Negative Equity, Household Debt Overhang, and Labor Supply

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I find that negative home equity causes a 2%-6% reduction in household labor supply. I utilize U.S. household-level data and plausibly exogenous variation in the location-timing of home purchases with a single lender. Supporting causality, households are observationally equivalent at origination and equally sensitive to local housing shocks that don't cause negative equity. Results also hold comparing purchases within the same year-MSA, that differ by only a few months. I then identify a new channel that explains as much as 1/5th of this relationship, "household debt overhang", wherein income-contingent mortgage renegotiations act like implicit taxes, resulting in labor supply disincentives.

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1. Introduction

Following the historic decline in house prices during the recent financial crisis more than 15 million U.S. mortgages, or approximately 1/3rd of mortgaged properties, had negative home equity[1]. At the same time, labor markets experienced a severe and prolonged deterioration, with not just employment, but also labor force participation rates, still below pre-recession levels for years after the crisis. While these declines may have been driven by common factors, previous work (ex. Mian and Sufi 2012; Verner and Gyongyosi 2017) suggests a causal link between employment and housing wealth where house price shocks affect equilibrium employment via local labor demand. What is less well understood is whether negative home equity, caused by house price shocks, may have altered not only labor demand, but also labor supply. If there is a significant relationship between negative home equity and labor supply it could improve our understanding of household financial decision making as well as provide potentially important implications for marcoprudential policies.

My primary contribution in this paper is to provide the first causal empirical estimates of the effect of negative home equity on overall household labor supply. I find that instrumented negative equity is associated with a 2%-6% reduction in household income. I utilize U.S. household-level data and plausibly exogenous variation in the location-timing of home purchases with a single lender. These results are consistent with a number of recent papers indicating that negative home equity could negatively affect labor supply. In particular, evidence suggests a reduction in home equity could reduce entrepreneurship (Adelino, Schoar, and Severino 2015 and Schmalz, Sraer, and Thesmar 2017), innovation and effort (Bernstein, McQuade, and Townsend 2017), employment opportunities among impoverished households (Bos et al. 2015), labor mobility (Ferreira et al. 2010; Ferreira et al. 2012; Foote 2016; Bernstein and Struyven 2017), job search (Brown and Matsa 2017) and labor income among bankrupt households (Dobbie and Song 2015a). While these results are suggestive, the average net effects on labor supply are still ambiguous. Many of the effects are likely to be limited to only a select subset of

[1] According to First American CoreLogic as of June 30, 2009.

homeowners, such as entrepreneurs, innovators, or bankrupt/impoverished[2] households, while the effects of others, such as housing lock and job search, are still heavily debated[3]. There are also other channels, such as wealth effects, which would predict the exact opposite behavior. For example, there is prior evidence that exogenous increases in wealth, via either lottery winnings (Imbens et al. 2001; Cesarini et al. 2017) or inheritance windfalls (Joulfaian and Wilhelm 1994) reduce labor supply. These would predict an increase, rather than a decrease, in labor supply from negative home equity, coming from a reduction in housing wealth. The multitude of potential channels mean the exact nature of the relationship, if it is significant, between negative home equity and labor supply is inevitably an empirical question. The findings in this paper of a positive relationship between housing wealth and labor supply, that occurs non-linearly for households with negative home equity, suggests prior evidence of labor market disruptions coming from housing market frictions have significant economic impacts on labor supply[4].

Empirical identification of the effect of negative home equity on labor supply faces a number of challenges which I address in this paper. First of all, few datasets have comprehensive household-level panel information on income, assets and liabilities. The few databases that do, such as the American Housing Survey (AHS), tend to be surveys that suffer from self-reporting biases and small sample sizes that confound clean identification[5]. Even with appropriate data, simple regressions of labor income on negative home equity are unlikely to provide causal interpretation. A number of omitted variables drive both house prices and labor income (ex. local

[2] For example, Bos et al. 2015 focuses on a sample of households who were delinquent on a loan from a pawnshop within the last two years. Not surprisingly this sample population has very low income. Only 43% are employed and only 6% are homeowners. Credit constraints that prevent this population from finding employment, such as being unable to use a credit card to buy a suit, seem unlikely to extend to the average U.S. homeowner.

[3] In these settings households are financially constrained by negative equity which prevents them from moving, also known as "housing lock". Due to the effectively non-recourse nature of mortgages in the U.S. the effect of housing lock on mobility is unclear and empirical evidence has historically been divided, with papers such as Schulhofer-Wohl (2012) and Mumford and Schultz (2014) finding no evidence of reduced mobility. Modestino and Dennett (2013) also point out that while non-pecuniary costs of immobility could be large, very few households in a given year have to move for employment, so the effect on aggregate labor supply may be limited.

[4] The findings in this paper are also related to recent findings in Sodini et al. (2017), who note that home ownership actually appears to increase labor income in Sweden among movers who take on more debt. In that setting the proposed explanation is that households respond to the need to service a higher level of monthly mortgage payments by working more, which comes from switching from owning to renting, not necessarily a change in home equity directly. The effects on labor supply of negative home equity are likely to differ in many ways from the effect of switching from renting to owning a home, but both may be a least partially driven by some of the aforementioned frictions that exist for homeowners with a significant amount of mortgage debt.

[5] For example, Cunningham and Reed (2012) use AHS data, but only have 652 household-year observations over the course of 9 years with negative equity, which is a very limited sample for something as noisy as self-reported household equity and labor income.

labor demand shocks) and reverse causality could be problematic since wealthier households are likely to invest more in home improvements.

In this paper I overcome these challenges with a new transaction-level dataset with comprehensive information on assets, liabilities, and deposits for all customers of a major U.S. financial institution from 2010-2014, referred to hereafter as *MyBank*, and an empirical methodology based on variation in the timing of housing purchases. The transaction-level deposit information allows me to generate accurate high frequency measures of household income, while the data on assets and liabilities lets me determine which households have negative home equity. Since I observe actual deposits rather than reported values any estimated effects represent actual changes in deposit behavior rather than changes in household reporting in response to eligibility criteria[6].

To overcome issues of identification I exploit plausibly exogenous variation in home equity from the interaction of the location and timing of home purchases, relative to households in the same region, as an instrumental variable for the probability a household has negative home equity. In this empirical strategy households are exposed to identical time-varying local house price shocks, but differ in their home equity based on when they happened to purchase their home relative to their neighbors. Since variation in the timing of home purchases is not randomly assigned I address concerns that omitted variables could be related to the timing of purchase and future income in a way that violates the exclusion restriction of the instrumental variables methodology. First I show that for low levels of expected loan-to-value, house price shocks have little effect, but as the probability of having negative equity rises, labor supply falls, consistent with an explanation driven by negative home equity. I also show that the results are robust to including household fixed effects, controlling flexibly for national cohort trends, and including a number of time-varying non-parametric household-level controls for household characteristics that could be related to local demand shock sensitivity.

