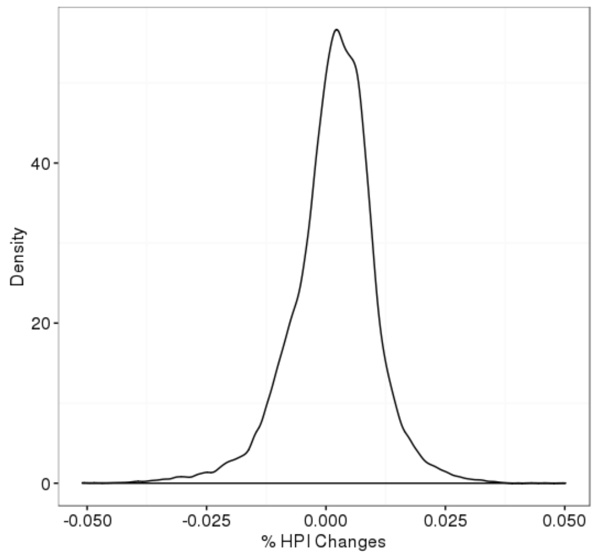
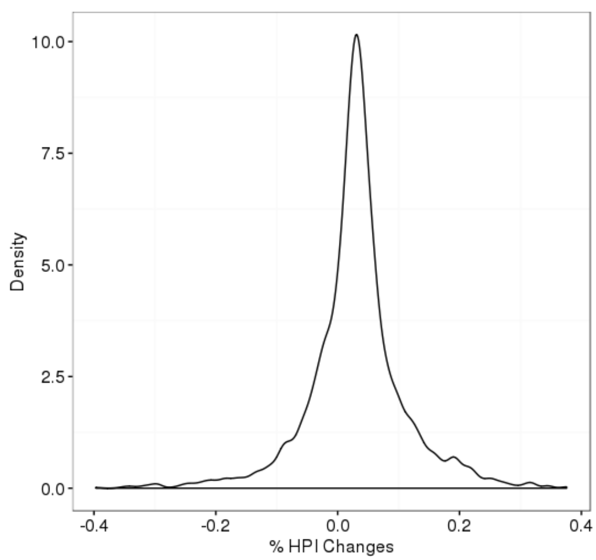


Figure 3: HPI Changes

This figure illustrates the distribution of monthly and annual changes in house price index (HPI) from Corelogic. The plots suggest that there is ample variation in HPI during our sample period.



Panel A: Monthly HPI Changes



Panel B: Annual HPI Changes

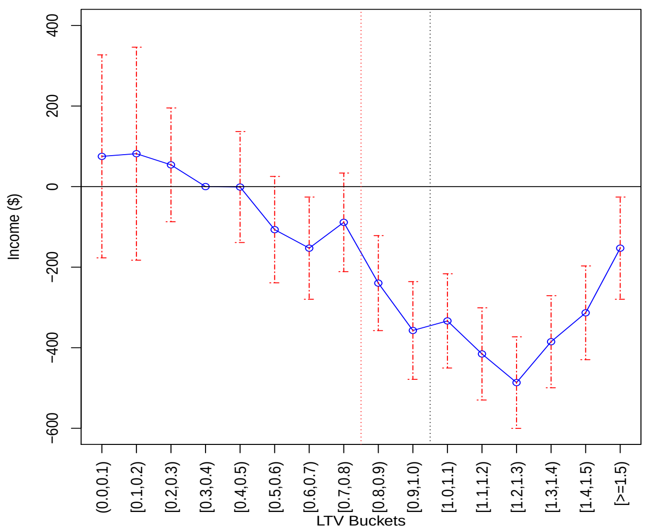
Figure 4: Home Equity & Income

This figure plots the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income:

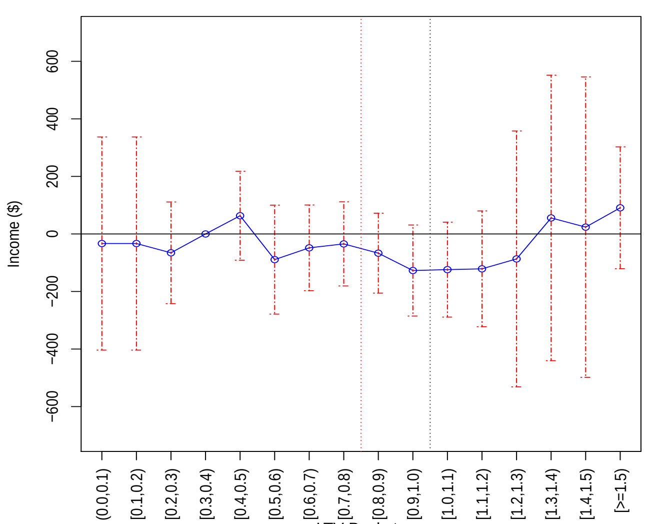
$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dots represent the estimates from the regression while the vertical bars represent standard errors at 95% level that are clustered at the zipcode level. Panel A plots estimates for our main sample while Panel B plots estimates for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters').