[6] Chetty et al. (2013) have shown that in the context of household response to the EITC individuals manipulate self-employment reported income.

There is a growing body of evidence (ex. Mian and Sufi 2009; Palmer 2015) that lending standards may have changed in the run-up to the financial crisis, leading to potentially different sensitivities for households who bought earlier vs. later to local demand shocks. The empirical design in this paper circumvents these concerns by including both region-time and origination date-time fixed effects for a single lender in all specifications. In other words, I compare households that bought properties financed with the same lender at the same time, but did so in different regions and compare them with households who bought at different times in those areas. The key source of variation is that households bought their properties at relatively fortunate or unfortunate times in their specific MSA, relative to their neighbors, but not earlier or later overall. This flexible set of controls means that any observed relationship between the instrumented home equity and labor income, can't be spuriously driven by changes in nationwide lending standards by *MyBank* or the entry of subprime lenders during the boom.

One remaining potential violation of the exclusion restriction, and causal interpretation, could occur if borrowers differed systematically in the timing of entry by region in a manner that was correlated with differential household sensitivity of labor income to local demand shocks among these borrowers. If for example, *MyBank*, happened to increase lending more to low credit quality or subprime borrowers in areas that subsequently experienced larger house price declines, that could potential confound causal interpretation of the observed relationship. While I find that my instrument for negative home equity has a valid first stage and predicts lower household income, I find it does not predict statistically or economically significant differences in reported income, credit scores, or interest rates at the time of mortgage origination. If these borrowers were really more sensitive to local demand shocks it seems likely that would show up in the form of lower income, higher credit scores, or high interest rates at the time of initial origination of the loan. Given the relatively strong power I have for most of these tests, it appears unlikely there was any substantive difference in observable characteristics of these borrowers that is correlated with the instrument. Therefore, it is unlikely there were substantive differences in "hard information" lenders used at origination across these regions or observable characteristics of these borrowers. I also find no differences in the probability of a mortgage

being "Alt-A" or using unverified income, and no difference in verified income at origination, suggesting no evidence of differential "soft-information" across these regions either.

While it seems unlikely, it is still possible that there exist some unobservable differences in these households that makes them more sensitive to local demand shocks. To address this concern I first include households fixed effects to flexibly control for any time-invariant differences in characteristics and take advantage of the panel nature of the data. I find that instrumented negative home equity is still associated with a decline in labor income. I then take advantage of the fact that most of the proposed theories for why reductions in home equity could reduce labor supply are based on frictions that occur non-linearly when households have negative home equity. I run a placebo test excluding all observations when a household actually has negative home equity and show that in reduced form changes in the instrument, that would normally increase the probability of negative home equity, are no longer associated with statistically significant changes in household income. In other words, once we exclude treatment, changes in local house prices, likely to be correlated with local demand shocks, have no differential effect on household income. This is also supported by non-linear forms of the analysis which show no relationship between instrumented home equity and income, even for large variation in instrumented home equity, except for circumstances when properties are likely to have negative equity. The placebo results show that these households are unlikely to differ even on unobservables that makes them more sensitive to local demand shocks, except for through the treatment of negative home equity. Supporting this causal interpretation, I also show my results hold after comparing only households who bought in the same MSA and year, but at different times of the year, just a few months apart. They also hold among the subset in my sample where I can observe student loans and control for the approximate date they enter the labor market.

One final concern I address is that households with *MyBank* mortgages and negative equity could be systematically hiding income from the institution they owe money. Since I measure only deposit inflows at *MyBank*, households who also have mortgages at *MyBank* could be closing accounts or reducing payroll inflows at that institution in order to appear less able to

pay and receive more assistance. To partially alleviate this concern throughout my analysis I use multiple restrictions to be sure households in the panel have active retail accounts, taking advantage of the inflow and level information I have for all retail accounts at *MyBank*. Results are robust to all choices of filter and measures of income. I also rerun the analysis for households with a *MyBank* retail and credit card account, but have a mortgage where *MyBank* does not own or service the mortgage. In this case the household has no incentive to hide deposits and I find that negative equity still reduces income. Overall these results are consistent with income shrouding playing little role in the observed decline in deposits, so that results represent actual declines in overall household deposits[7].

In addition to providing support for the economic importance of the aforementioned channels, another contribution of this paper is providing evidence of a new component of the relationship between negative equity and labor supply. I find that as much as 1/5th of the overall effect can be can be attributed to what I call the "household debt overhang" channel. As shown theoretically by Donaldson, Piacentino, and Thakor (2017), since households have limited liability, if housing collateral value falls below the outstanding mortgage balance, the recovery value for lenders in default can depend on the household's income. Mulligan (2008, 2009, and 2010) has shown that in practice lenders engaged in widespread income-contingent renegotiations for households with negative equity prior to default which created implicit taxes that disincentivized labor supply. Due to the detailed nature of the data I can observe actual modifications, which lets me examine the role these sort of incentives may play in practice. I find that the effects of negative home equity on labor supply are statistically significantly bigger for households who ever receive a mortgage modification. It is also the case that instrumented negative home equity is still is associated with a statistically significant reduction in labor supply, even though it is only about 4/5ths the size it is for the full sample. This suggests that it is likely many of the previous channels for non-linear frictions highlighted in the literature also

[7] In subsequent work, researchers unaffiliated with this paper (Gopalan et al. 2017), replicate the identification strategy developed in this paper in an entirely new dataset based on verified employer income records, rather than deposits at *MyBank*, and also find a negative relationship between instrumented negative home equity and labor supply. These results provide additional confidence that results are not driven by income hiding at *MyBank* and support the general external validity of the findings in this paper.

likely play a significant role in the observed relationship between negative home equity and labor supply. Since receiving modifications is not randomly assigned I also show that the response is amplified in regions where mortgages are modified at a higher rate, even controlling for delinquency and foreclosure rates in those regions. One reason for regional variation in mortgage modification rates is state-level differences in the costs of foreclosure driven by judicial foreclosure requirements. Taking advantage of this I compare MSAs just across state border laws with different judicial foreclosure requirements, which discontinuously alter mortgage modification rates, and find effects are concentrated in states with more modification rates, driven by judicial foreclosure laws. Despite the potential economic importance of such a mechanism in policy discussions, to the best of my knowledge, this is the first paper to establish empirical evidence of the role mortgage debt overhang play in household labor supply decisions.

2. Household debt overhang

For highly levered firms a reduction in firm wealth reduces the marginal incentives for investment in positive net present value projects because the benefits accrue disproportionally to existing debt holders (Myers 1977). Highly levered households face a similar problem when deciding to invest in the effort needed to earn labor income. If a portion of any marginal income earned by an indebted household is transferred to a lender via increased liability repayment, then this transfer to debt holders acts just like an implicit tax that incentivizes households to reduce their labor supply (Mulligan 2008, 2009, and 2010, Herkenhoff and Ohanian 2011, Donaldson et al. 2014). This doesn't mean households necessarily purposefully leave existing employment, but could suggest, as in Donaldson et al. (2014), that already unemployed workers could be incentivized to prolong job search in an effort to find a better match.

While in practice income-contingent repayment for foreclosed properties in deficiency judgments are rare (Ghent and Kudlyak 2011), income contingent mortgage modifications were ubiquitous following the crisis (Goodman et al. 2011) and likely to provide a major channel through which household debt overhang problems occur. In response to the substantial rise in

mortgage delinquencies during the crisis, lenders engaged in large scale mortgage modification programs to help distressed borrowers. In fact from January 2008-May 2011 51% of all non-performing or re-performing subprime mortgages received a mortgage modification (Goodman et al. 2011)[8]. While these modifications may have been optimal collection strategies by lenders they may have also provided perverse labor supply incentives. Mulligan (2009) has shown that in theory and in practice lenders are more likely to engage in loss-mitigation actions for delinquent borrowers if they demonstrate a reduced ability to pay their liabilities. These income-contingent loss mitigations result in implicit marginal tax rates with strong moral hazard incentives for households to reduce labor supply. In the case of the majority of public mortgage modification programs debt-to-income targets create implied marginal tax rates in excess of 100%[9] for households with negative equity, which as noted by Mulligan (2009) "is significant even from a macroeconomic perspective" and likely to "produce distortions that are large enough to be visible in the national employment data".

These income-contingent loss mitigations mean that for many households with negative equity the majority of benefits from additional time and effort invested in employment income accrue to the debt holders rather than the household. For example, if an average negative home equity household with $4,000/month in gross income and $1,500 in monthly mortgage payments was seeking a mortgage modification via the Home Affordable Modification Program (HAMP) and worked to earn an extra $500/month in income not only would all of the additional $500/month in income accrue to the lender, the household would actually end up losing at least $3,271 over the next 5 years despite the additional time/effort[10]. Just like in the classic corporate debt overhang problem faced by firms "the gain in the market value of debt acts like a tax on new investment [and] if that tax is high enough, managers may try to shrink the firm" (Myers

[8] For Prime, Alt A, and Option ARM, the modification rates were 23%, 31%, and 29% respectively.

[9] It is worth noting as that these are the implied marginal taxes for a one-time change in one year of labor income based on its effects over the next five years. While it is more standard to think of how a one year change in income alters only the next one year's take home salary, in this instance it severely understates the loss of benefits that accrues over multiple years. Therefore, the actual implicit marginal tax isn't that straight forward since it requires discounting the value of future lost benefits. Under a wide range of reasonable assumptions though Mulligan (2009) has shown these implied rates are typically in excess of 100%.

[10] Calculation by author based on checkmynpv.com.

2001), where in the case of this household debt overhang problem the borrower reduces the "firm" by reducing their labor supply. This could mean that a fall in housing wealth, which via a wealth effect would normally suggest a rise (weakly) in household labor supply, could actually cause a reduction in labor supply via a substitution effect coming from the implicit marginal tax of the income-contingent loss mitigation by the lender.

Melzer (2015) has also shown that households with negative home equity reduce investments in their house, since they anticipate no longer being residual claimants. Mayer et al. (2014) found that households were aware of the announcement of a large scale mortgage modification program by Countrywide and responded by falling delinquent, despite the ability to pay. Taken together these results suggest that a significant number of households are aware of their home equity and loss mitigation programs, and are willing to respond strategically via their home investment and mortgage payment decisions[11]. This paper contributes to this literature by showing that households also reduce their labor supply in response to the incentives provided by negative home equity and mortgage assistance programs. This paper also contributes more broadly to our understanding of the effect of contract modifications including large scale loan modifications programs (Agarwal et al. 2010; Calomiris et al. 2011; Agarwal et al. 2012; Chang and Weizheng 2013; Collins and Urban 2015; Dobbie and Song 2015a; Dobbie and Song 2015b) and household responses to explicit and implicit taxes (Chetty 2008; Jacob and Ludwig 2012; Chetty, Friedman, and Saez 2013; Card, Johnston, Leung, Mas, and Pei 2016; Blundell, Costa Dias, Meghir, and Shaw 2016;, Blundell, Pistaferri, and Saporta-Eksten 2016).

3. Data description and validation

The majority of my data comes from a major U.S. financial institution but I also merge in zip-code level income data from the Internal Revenue Service (IRS) to validate my income measures

[11] Even though the authors are unable to investigate the effects on income of the announcement of the countrywide program it is worth noting that settlement had debt-to-income targets of 34% for at least 5 years based on the previous 1 year of income, which like HAMP imply marginal tax rates in excess of 100%. A household willing to stop paying their mortgage and forgo an employment opportunity would be eligible for more than 100% of the forgone income in reduced monthly payments once they received a modification.

and state-level judicial foreclosure law information. The data provider for this project is a major U.S. financial institution, who I refer to as *MyBank*, with transaction-level client account information on more than 1/4th of all U.S. households over the 5 years from 2010-2014[12]. For the purposes of this project I focus on households with sufficient *MyBank* relationships to estimate income and mortgage information and analyze income decisions at a monthly household level. Income is estimated using retail account deposit information and mortgage information is either derived from credit bureau data (only available for households w/ *MyBank* credit card accounts) or *MyBank* mortgage account information. In appendix A I detail how combining household information from multiple *MyBank* accounts alters the sample size.

For each mortgage account I have detailed information on the mortgage type (ex. fixed rate 30 year), characteristics at origination including the date, reported income, credit score, interest rate, appraised loan-to-value, and ongoing monthly mortgage performance, characteristics, and actions, including delinquency status, current loan-to-value updated using internal LPS MSA-level HPI data, any loss mitigation actions taken, such as mortgage modifications, and current interest rates. Perhaps not surprisingly given the substantial coverage of this data provider, in Figure B2 in the appendix I show that the time series of delinquency rates for *MyBank* mortgage data matches closely with the levels and trends seen in national Federal Reserve economic mortgage data over the past 5 years.