Panel A: Main Sample



Panel B: Placebo Sample

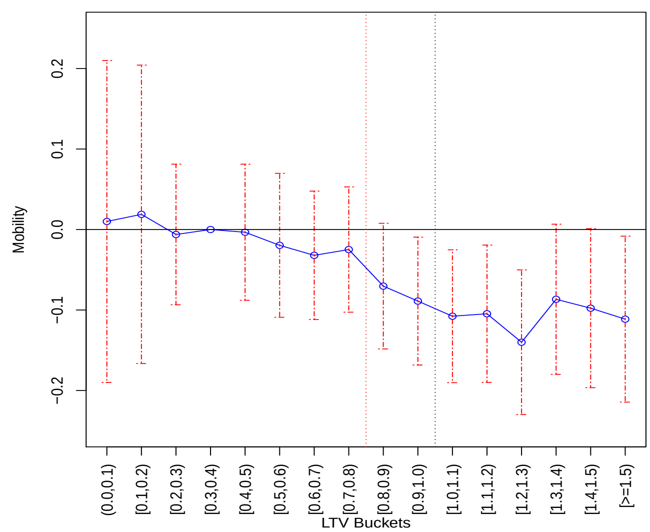
Figure 5: Home Equity & Mobility

This figure plots the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor mobility:

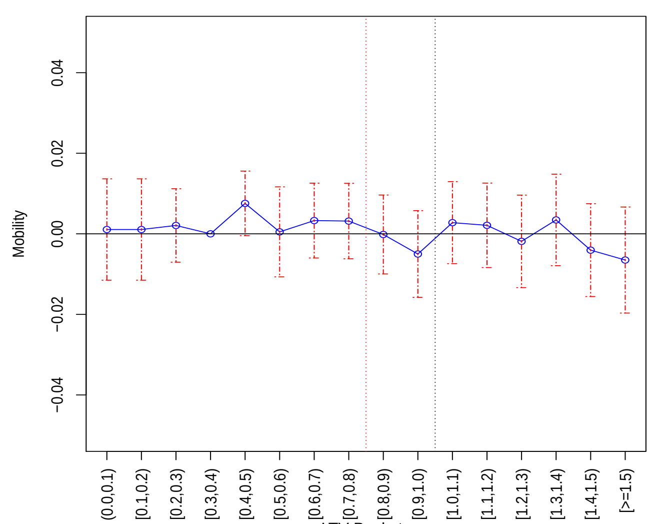
$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dots represent the estimates from the regression while the vertical bars represent standard errors at 95% level that are clustered at the zipcode level. Panel A plots estimates for our main sample while Panel B plots estimates for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). All coefficients and standard errors are scaled by 100 for the ease of interpretation.



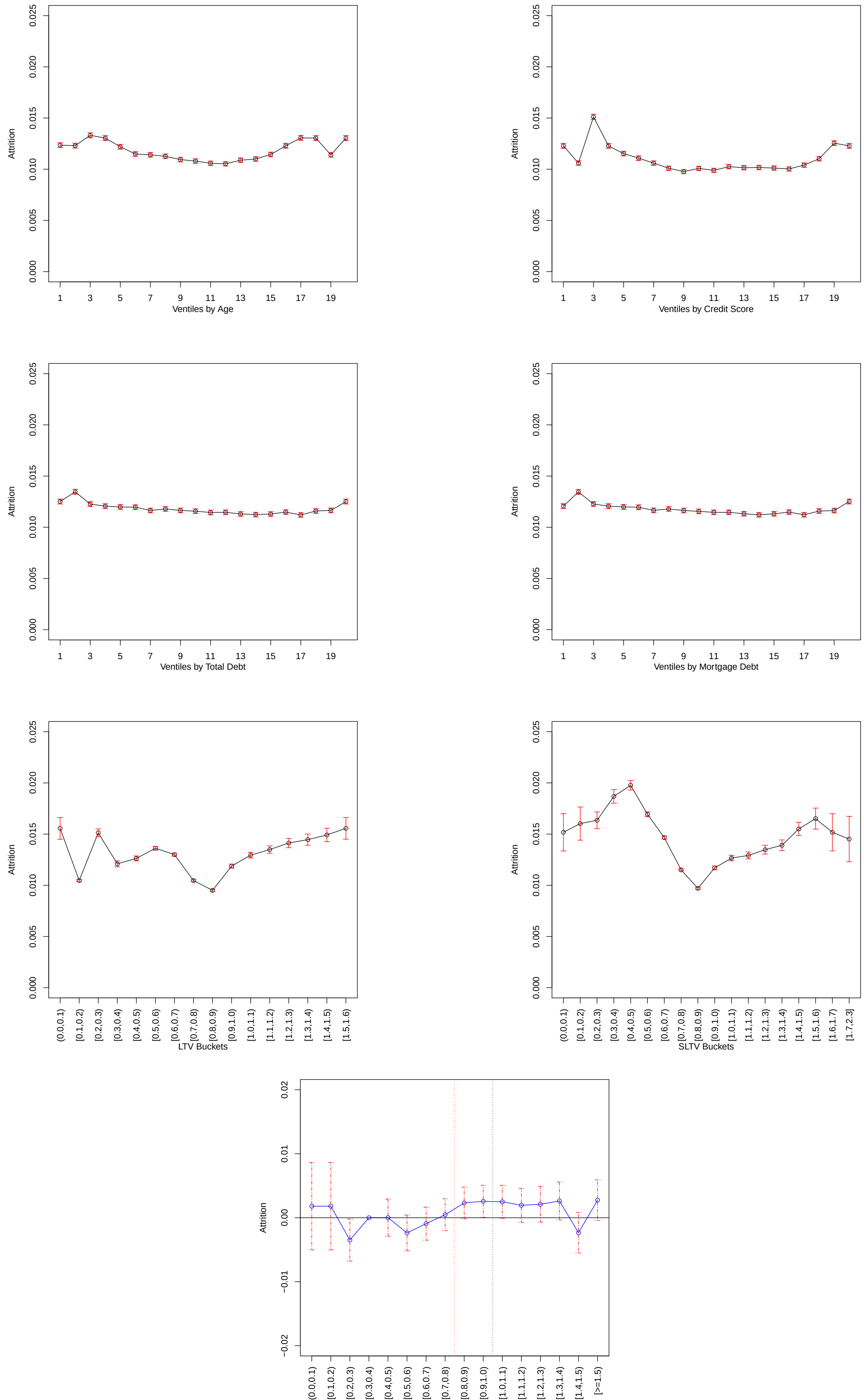
Panel A: Main Sample



Panel B: Placebo Sample

Figure 6: Attrition from Employment Data

This figure plots the mean attrition rate from the employment data by different characteristics. The first row plots mean attrition rate for different ventiles of age and credit score while the second row plots it for different ventiles of total debt and mortgage debt. The third row plots these means for different LTV and SLTV buckets. The last row plots the coefficients of regressions similar to previous two figures with a dummy variable for attrition as an outcome variable.