By a substantial margin the largest population of households with a *MyBank* relationship are credit card customers. This should be expected since households very often only have one mortgage lender, but will have multiple credit cards. For each credit card account and month *MyBank* pulls credit bureau data on the associated customer liabilities. For the purposes of this paper this monthly frequency credit bureau data is the only information used from the credit card accounts. The credit bureau data includes comprehensive data on all customer liabilities across all lenders including mortgages, auto-loans, student loans, home equity lines of credit, credit

[12] According to census.gov from 2009-2013 there were about 116 million U.S. households and *MyBank* has client accounts covering more than 31 million households (see Table A1 for details), which would be about 27% of all U.S. households. The coverage is lower when looking at individuals, which is likely because dependents are unlikely to have separate *MyBank* accounts (ex. children) and some households with multiple adults still may choose to list only one person in the account information.

cards, and installment credit as well as monthly updated credit scores. For each credit category the dataset includes information on the balance, monthly payments, and initial balance[13].

Retail accounts include any checking or savings accounts. The raw data includes every single transaction into these accounts (inflows and outflows) but to protect privacy include only the day a transaction occurred, the amount of the transaction, and very general transaction category types (ex. "ACH direct deposit"). The dataset includes billions of transactions over the period 2010-2014, but since my goal is to measure income I focus on the subset of transactions labeled as deposits, which include direct deposits, physical deposits including at the teller and ATM, and other deposit types including mobile RDC deposits. Since some of these accounts are not being used to deposit the majority of income I restrict my analysis to households with active accounts[14] that appear to contain the majority of their income[15].

To explore the validity of using deposits as an income measure I confirm the validity of my income measure by comparing the average annual income based on my deposit data at a zip code-level with those reported by the IRS Statistics of Income (SOI)[16] over the period 2010-2013. In Figure B1 you can see a very strong correlation between these measures of income. Regardless of the type of income measure used and the subsample explored I find that zip code level correlations between my measure and the IRS SOI are very high and range from 0.736 all the way up to 0.911. The fact that the relationship is so strong between these two measures and neither appears to be systematically higher suggests that for the subset of households analyzed deposits represent an effective measure of household income. I also extract households receiving

[13] Maturities and interest rates on these liabilities are estimated and validated for the subset of data where both are available. In particular, given the panel nature of the data I am able to observe total monthly payments in addition to changes in the outstanding balance for each account month over month. Assuming a fixed interest rate, maturity, and standard amortization schedule I numerically estimate what would be the implied interest rate and maturity from a selection of discrete interest rates and maturities that exist in the data for each set of back-to-back months. If less than 75% of estimated interest rates and maturities for given product do not match or I have less than 20 observed estimates, I do not include them in the sample. Even with sufficient information these could have floating rates, non-standard amortization schedules, or unusual pre-payment behavior which would confound clean identification of the underlying maturities and rates. More information on the internal quality of the method are available upon request.

[14] A household is defined to have "active" accounts if across all accounts in a given month they deposit at least $100 or have $200 in financial assets.

[15] To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$25k, a mean and median level of deposits across all accounts >=$500 & <=$25k.

[16] For the purposes of income validation, I utilize publicly available zip-code level income data from the IRS (Internal Revenue Service) Statistics of Income for 2010-2013. This data is based on administrative records of individual income tax returns (Forms 1040) from the IRS Individual Master File (IMF) system. More details about IRS SOI income data are available online at www.irs.gov.

social security or disability checks. After excluding regularly schedule job-related deposits, I assign any remaining direct deposits that are paid on either the 3rd of each month, or the 2nd, 3rd or 4th Wednesday of each month as social security-related. According to the Social Security Administration the mean monthly benefit for a beneficiary is $1,223/month which matches closely with the mean of $1,268/month I find per social security recipient in my sample. This validates not only the data overall, but also this method of extracting social security payments.

For the majority of my analysis I focus on households with retail deposits that let me measure income, and mortgages at *MyBank* that let me see their level of home equity which include about 200k households in the final sample representing approximately 7.8 million household-month observations. For most of my analysis I focus on households with income at origination, loan origination date, and additional information which restricts that to approximately 5.4 million household-month observations. I also consider households with *MyBank* retail and credit card accounts and mortgages with any lender as robustness check, which increases the sample to about 20.1 million household-month observations. For more details on the data merging see Table A1 in the appendix.

I analyze a broad range of characteristics for each sub-sample of *MyBank* in Table 1 and in more detail in Table B1 in the appendix. From the tables we can see that the median household income for households with mortgages is about $5-6k/month and as expected the majority of household liabilities are mortgage related. The median level of income, non-housing financial assets, mortgage leverage, and mortgage interest rates are similar to self-reported information collected by the Survey of Consumer Finance (SCF) for households with at least $1,000 in active mortgage balance in 2010 consistent with the representative nature of the *MyBank* national coverage and lends credibility to the external validity of the conclusions of this paper. For more details on this comparison see Table B2 in the appendix.

The *MyBank* mortgage data includes information on reported income at origination which provides a nice opportunity to test the validity of the cross-lines of business data matches as well as providing another check of the quality of my deposit-based income measure. In Figure B1 I plot the cumulative distribution function of income at origination and income based on deposits

for a match sample of individual households who originated a mortgage in the same year when sufficient deposit information is available to estimate income. These distributions appear remarkably similar and the individual income correlations range from 0.378 to 0.449 depending on the measure of deposit income used, all of which lend substantial credibility to the internal matches across *MyBank* lines of business as well as validating my income measure across the income distribution.

As noted by Mian et al. (2015), states that don't require judicial procedures for mortgage lenders to foreclose on delinquent borrowers are twice as likely to foreclose. The increased ease and likelihood of foreclosure reduces the likelihood that non-performing mortgages will receive a modification. For example, the documentation for the net present value tests for mortgage modifications under HAMP includes "state-level foreclosure timelines" and "state-level average foreclosure costs" as major determinants of whether or not a mortgage modification should be undertaken. To explore this source of variation I merge in state-level judicial foreclosure requirements based on RealtyTrac's website, just as was carried out in Mian et al. (2015).

4. Empirical method

To understand the effect of negative household equity on labor supply I run an instrumental variables regression using variation in the likelihood of negative equity based on the timing and location of home purchase relative to households living in the same region at the same time. To build intuition for the instrumental variables approach though I start by running the following regression

$$y_{icrt} = \alpha + \gamma_{rt} + \phi_{ct} + \sum_k \delta_{1k} \cdot 1_{\{l_k \leq LTV_{it} \leq \mathrm{h_k}\}} + \epsilon_{icrt} \tag{1}$$

where for household *i* in month *t* in region *r* that originated their mortgage in month *c*, this regresses household income, y_{icrt}, on a dummy variables which equals 1 only if the households loan-to-value ratio, LTV_{it} is greater than l_k and less than $\mathrm{h_k}$ for *k* loan-to-value buckets, region x time fixed effects, γ_{rt}, and cohort (month of mortgage origination) x time fixed effects, ϕ_{ct}. The problem with a naïve regression of income on home equity is that reverse causality or omitted

variables are not only possible, but are likely to prevent confidence in any causal interpretation of the effect of negative equity on labor supply. For example, time varying local demand shocks and initial credit quality could affect both income and home equity, and households with higher income likely invest more in home maintenance. Since I compute changes in house prices at MSA level, the inclusion of MSA x time fixed effects precludes the possibility that results are driven by variation in local demand shocks or individual variation in home investment. The cohort x time fixed effects means analysis is not confounded by changes over time in the nationwide composition of borrowers at *MyBank* or entry of subprime borrowers during the boom. I also include multiple loan-to-value indicator buckets to see if, as would be predicted by many of the theories of labor market frictions from housing wealth, declines in income occur only for high loan-to-value ratios.