Internet Appendix (IA): Not Intended for Publication

Table IA1: Robustness : Excluding individuals after they refinance their mortgage

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income for the sample that drops observations after individuals refinance:

$$1_{\{l_k \leq \mathrm{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \mathrm{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\mathrm{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\mathrm{LTV}} < 0.3\}}$	84.3	1.2	1.5
	(72.4)	(0.8)	(1.0)
$\mathbf{1}_{\{0.4 \leq \widehat{\mathrm{LTV}} < 0.8\}}$	-92.8	0.2	-0.04
	(70.1)	(0.7)	(0.8)
$\mathbf{1}_{\{0.8 \leq \widehat{\mathrm{LTV}} < 1\}}$	-249.9***	-1.6***	-2.3***
	(58.2)	(0.6)	(0.7)
$\mathbf{1}_{\{1 \leq \widehat{\mathrm{LTV}} < 1.5\}}$	-336.8***	-2.4***	-3.3***
	(58.1)	(0.6)	(0.7)
$\mathbf{1}_{\{1.5 \leq \widehat{\mathrm{LTV}}\}}$	-156.2***	-1.6***	-2.2**
	(59.6)	(0.6)	(0.7)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434
R^2	0.926	0.958	0.762

Table IA2: Robustness : Controlling for msa x cohort x time effects

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income after including msa x cohort x time fixed effects:

$$1_{\{l_k \leq \mathrm{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{mct} + \sum_k \alpha_k \times 1_{\{l_k \leq \mathrm{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczmt}$$

$$y_{iczmt} = \delta_i + \delta_{zt} + \delta_{mct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\mathrm{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczmt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z (m), the zipcode (msa) where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{mct} are msa×purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	%$\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\mathrm{LTV}} < 0.3\}}$	121.76	1.60*	1.1
	(85.26)	(0.96)	(1.20)
$\mathbf{1}_{\{0.4 \leq \widehat{\mathrm{LTV}} < 0.8\}}$	11.49	0.4	-1.8
	(75.85)	(0.75)	(1.30)
$\mathbf{1}_{\{0.8 \leq \widehat{\mathrm{LTV}} < 1\}}$	-290.84***	-2.7***	-3.6**
	(66.43)	(0.65)	(0.76)
$\mathbf{1}_{\{1 \leq \widehat{\mathrm{LTV}} < 1.5\}}$	-391.92***	-3.3***	-4.7***
	(66.65)	(0.66)	(0.78)
$\mathbf{1}_{\{1.5 \leq \widehat{\mathrm{LTV}}\}}$	-195.93***	-3.7***	-2.9***
	(63.33)	(0.77)	(0.91)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
MSA x Cohort x Month FE	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434
R^2	0.926	0.958	0.764

Table IA3: Non-Compete Enforceability Index

This table reports the non-compete enforceability index from Garmaise [2011].

State	Score	State	Score
Alabama	5	Montana	2
Alaska	3	Nebraska	4
Arizona	3	Nevada	5
Arkansas	5	New Hampshire	2
California	0	New Jersey	4
Colorado	2	New Mexico	2
Connecticut	3	New York	3
Delaware	6	North Carolina	4
District of Columbia	7	North Dakota	0
Florida	9	Ohio	5
Georgia	5	Oklahoma	1
Hawaii	3	Oregon	6
Idaho	6	Pennsylvania	6
Illinois	5	Rhode Island	3
Indiana	5	South Carolina	5
Iowa	6	South Dakota	5
Kansas	6	Tennessee	7
Kentucky	6	Texas	3
Louisiana	4	Utah	6
Maine	4	Vermont	5
Maryland	5	Virginia	3
Massachusetts	6	Washington	5
Michigan	5	West Virginia	2
Minnesota	5	Wisconsin	3
Mississippi	4	Wyoming	4
Missouri	7		

Table IA4: Home Equity and Income gains with inter-MSA job change : Controlling for Income in Previous Job

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on income gains associated with changing jobs across different MSAs:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_{zt} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_{zt} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_{zt} are zipcode×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} include quadratic controls for individual's tenure at the firm, her age and income in the previous job. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the log-changes and percentage changes in income for the new job relative to the old job. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors are scaled by 100 for the ease of interpretation.

	$Log(\frac{Income_t}{Income_{t-1}})$	$\%\Delta Income$
	(1)	(2)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	0.3	0.4
	(0.6)	(0.6)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	0.5	0.5
	(0.4)	(0.4)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	0.7*	0.9**
	(0.4)	(0.4)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	0.9**	1.1***
	(0.4)	(0.4)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	2.0***	2.5***
	(0.7)	(0.6)
Sample	Job Changes outside MSA	
Tenure and Age Controls	Yes	Yes
Zipcode×Month FE	Yes	Yes
Observations	63,393	63,393
R^2	0.009	0.009

Table IA5: Debt Overhang vs Mobility Channel: Alternate definition for job change without changing residence

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on the likelihood that individuals change the firm they are employed at but not their residence in the month of job change, and heterogeneity in this effect based on access to liquidity and market conditions:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{ir-1} < h_k}\}} \times Above + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{ir-1} < h_k}\}} \times Below + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ indicate different LTV value buckets which take a value of one when the loan-to-value ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variable y_{iczt} is $JobChange$ which is defined as a dummy variable that takes a value of one for months when the firm of individual's employment changes but her residence remains the same. *Above* (*Below*) is a dummy variable that takes a value of one for individuals with above (below) median levels of access to liquidity and market conditions based on the cross-sectional measures reported in different columns. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Panel A reports results for our main sample while Panel B reports results for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). All coefficients and standard errors are scaled by 100 for the ease of interpretation.

	Job Change				
	(1)	(2)	(3)	(4)	(5)
$\mathbf{1}_{\{\mathbf{0.8} \leq \widehat{\text{LTV}} < \mathbf{1}\}}$	0.1				
	(0.2)				
$\mathbf{1}_{\{\mathbf{1.0} \leq \widehat{\text{LTV}} < \mathbf{1.5}\}}$	0.2**				
	(0.1)				
$1_{\{0.8 \leq \widehat{\text{LTV} < 1}\}} \times Above$		-1.2	-0.3	0.9***	-0.3
		(1.5)	(0.3)	(0.2)	(0.4)
$1_{\{1 \leq \widehat{\text{LTV} < 1.5}\}} \times Above$		-1.1	-0.6	1.0***	-0.1
		(1.5)	(0.4)	(0.2)	(0.3)
$1_{\{0.8 \leq \widehat{\text{LTV} < 1}\}} \times Below$		0.1	0.4**	-0.1	0.2
		(0.2)	(0.2)	(0.3)	(0.3)
$1_{\{1 \leq \widehat{\text{LTV} < 1.5}\}} \times Below$		0.1	0.5*	-0.2	0.3
		(0.2)	(0.3)	(0.3)	(0.2)
Cross-Sectional Variable		Unused Credit	Credit Score	Industry Jobs	Non-Compete
$1_{\{0.8 \leq \widehat{\text{LTV} < 1\}} \times [}Above - Below]$		-1.3	-0.7*	1.0***	-0.5
$1_{\{1 \leq \widehat{\text{LTV} < 1.5\}} \times [}Above - Below]$		-1.2	-1.1**	1.2***	-0.4
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	14,031,645	14,031,645	14,031,645
R^2	0.375	0.375	0.375	0.375	0.385

Table IA6: Home Equity and Income gains with job changes

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on income gains associated with changing jobs:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_{zt} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_{zt} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_{zt} are zipcode×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} include quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the log-changes and percentage changes in income for the new job relative to the old job. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors are scaled by 100 for the ease of interpretation.

	$Log(\frac{Income_t}{Income_{t-1}})$	$\%\Delta Income$
	(1)	(2)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	1.8	2.5
	(1.5)	(1.7)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	-0.9	-0.7
	(1.2)	(1.3)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-2.0**	-1.9*
	(1.0)	(1.2)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-2.0**	-1.9*
	(1.0)	(1.1)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	-1.7	-1.7
	(1.0)	(1.1)
Sample	All Job Changes	
Tenure and Age Controls	Yes	Yes
Zipcode×Month FE	Yes	Yes
Observations	94,753	94,753
R^2	0.007	0.010

Table IA7: Robustness : Dropping Delinquent Borowers

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income after dropping the delinquent borrowers from the sample:

$$1_{\{l_k \leq \mathrm{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \mathrm{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\mathrm{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	%$\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\mathrm{LTV}} < 0.3\}}$	106.39	1.1	1.7*
	(79.56)	(0.8)	(1.0)
$\mathbf{1}_{\{0.4 \leq \widehat{\mathrm{LTV}} < 0.8\}}$	52.10	0.5	0.7
	(76.80)	(0.8)	(0.9)
$\mathbf{1}_{\{0.8 \leq \widehat{\mathrm{LTV}} < 1\}}$	-317.26***	-1.5**	-2.0***
	(63.18)	(0.6)	(0.8)
$\mathbf{1}_{\{1 \leq \widehat{\mathrm{LTV}} < 1.5\}}$	-389.81***	-2.1***	-3.0***
	(63.04)	(0.6)	(0.8)
$\mathbf{1}_{\{1.5 \leq \widehat{\mathrm{LTV}}\}}$	-214.12***	-1.3**	-1.9**
	(64.81)	(0.7)	(0.8)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	12,832,166	12,832,166	12,390,853
R^2	0.926	0.959	0.762

Table IA8: Robustness : Confining to Individuals that Stay in Employment Data though Sample Period

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income for the sample of individuals who stay within our employment data through sample period:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	41.3	1.0	1.1
	(72.2)	(0.9)	(1.0)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	83.9	0.6	0.8
	(79.4)	(0.9)	(1.1)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-248.9***	-1.5**	-2.3***
	(62.2)	(0.7)	(0.8)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-343.9***	-2.3***	-3.3***
	(62.4)	(0.7)	(0.8)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	-136.6**	-1.4*	-2.0**
	(64.5)	(0.7)	(0.9)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	8,131,265	8,131,265	7,781,890
R^2	0.915	0.948	0.753

Table IA9: LTV Distribution

This table compares our LTV distribution to those from Gerardi et al (2018) using data that they make publicly available.

	Our Sample		CRISM 1		CRISM 2	
	2011	2013	2011	2013	2011	2013
LTV <= 0.8	47.5%	65.4%	50.3%	64.5%	45.1%	62.2%
0.8 < LTV <= 1.0	34.2%	27.3%	27.5%	26.5%	33.3%	30.3%
LTV > 1.0	18.3%	7.3%	22.2%	9.0%	21.7%	7.5%

Table IA10: Robustness : Specification with only two LTV buckets

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income with one dummy variable:

$$1_{\{l_k \leq \mathrm{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \mathrm{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{\widehat{l_k \leq \mathrm{LTV}_{it-1} < h_k}\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation. Panel A reports results for the entire sample where Panel B reports results after dropping observations with $LTV \in [0.7, 0.9]$.