Despite the inclusion of all these controls time-varying household level variation in LTV still has the potential to confound causal interpretation. In equation 2 I make this more transparent by decomposing the current household's LTV into three distinct components; (1) house prices changes, (2) changes in the balance of the mortgage, and (3) origination LTV.

$$LTV_{it} \equiv \frac{1}{\%\Delta HP_{rct}} \times \%\Delta Loan_{it} \times LTV_{ic} \tag{2}$$

Since households with improved income are more likely to prepay their mortgage, reducing the LTV, prepayment poses an empirical challenge for identification. To circumvent this rather than using actual changes in loan amount, I compute what the loan reduction would be if the mortgage was a 30-year (360 months = *T*) fixed rate loan paying the median national monthly mortgage rate, *r* (I use 6.75% based on my sample statistics).

$$\%\Delta \text{Synth}Loan_{ct} \equiv -\frac{(1+r)^{t-c}-1}{(1+r)^{T}-1} \tag{3}$$

The resulting formula in equation (3) varies across mortgages based on the age of the loan, but no longer depends on any other source of household-specific variation. An additional concern is that origination LTV could be a function of household specific characteristics, such as income or credit quality. Since I include household-level fixed effects in specification (1), time-invariant

factors, like LTV at origination, are only a concern when interacted with a time-varying factor, as is the case here. In particular, if high LTV at origination individuals are more sensitive to local demand shocks then this could be driving any simultaneous movement in income and household equity, rather than labor supply. To alleviate this concern I use the median national LTV at origination for each cohort for all households. Combining these I get a "synthetic" LTV, or *SLTV*, which only varies at the cohort-region-time level, and, controlling for all previously mentioned fixed effects, provides a plausible instrument for the probability of a household having negative home equity:

$$SLTV_{rct} \equiv LTV_c \times \frac{1}{\%\Delta HPI_{rct}} \times \%\Delta \text{Synth}Loan_{ct} \tag{4}$$

Variation in SLTV, after including all controls in equation (1), will be driven almost entirely by how fortunate the timing of house purchase was for a household within a particular region relative to their neighbors. Households that bought homes prior to relative local house price declines will tend to have higher SLTVs relative to those who bought immediately afterward.

To formalize the instrumental variable approach define I run the following 2SLS regression

$$\begin{aligned} U_{it} &= \alpha + \gamma_{rt} + \phi_{ct} + \delta_1 \cdot 1_{\{SLTV_{rct} \geq 100\}} + X_{it}'\beta + \eta_{icrt} \\ y_{icrt} &= \alpha + \gamma_{rt} + \phi_{ct} + \delta_2 \cdot \widehat{U}_{it} + X_{it}'\beta + \epsilon_{icrt} \end{aligned} \tag{6}$$

where I defined a household who has negative home equity (aka underwater) as $U_{it} \equiv 1_{\{LTV_{it} \geq 100\}}$.[17] The necessary assumption for the exclusion restriction is that after controlling for all fixed effects the synthetic LTV only affects income via the probability the house has negative home equity. In my analysis I find support for exactly this, since after including all fixed effects negative SLTV is not correlated with observable, or even difficult to observe, measures of local demand sensitivity, but still relates to the probability of negative home equity and subsequent labor income. These results are also robust to a wide range of variations of specification 6

[17] I run this using the 1st stage as a linear probability model using negative SLTV as the instrumental variable. For robustness I also show results using multiple loan-to-value bucket indicators in the 1st stage, but not probit or linear-linear models. As noted by many papers (ex. Greene 2004) probit estimates are inconsistent in a fixed effect panel regression as are purely linear models when the underlying treatment effect varies non-linearly.

including the primary specification which controls flexibly for any time-invariant characteristics via household fixed effects as well as comparing just those households who bought their homes within the same MSA and year, separated by only a few months.

5. Results

5.1. Validity of the Instrument

Since negative home equity and household labor supply are likely to be jointly determined, to assess the causal effect of negative home equity on labor supply I employ the instrument outlined in the two stage least squares regression of specification 6. In particular, I use a dummy variable equal to one if the SLTV is greater than 1, after controlling for region-time and origination date-time fixed effects as an instrument. As detailed in section 4, SLTV is a measure of home equity based on the timing of home purchase in a given MSA, that doesn't depend on household-specific behaviors or characteristics. In Table II column 1 I show that this instrument meets the relevance criterion for a valid instrument. After including MSA x time and origination date x time fixed effects if the SLTV is greater than 1 a mortgage is a statistically significant 62.3 percentage points more likely to actually have negative home equity. In fact, throughout the analysis the first stage f-statistics are always very strong, because SLTV is mechanically related to LTV. The timing of home purchase is almost certainly going to be a strong predictor of current LTV, even with a broad set of fixed effects. Since SLTV meets the relevance criterion of being a valid instrument, if there is a relationship between negative home equity and labor supply, we would expect to observe it in reduced form between negative SLTV and household income. I find exactly this relationship in Table II columns 2 and 3. Negative SLTV is associated with a statistically significant reduction in the percent change in income per month, relative to income reported at origination, as well as raw observed household income/month. These are already suggestive of a potential causal link between negative home equity and labor supply. The remaining concern for causal interpretation would be a violation of the exclusion restriction.

Since the timing of home purchase, even from a single lender, within a region, relative to that same timing in other regions is not fundamentally randomly assigned, it is reasonable to be concerned that this region-specific timing could violate the exclusion restriction. If *MyBank* engaged in regional variation in the timing of different lending policies, that predicted future changes in house prices, and differences in local demand sensitivity of these borrowers, that would confound causal interpretation of these findings. Since the specifications in Table II don't include household fixed effects, I can test explicitly for any evidence of observable differences in these borrowers at the time of origination correlated with the instrument. In Table II columns 4-6 I show that negative STLV doesn't predict statistically or economically significant differences in reported income, credit scores, or interest rates at the time of mortgage origination, despite the fact that these households' current income is lower in columns 2 and 3. In other words, there is no evidence that *MyBank* was lending to borrowers with income more sensitive, ex-ante, to house price movements. The borrower reported incomes at origination weren't lower, the credit agency determination of credit quality or sensitivity to future economic shocks wasn't higher, and even a proxy for bank's internal measure of risk, the mortgage interest rate charged to the borrower, didn't differ for these households. One limitation of looking at reported income at origination is that the borrowers might be misreporting these values systematically. To alleviate that concern I show in Table III. Columns 1-3 that these borrowers aren't any more likely to originate a mortgage that is Alt-A ("liar loans") or without income documentation, and focusing on only verified income still reveals no income difference at the time of origination.

Despite finding no evidence of differences on important observable characteristics at origination, it may not be probable, but it is still possible that the borrower's differed on some unobservable qualities that makes them more sensitive to local demand shocks. To deal with even this concern I first show in Table III column 4 that negative SLTV is still associated with a decline in income after including household fixed effects to control flexibly for any time-invariant component in any unobservable differences. This still doesn't deal with any differences in unobserved household sensitivity to time-varying shocks. To address that concern I use the fact that the likely theories explaining a positive relationship between housing wealth and labor

supply, predict effects concentrated among households with negative home equity. In Table III column 4 I show that omitting household-month observations with negative home equity there is no relationship between negative SLTV and household income. This placebo test shows that once the source of treatment, actual negative home equity, is omitted there is no relationship between house price changes and household income. In other words, these households are not generally more sensitive to local demand shocks, but only when it also happens to cause them to have negative home equity. The non-linearity of this sensitivity is also confirmed in Figure 1, which shows the relationship between non-linear categorical dummies for LTV and the percent change in household income since origination, after controlling for MSA x time, origination date x time, and household fixed effects. As can be seen in the figure for low levels of LTV, but large variation in relative terms, there is no relationship between LTV and household income. Only when households approach negative home equity is there a decline that occurs non-linearly in household income[18], and remains low for households with negative equity. Figure II repeats this analysis with the same fixed effects, but looks at interactions with 10% buckets of house price movements since origination. Just like with SLTV this eliminates concerns that the overall effects could be driven by household choices at origination or during the life of the mortgage. Again for cases where households are unlikely to have negative home equity, but large relative variation in housing wealth, there is no change in household income. Only when housing wealth declines are likely to lead to increased probability of negative home equity is there a non-linear and persistent decline in household income. Taken together these provide compelling evidence that negative SLTV leads to declines in household income, only through the increased probability of negative home equity, and a causal link from negative home equity to reduced household labor supply.

5.2. Negative Home Equity and Household Labor Supply

[18] It is worth noting that since actual home value is estimated from MSA-level indices it is possible that some households with LTVs of 90% to 100%, or even 80% to 90%, would actually have negative home equity if they tried to sell their home. Given that we would expect some decline in income even for households measured as having some small amount of positive equity.

In Section 5.1 I provide evidence that strongly supports the validity of negative SLTV as an instrument for negative home equity. Using that instrument in Table IV column 1 and 2 I show that instrumented negative home equity is associated with a $298/month or 3.47% decline in household labor supply. While the previous section provided substantial support for causal interpretation of this estimate, it focused primarily on the lack of any evidence of observable differences in the borrowers correlated with negative SLTV. By contrast in the remainder of Table IV I further the confidence in the causal interpretation by showing that baseline findings are robust to a wide variety of additional controls. In column 3 I address concerns that the timing of home purchase and location even within a given MSA may have been correlated in a way that exposed these households to larger local demand shocks for the same MSA-level shock. In this specification I include zip code x time fixed effects, instead of MSA x time fixed effects, and again I find similar declines in labor supply, suggesting selection within MSA is not driving the observed results. In column 4, I also include a large range of non-parametric household-specific time varying controls that might be expected to be correlated with labor demand sensitivity. These include deciles for origination income and property value, mortgage original interest rate by percentage buckets, and original credit score in bins of 50 all interacted with time fixed effects. These results show a 4.9% decline in household income, again consistent with overall findings and suggest results are not driven by any non-linear selection at origination.

Unfortunately, in this data I don't see when workers enter the labor force and it is possible that those who buy earlier or later in specific markets also entered the labor market in a systematically different ways and may be more likely to be laid off. To partially mitigate that concern in column 5 I show that instrumented negative home equity is associated with a 2.2% decline in labor supply, even controlling for the origination year interacted with the MSA and time. That means my identification is coming from comparing households who bought in the same MSA and year, but at different times of the year, just a few months apart. Given the proximity of these purchases it is very unlikely that there are large systematic differences between these buyers. That being said it is still possible, though significantly less likely, that even within a given year later buyers also entered the labor market later, making them more

exposed to economic downturns. While I don't see the year people enter the labor force, for the subset with student debt I can estimate the time since a household attended college, as proxied by the average origination date of all student loans[19]. In column 6 I use this same sample of households with information on approximate college graduation date, but now include fixed effects for MSA x time x college graduation year. This allows me to control flexibly for the duration of time the household head has been in the local labor market, which is likely to be correlated with age and more likely to be related to job duration. Even with this more stringent level of controls I find a valid first stage and a statistically significant decline of 5.6% in household labor supply in response to instrumented negative home equity.

One additional potential concern with all the analysis up to this point could be that I measure deposits at only one institution and in particular I use deposits from the same institution that is the household's mortgage lender. If household hides or shifts deposits away from their lender when they have negative equity this could mean that the reduction in deposits seen for households with negative equity is actually just movement of deposits to another institution rather than an actual decline in overall deposits from income. With this concern in mind throughout my analysis I use multiple restrictions to be sure households in the panel have active retail accounts, taking advantage of the inflow and level information I have for all deposit accounts at *MyBank* and results are robust to all choices of filters and measures of income. In particular, in appendix Table BIII column 2 I show that results are robust to including only direct deposits instead of all deposits as the measure of income[20]. To address the concern more directly I show in Table V column 1 that results are unchanged excluding cases where households deposit $0 into their accounts. This suggests results are not driven by households systematically leaving the bank. This is despite the fact that Table V column 2 shows results are driven entirely by declines of greater than 25% in household income. This suggests households make large

[19] For a small subsample of households with credit cards I have information on when they graduated college. This sample is too small to use as an instrument, but has provided credibility that as would be expected, average origination date of student loans is highly correlated with the timing of college graduation. Validation results are available upon request.

[20] Columns 1, 3 and 5 also show that results are not altered by normalizing percent changes in deposits by the mean of the household over the whole sample, focusing on just the log of deposits, or using *MyBank*'s internal measure of a household.

extensive margin changes in labor income, say by increasing search duration. It also makes it unlikely results are driven by household systematically moving deposits into retirement savings accounts prior to depositing. Shifts in this kind of long-run savings behavior would cause small pervasive changes in deposit inflows rather than large concentrated reductions of the magnitude observed. This is also supported by the results in Table V column 3. Despite the overall reduction in deposit income shown previously, in column 3 I show that households are actually more likely to receive social security or disability checks. This suggests again that these households are either more likely to retire or move onto disability and in doing so reduce their labor supply by reduced labor force participation. The fact that we observe an increase in social security or disability checks again suggests there isn't a systematic shift of deposits away from *MyBank* in response to the instrumented negative home equity.