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{\widehat{0.8 \leq \mathrm{LTV}}\}}$	-282.1***	-0.7***	-2.0***
	(8.5)	(0.1)	(0.1)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434
R^2	0.893	0.921	0.74

Panel A: Full Sample

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{\widehat{0.8 \leq \mathrm{LTV}}\}}$	-240.3***	-1.0***	-2.6***
	(29.4)	(0.3)	(0.4)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	9,855,465	9,855,465	9,537,860
R^2	0.893	0.921	0.74

Panel B: Dropping observations with $LTV \in [0.7, 0.9]$

Table IA11: Robustness : Sub-sample of Individuals with Low Income as of Jan, 2010

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income for the sub-sample of individuals with below median levels of income as of 2010:

$$1_{\{l_k \le \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \le \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \le \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \le LTV_{it-1} \le h_k\}}$ ($1_{\{l_k \le SLTV_{it-1} \le h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \le \widehat{\text{LTV}} < 0.3\}}$	51.3	0.8	0.9
	(57.8)	(1.0)	(1.1)
$\mathbf{1}_{\{0.4 \le \widehat{\text{LTV}} < 0.8\}}$	25.5	-0.4	-0.7
	(52.5)	(0.9)	(1.1)
$\mathbf{1}_{\{0.8 \le \widehat{\text{LTV}} < 1\}}$	-254.9***	-1.5**	-2.0**
	(43.3)	(0.7)	(0.9)
$\mathbf{1}_{\{1 \le \widehat{\text{LTV}} < 1.5\}}$	-279.6***	-1.9***	-2.9***
	(41.6)	(0.7)	(0.9)
$\mathbf{1}_{\{1.5 \le \widehat{\text{LTV}}\}}$	-40.6	-0.4	-2.4***
	(41.5)	(0.9)	(1.0)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
	14,314,520	14,314,520	13,773,814
Observations	7,081,254	7,081,254	7,014,024
R^2	0.891	0.91	0.79

Table IA12: Robustness : Sub-sample of Individuals with a Single Mortgage

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income for the sub-sample of individuals who never originate HELOC, home improvement or other home equity loans:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	93.7	1.3	1.4
	(73.4)	(0.8)	(0.9)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	-85.2	0.2	-0.1
	(70.6)	(0.8)	(0.9)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-227.9***	-1.3**	-1.9***
	(58.7)	(0.6)	(0.7)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-310.7***	-1.9***	-2.9***
	(58.6)	(0.6)	(0.7)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	-137.7**	-1.2**	-2.0***
	(60.2)	(0.6)	(0.7)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	13,605,800	13,605,800	13,093,508
R^2	0.927	0.958	0.752

Table IA13: Robustness : Alternate values for LTV at origination

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income where we use different assumptions to calculate LTV:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. In Panel A we compute LTV assuming different origination LTV values for individuals originating only one mortgage account. In Panel B we compute LTV using zipcode-level house prices for a subsample of individuals residing in homogenous zipcodes. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

Panel A : LTV at Origination Values						
	Income ($) (1)	Log(Income) (2)	$\%\Delta Income$ (3)	Income ($) (4)	Log(Income) (5)	$\%\Delta Income$ (6)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	89.8	1.1	1.4	69.2	0.9	0.9
	(63.2)	(0.8)	(1.1)	(71.4)	(0.8)	(0.9)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	-81.1	0.1	0.1	91.2	0.3	0.1
	(79.6)	(0.7)	(0.9)	(69.9)	(0.7)	(0.8)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-322.4***	-2.2***	-2.9***	-257.6***	-1.3**	-1.7***
	(48.2)	(0.5)	(0.6)	(38.9)	(0.4)	(0.3)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-419.5***	-2.4***	-3.4***	-403.8***	-2.2***	-3.2***
	(51.8)	(0.5)	(0.5)	(39.1)	(0.4)	(0.5)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	-269.7***	-1.7***	-2.6***	-256.7***	-1.6***	-2.4***
	(52.3)	(0.5)	(0.6)	(38.7)	(0.3)	(0.4)
Origination LTV Values	0.75	0.75	0.75	0.85	0.85	0.85
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	14,031,645	14,031,645	13,506,434
R^2	0.926	0.958	0.762	0.926	0.958	0.762

Table IA13 (contd)

Panel B : Zip code Level House Prices for Homogeneous zip codes			
	Income ($)	Log(Income)	%$\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0\leq\widehat{\text{LTV}}<0.3\}}$	31.4	0.2	1.1
	(95.1)	(0.8)	(1.1)
$\mathbf{1}_{\{0.4\leq\widehat{\text{LTV}}<0.8\}}$	62.8	-0.2	-0.2
	(104.3)	(0.8)	(0.9)
$\mathbf{1}_{\{0.8\leq\widehat{\text{LTV}}<1\}}$	-134.6	-1.7*	-2.3**
	(101.7)	(1.0)	(1.0)
$\mathbf{1}_{\{1\leq\widehat{\text{LTV}}<1.5\}}$	-359.7***	-2.0**	-2.5**
	(112.8)	(0.9)	(1.1)
$\mathbf{1}_{\{1.5\leq\widehat{\text{LTV}}\}}$	-184.5*	-1.1	-1.7*
	(103.2)	(0.9)	(0.9)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	1,571,973	1,571,973	1,521,667
R^2	0.925	0.959	0.761

Table IA14: Robustness : Instrument Construction

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income where we use different assumptions on interest rates and maturity to construct the instrument:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. In Panel A we compute $SLTV$ using the median interest rate during the year of the purchase as the interest rate for all mortgages originated in that year. In Panel B we compute $SLTV$ using median interest rate and maturity based on the state and year of mortgage origination. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