To be even more careful though, I rerun my analysis focusing on *MyBank* retail customers with a mortgage from another lender. Since I no longer have detailed mortgage information I use the zip code households enter in their retail accounts[21] as a proxy for the MSA the property is located in and information from the credit bureau data on mortgage origination dates. I then regress the $ amount of deposits per month on the percent of mortgages in that estimated zip code x origination year x time with negative home equity, after including MSA x time, origination date x time, and household fixed effects. Note that in this case these are reduced form regressions since current LTV is not available in credit bureau data to run the 1st stage. This method is likely to reduce the power of the regression, but the reduced form regression shown in column 4 still finds that a higher probability of negative home equity due to the timing of home purchase is associated with lower current deposits, after including all region x time, cohort x time, and household fixed effects. The result holds in column 5 when analyzing households with mortgages at any lender or for the subset of households where *MyBank* is not a servicer or owner of the mortgage. Since in these cases *MyBank* is not the lender there is no reason for the borrower to systematically shift deposits away from the institution. Overall these results suggest

[21] For households with multiple zip code I use the zip code of the largest account and the date closest to the origination of the most recently originated mortgage.

that hiding income is unlikely to explain the reduction in monthly deposit inflows seen for households with negative equity.

Overall these results are consistent with negative home equity causing an average labor income decline of 2.2%-5.6%. While a complete assessment of the macroeconomic implications of this labor supply response is beyond the scope this paper, some simple back-of-envelope calculations suggest channel may have had important implications for the sluggish nature of the labor market recovery following the crisis. If the average unemployed household earns half of their employed level of income and all changes in labor supply occur via the extensive margin, then a 2.2%-5.6% reduction in labor income is consistent with a 4.4%-11.2% rise in unemployment among negative equity households. CoreLogic estimates that approximately 15 million households had negative equity following the crisis. Combining these estimates and assuming a linear aggregation of the partial equilibrium results would suggest a 0.66-1.68 million shock in job-equivalent labor supply because of household debt overhang. From the peak of 2008 to the trough in 2010 non-farm payrolls fell by about 8.6 million jobs, so the estimated aggregate partial equilibrium shock from household debt overhang would be 8%-20% the size of the total general equilibrium employment decline following the crisis. Since these are just the linear aggregations of partial equilibrium estimates these give us some idea that the magnitudes are unlikely to be large enough to explain most of the observed labor participation decline, unless there are amplification mechanisms, but have the potential to exacerbate existing employment declines, especially in regions with high concentrations of negative home equity[22]. While it still seems likely that coincident factors and demand shocks explain most of the observed persistently low labor force participation following the Great Recession, the evidence presented in this paper suggests household labor supply may have also had role to play in more full understanding the slow-moving labor market recovery following the crisis.

[22] The actual total amount of reduced labor participation following the crisis that can be explained by household debt overhang will depend critically on labor demand and in particular the stickiness of wages. This exercise is meant to provide some benchmark for the potential aggregate partial equilibrium shocks generated by this channel, but are by no means intended as an estimate of the actual general macroeconomic equilibrium effects. As is noted by Chetty, Friedman, Olsen, and Pistaferri (2011), among others, macroeconomic estimated labor supply elasticities tend to exceed microeconomic estimates and typically cannot be easily recovered without the benefit of an underlying structural model.

5.3. Household Debt Overhang and Labor Supply

The finding of a causal negative relationship between negative home equity and labor supply supports a wide range of potential channels, mentioned in more detail previously, that predict such a relationship including collateral constraints for entrepreneurship and job search, effort and risk taking incentives for innovators, job search constraints due to housing lock, and the effects of bankruptcy. While a decomposition of the overall findings into all these possible channels is beyond the scope of this paper, I am able to provide empirical evidence for a new channel not previously in the literature, household debt overhang. As noted in more detail in Section 2, household debt overhang occurs if negative equity encourages income-contingent renegotiations, which act like implicit taxes with a moral hazard problem that disincentivizes labor supply. As is shown in Figure 3, negative equity does appear to dramatically raise the probability of default and renegotiations via mortgage modifications, the vast majority of which are means-tested. If mortgage modifications are an important contributor to the overall relationship, we should expect to see that income responses to instrumented negative home equity are larger among those households that ever receive one. In Table VI column 1 I show exactly this. Interacting negative SLTV with a dummy variable equal to one if a household ever receives a mortgage modification in the first stage shows that households who receive a mortgage modification have a statistically significant 5.7% larger decline labor supply in response to instrumented negative home equity. The fact that even among those who never receive a modification instrumented negative home equity still has a statistically significant 2.72% decline suggests that channels besides debt overhang still likely explain a large portion of the overall relationship. Relative to the initial estimate of a 3.47% decline under the same specification without any interactions suggests that as much as 1/5th of the overall effect can be attributed to the subset of borrowers who receive mortgage modifications.

While this is suggestive of evidence for the debt overhang channel playing an important role it is possible that the observed effects among this sub-group being higher could be at least

partially driven by heterogeneous effects among these borrowers unrelated to actually receiving the mortgage modification. To address this concern in Table VI columns 2 I show that the effect of instrumented negative equity on labor supply is larger in regions with higher mortgage modifications rates. Typically, it would be challenging to distinguish the effects of some of the other channels, like say housing lock from debt overhang, since both are driven by negative equity and can alter job search. In this case though, if the variation in treatment effects are really driven by the causal effect of modification rates it would be compelling evidence that housing debt overhang plays an important role beyond housing lock. Debt overhang would be stronger if modification rates are higher, while these mortgage renegotiations, such as short sales or increased home equity, would actually reduce the effect of housing lock. One concern with this approach is that mortgage modification rates are not randomly assigned and may be in areas with larger house price declines and/or more delinquencies. To alleviate those concerns I show that the effects of instrumented negative home equity on labor supply are larger in areas with more mortgage modifications even controlling for the percent of mortgages with negative equity (column 3) and the delinquency rate (column 5 weakly) in those regions. In fact, in columns 4 and 5 I show there is no evidence that excess delinquency rates in a given MSA have any relationship with responses to instrumented negative home equity, suggesting the observed heterogeneity is not simply driven by cross-sectional differences in delinquency rates. Chetty et al. (2013) provide evidence of strong geographic variation in awareness of EITC schedules that leads to increased labor responses/manipulation. It also seems likely that areas with more knowledge of modification programs would also be more likely to see responses, regardless of the local rates of actual eligibility.