Panel A : Time Varying Interest Rate Values

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	77.1	0.9	0.8
	(72.4)	(0.8)	(1.0)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	-15.8	0.4	-0.5
	(29.6)	(0.6)	(1.0)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-306.9***	-1.4***	-2.4***
	(34.5)	(0.3)	(0.5)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-381.6***	-2.0***	-3.3***
	(32.9)	(0.4)	(0.6)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	-211.4***	-1.3***	-2.4***
	(32.7)	(0.3)	(0.5)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434
R^2	0.926	0.958	0.762

Table IA14 (contd)

Panel B : Interest Rate and Maturity by State and Year of Origination			
	Income ($)	Log(Income)	%$\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0\leq \widehat{LTV}<0.3\}}$	52.6	1.3	1.4
	(108.7)	(1.4)	(1.6)
$\mathbf{1}_{\{0.4\leq \widehat{LTV}<0.8\}}$	-33.9	-0.2	-0.08
	(65.5)	(0.9)	(0.3)
$\mathbf{1}_{\{0.8\leq \widehat{LTV}<1\}}$	-365.7***	-1.6***	-2.3***
	(42.1)	(0.4)	(0.5)
$\mathbf{1}_{\{1\leq \widehat{LTV}<1.5\}}$	-439.2***	-2.2***	-3.2***
	(40.4)	(0.4)	(0.5)
$\mathbf{1}_{\{1.5\leq \widehat{LTV}\}}$	-279.9***	-1.6***	-2.3***
	(38.5)	(0.4)	(0.5)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434
R^2	0.926	0.958	0.762

Table IA15: Using an Alternative Instrument from Bernstein and Struyven, 2017

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_{zt} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

$$y_{iczt} = \delta_{zt} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ $(1_{\{l_k \leq SLTV_{it-1} \leq h_k\}})$ indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$ where $SLTV$ is defined as in Berstein and Struyven (2016), and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. 2 Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Panel A reports results on the first stage while Panel B reports findings from the second stage. Columns (1) through (3) report results for our main sample while the remaining columns report results for the placebo sample which consists of individuals who reside in the same zipcode and work for the same firm with the same job role as individuals in the main sample but do not have an open mortgage account in their name ('renters'). Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

Panel A: First stage regression					
	$\mathbf{1}_{\{0 \leq \text{LTV} < 0.3\}}$ (1)	$\mathbf{1}_{\{0.4 \leq \text{LTV} < 0.8\}}$ (2)	$\mathbf{1}_{\{0.8 \leq \text{LTV} < 1\}}$ (3)	$\mathbf{1}_{\{1 \leq \text{LTV} < 1.5\}}$ (4)	$\mathbf{1}_{\{1.5 \leq \text{LTV}\}}$ (5)
$\mathbf{1}_{\{0 \leq SLTV < 0.3\}}$	0.093***	0.115***	0.009*	0.009***	0.0004
	(0.014)	(0.009)	(0.005)	(0.002)	(0.0003)
$\mathbf{1}_{\{0.4 \leq SLTV < 0.8\}}$	-0.271***	0.034***	0.014***	0.002	0.001**
	(0.01)	(0.008)	(0.003)	(0.001)	(0.0002)
$\mathbf{1}_{\{0.8 \leq SLTV < 1\}}$	-0.272***	-0.350***	0.423***	-0.002	-0.001**
	(0.01)	(0.009)	(0.004)	(0.001)	(0.0003)
$\mathbf{1}_{\{1 \leq SLTV < 1.5\}}$	-0.263***	-0.639***	0.281***	0.436***	-0.001***
	(0.01)	(0.009)	(0.004)	(0.004)	(0.0004)
$\mathbf{1}_{\{1.5 \leq SLTV\}}$	-0.248***	-0.694***	0.091***	0.323***	0.339***
	(0.01)	(0.01)	(0.005)	(0.008)	(0.006)
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	14,031,645	14,031,645	14,031,645
F-Statistic	0.656	0.67	0.592	0.747	0.552

Table IA15 (contd)

Panel B: Second stage regression						
	Main Sample			Placebo		
	Income ($)	Log(Income)	%$\Delta Income$	Income ($)	Log(Income)	%$\Delta Income$
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbf{1}_{\{0\leq \widehat{\text{LTV}}<0.3\}}$	31.73	1.3	2.3	-131.3	-0.6	0.7
	(73.96)	(1.2)	(2.5)	(126.2)	(1.9)	(2.2)
$\mathbf{1}_{\{0.4\leq \widehat{\text{LTV}}<0.8\}}$	-24.15	-1.4	-1.9	-69.4	-1.3	-1.3
	(43.81)	(1.3)	(1.3)	(67.5)	(1.5)	(1.9)
$\mathbf{1}_{\{0.8\leq \widehat{\text{LTV}}<1\}}$	-229.03***	-2.3***	-3.6**	-88.4	-1.2	-1.5
	(70.50)	(0.7)	(1.7)	(79.2)	(1.5)	(1.8)
$\mathbf{1}_{\{1\leq \widehat{\text{LTV}}<1.5\}}$	-303.24***	-3.3***	-3.8***	-71.1	-1.4	-1.7
	(71.68)	(0.6)	(1.5)	(75.3)	(1.5)	(1.9)
$\mathbf{1}_{\{1.5\leq \widehat{\text{LTV}}\}}$	-144.84**	-0.5	-2.4*	-39.1	-0.7	-0.9
	(70.38)	(0.7)	(1.3)	(89.4)	(1.6)	(2.1)
Tenure and Age Controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434	8,523,618	8,523,618	8,216,384
R^2	0.926	0.958	0.762	0.943	0.967	0.826

Table IA16: Robustness : Including zipcode x industry x time fixed effects

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor income:

$$1_{\{l_k \leq \text{LTV}_{it-1} < h_k\}} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \alpha_k \times 1_{\{l_k \leq \text{SLTV}_{it-1} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\text{LTV}_{it-1}} < h_k\}} + X_{it-1}\gamma + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides, j the industry in which she is employed and t is time in year-month, δ_i are individual fixed effects, δ_{zjt} are zipcode×industry×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ ($1_{\{l_k \leq SLTV_{it-1} \leq h_k\}}$) indicate different LTV ($SLTV$) value buckets which take a value of one when the loan-to-value (synthetic loan-to-value) ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1}(SLTV_{it-1}) \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variables y_{iczt} include the dollar value of income, logarithm of income and percentage change in income relative to the beginning of the sample. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Coefficients and standard errors on log of income are scaled by 100 for ease of interpretation.

	Income ($)	Log(Income)	$\%\Delta Income$
	(1)	(2)	(3)
$\mathbf{1}_{\{0 \leq \widehat{\text{LTV}} < 0.3\}}$	105.6	1.2	1.3
	(81.4)	(0.8)	(1.0)
$\mathbf{1}_{\{0.4 \leq \widehat{\text{LTV}} < 0.8\}}$	-77.6	-0.7	-1.5
	(78.4)	(0.8)	(1.0)
$\mathbf{1}_{\{0.8 \leq \widehat{\text{LTV}} < 1\}}$	-236.4***	-1.6**	-2.7***
	(66.9)	(0.7)	(0.9)
$\mathbf{1}_{\{1 \leq \widehat{\text{LTV}} < 1.5\}}$	-323.7***	-2.2***	-3.7***
	(65.1)	(0.7)	(0.9)
$\mathbf{1}_{\{1.5 \leq \widehat{\text{LTV}}\}}$	-194.8***	-1.8***	-3.2***
	(67.5)	(0.7)	(0.9)
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Industry×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	14,031,645	14,031,645	13,506,434
R^2	0.926	0.958	0.762

Table IA17: Robustness : Defining Mobility at the zip code level

This table reports the coefficient estimates from the following IV regressions that estimate the effect of LTV on labor mobility defined at the zip code level, and heterogeneity in this effect based on access to liquidity and credit:

$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\mathrm{LTV}_{it-1}} < h_k\}} + \gamma \times X_{it-1} + \epsilon_{iczt}$$

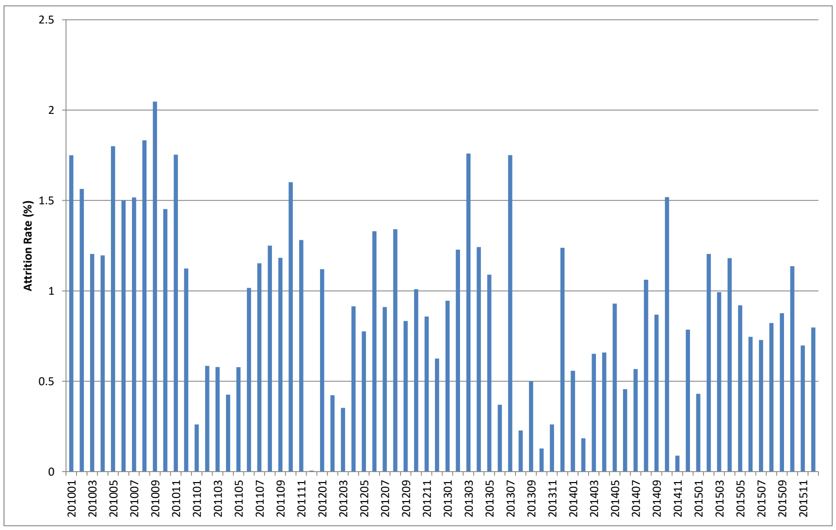
$$y_{iczt} = \delta_i + \delta_{zt} + \delta_{ct} + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\mathrm{LTV}_{ir-1}} < h_k\}} \times Above + \sum_k \beta_k \times 1_{\{l_k \leq \widehat{\mathrm{LTV}_{it-1}} < h_k\}} \times Below + \gamma \times X_{it-1} + \epsilon_{iczt}$$

where the subscript i refers to the individual, c, the purchase cohort to which the individual belongs based on when she bought her house, z, the zipcode where the individual resides and t is time in year-month, δ_i are individual fixed effects, δ_{zt} are zipcode×month fixed effects, δ_{ct} are purchase cohort×month fixed effects, the indicator functions, $1_{\{l_k \leq LTV_{it-1} \leq h_k\}}$ indicate different LTV value buckets which take a value of one when the loan-to-value ratio of an individual's primary residence at the end of month t-1 is between l_k and h_k i.e., $LTV_{it-1} \in (l_k, h_k]$, and X_{it-1} are quadratic controls for individual's tenure at the firm and her age. We exclude LTV bucket (0.3,0.4] as base for comparison. The dependent variable y_{iczt} is $Mobility$ which is defined as a dummy variable that takes a value of one for month t if the zipcode of an individual's residence changes in month t. *Above* (*Below*) is a dummy variable that takes a value of one for individuals with above (below) median levels of access to liquidity based on the cross-sectional measures reported in different columns. Standard errors are clustered at the zipcode level, and are reported in the parantheses below the estimates. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. All coefficients and standard errors are scaled by 100 for the ease of interpretation.

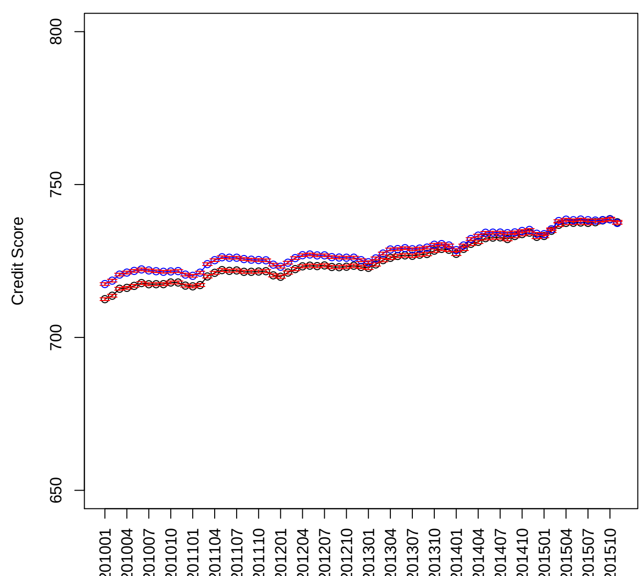
	Mobility (1)	Mobility (2)	Mobility (3)
$\mathbf{1}_{\{\mathbf{0.8} \leq \widehat{\mathrm{LTV}} < \mathbf{1}\}}$	-0.9***		
	(0.1)		
$\mathbf{1}_{\{\mathbf{1.0} \leq \widehat{\mathrm{LTV}} < \mathbf{1.5}\}}$	-1.0***		
	(0.1)		
$\mathbf{1}_{\{\mathbf{0.8} \leq \widehat{\mathrm{LTV}} < \mathbf{1}\}} \times Above$		-0.8	-0.6***
		(1.2)	(0.2)
$\mathbf{1}_{\{\mathbf{1.0} \leq \widehat{\mathrm{LTV}} < \mathbf{1.5}\}} \times Above$		-0.9	-0.7**
		(1.3)	(0.3)
$\mathbf{1}_{\{\mathbf{0.8} \leq \widehat{\mathrm{LTV}} < \mathbf{1}\}} \times Below$		-1.2***	-1.5***
		(0.1)	(0.2)
$\mathbf{1}_{\{\mathbf{1.0} \leq \widehat{\mathrm{LTV}} < \mathbf{1.5}\}} \times Below$		-1.3***	-1.7***
		(0.2)	(0.2)
Cross-Sectional Variable		Unused Credit	Credit Score
$\mathbf{1}_{\{\mathbf{0.8} \leq \widehat{\mathrm{LTV}} < \mathbf{1}\}} \times [Above - Below]$		0.4	0.9***
$\mathbf{1}_{\{\mathbf{1.0} \leq \widehat{\mathrm{LTV}} < \mathbf{1.5}\}} \times [Above - Below]$		0.4	1.0***
Tenure and Age Controls	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Zipcode×Month FE	Yes	Yes	Yes
Purchase cohort×Month FE	Yes	Yes	Yes
Observations	14,031,645	14,031,645	14,031,645
R^2	0.465	0.465	0.465

Figure IA1: Attrition Rate and Characteristics of those who Attrition from Employment Data

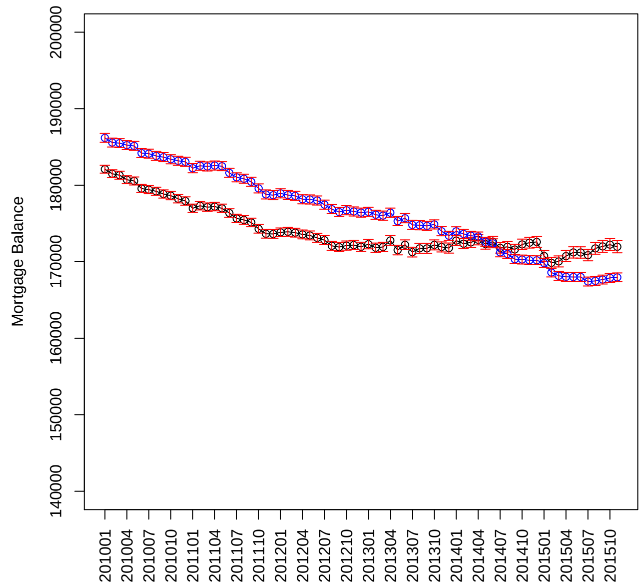
Panel A plots the unconditional attrition rate through time while panels B through D plot credit scores, mortgage balances and non-mortgage balances for homeowners who dropped out of our sample (black color) to those who don't (blue color).



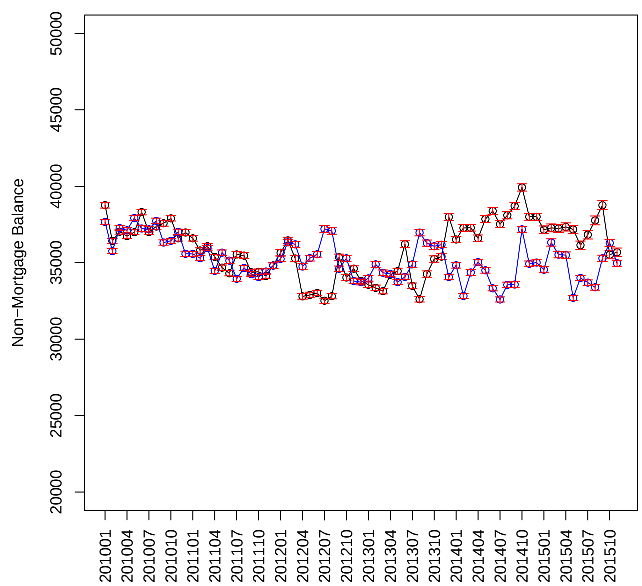
Panel A: Attrition Rate



Panel B: Credit Score



Panel C: Mortgage Balance



Panel D: Non-Mortgage Balance

IA Sample Weights

To facilitate replication of our results, we generate two sets of weights using our data. Our analysis focuses on single-family, owner-occupied employed home-owners as of 2010. The first set of weights split the sample by income decile and states while the second set splits the sample based on income decile, age quintile and state bins. We then compute ratios of population shares in those bins. This will facilitate comparison of our sample to other data sources. The first set of weights including 510 buckets while the second includes 2507 buckets with non-zero weights in our sample.