As one final robustness check I use the fact that time to delinquency, largely driven by variation in judicial foreclosure requirements is one of the most important components in the decision to modify a mortgage (hmpadmin.com). Mian et al. (2015) and Ghent (2012) convincingly argue that state foreclosure laws differ based on historical path dependent exogenous events and provide evidence of co-variate balance across these borders for a broad range of characteristics for states with and without judicial foreclosure requirements prior to the

Great Recession. In figure IV panel A I show that among mortgages at least 60+ days delinquent within 100 miles of a state border there is a statistically significant decrease in the probability of receiving a mortgage modification in non-judicial foreclosure states that occurs discontinuously at the border. By contrast, no such decline is shown for recourse vs. non-recourse states in panel B. The increased rate of mortgage modifications is also confirmed in appendix Table BIV comparing mortgages within 50 miles of the border and increases as the level of delinquency for included mortgages rises, as expected. In Table VI column 6 I interact instrumented negative home equity with being in a judicial foreclosure state among households within 50 miles of a state border with differing laws. While I have very limited power I still find that the effect of negative equity on household labor supply is statistically significantly larger for households in judicial foreclosure states, where modification rates are higher[23].

Overall these results are consistent with debt overhang being responsible for as much as 1/5th of the overall observed 2.2%-5.6% decline in household labor supply caused by negative home equity. With some additional assumptions I can estimate the labor supply elasticity with respect to the implicit tax rate of mortgage modifications. In my mortgage data households with negative equity are 21 percentage points more likely to receive mortgage modifications than those without negative equity. From Mulligan (2009) we know that national mortgage modification programs create a substantial implicit tax, but lost income occurs immediately while lost benefits occur over the following 5 years. We know that total benefits over those 5 years are 1.2-1.5 times larger than the loss in income, so an implicit present value tax rate of 100% is consistent with reasonable discount rate benchmarks. Combining these we can say that the average household with negative equity faces an expected implicit marginal tax rate of 21% and since they reduce their labor supply by 0.48-1.21% attributable to household debt overhang this implies an elasticity of 0.02-0.06. These estimates are substantially lower than estimates of the Frisch or Hicksian extensive margin elasticity of labor supply in the microeconomic

[23] In this setting I am using a border discontinuity combined with an instrumental variables approach so while evidence of a statistically significant difference between responses and judicial and non-judicial states is compelling the absence of a negative response non-judicial states is unlikely to be of much significance (and is not statistically significantly different from zero) and likely to be driven by the substantially reduced power in this specification.

literature, which tend to be near 0.25 (Chetty 2012). Since only about ¼ of households likely to be eligible for a mortgage renegotiation actually received one (Goodman et al. 2011), if only around ½ of those households that received a modification were aware that the assistance is strongly means-tested that would suggest an elasticity of 0.16-0.48 conditional on being aware of the modification programs and the implicit tax. Those would be in-line with Frisch one-time tax elasticities from the microeconomics literature of 0.27-0.53 used by the CBO (Reichling and Whalen 2012). While obviously these are very rough estimates, they still suggest the observed responses could be consistent with elasticities estimated in previous microeconomic literature. It also suggests that in regions that are highly knowledgeable about the means-tested nature of the program the inclusion of such high implicit marginal taxes are important to consider when implementing targeted assistance programs.

6. Conclusions

In this paper, I provide the first empirical evidence of the causal effect of negative home equity on household labor supply. I use a new comprehensive dataset with information on household-level liabilities, assets, and all deposit transactions for all customers of a major U.S. financial institution from 2010-2014 and variation in home equity based on the timing of home purchases among households in the same region at the same time from the same lender, controlling for any aggregate origination cohort trends. I find that instrumented negative home equity causes an average reduction of 2.2%-5.6% in household labor income. I then explore the channels driving this result and find evidence for a new link, household debt overhang, between negative home equity and labor supply. Household debt overhang arises when income-contingent means-tested mortgage modifications act like implicit taxes, resulting in a moral hazard problem that causes labor supply disincentives. Consistent with households responding to an implicit tax I find that responses are larger for households that receive a modification and in MSAs with higher than expected mortgage modifications, not driven by local economic conditions.

These results shed new light on the role house price declines played in exacerbating employment declines following the crisis. Mian and Sufi (2012) have examined how house price shocks affect equilibrium employment via local labor demand, but this is the first paper to demonstrate the role house price declines played in labor markets via the supply channel. While identifying the aggregate general equilibrium response to home equity is beyond the scope of this paper, my results do suggest that it has a role to play in understanding how household balance sheets can exacerbate financial crises. While the overall relationship between negative home equity and labor supply is important in analyzing the sluggish recovery following the crisis, evidence of a role played by income-contingent renegotiation has additional important policy implications. In particular, evidence that household debt overhang effects household labor supply decisions, suggests that assistance programs should be designed taking into account the trade-off between more targeted assistance via income-contingency with the potential labor supply consequences.

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Table I. Summary Statistics

This table includes simple summary statistics for *MyBank* data. To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$50k and a mean and median level of deposits across all accounts >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel. All data winsorized at 99th percentile. This sample includes only households that have retail and mortgage accounts at *MyBank* from 2010-2014.

	Mean	Median	Std. Dev	# Obs (mil)	# HHs (mil)
A. Households w/ *MyBank* Retail & *MyBank* Mortgage 2010-2014					
Retail Data					
Income (All)	$7,663	$5,315	$8,439	7.835	0.200
Income (Dir. Dep.)	$4,142	$2,826	$4,742	7.835	0.200
Income (Dir. Dep. w/ Filter)	$6,470	$5,172	$5,226	2.291	0.058
Savings	$35,370	$10,100	$60,626	7.835	0.200
Card/Credit Bureau Data (w/ *MyBank* Credit Card Account)					
All Liabilities	$266,300	$225,000	$210,610	5.158	0.144
Has Auto loan	30%			5.158	0.144
Bal Used/Available All Credit	20%	10%	29.3%	5.158	0.144
FICO Bank Credit Score	767	782	74.4	5.158	0.144
Mortgage Data					
Primary MTG Balance	$199,900	$170,700	$137,130	7.835	0.200
MTG Interest Rate @ Origination	5.373	5.375	1.227	7.835	0.200
MTG Age (Months)	64	58	49	7.835	0.200
Income @ Origination	$7,494	$6,237	$5,171	5.419	0.147
Origination Loan-to-Value (%)	64	68	22.1	7.835	0.200
Current Loan-to-Value (%)	58	58	31.5	7.835	0.200
Is Owner Occupied	92.0%			7.835	0.200
Is Fixed Rate	83.9%			7.835	0.200

Table II. Validity of SLTV Instrument and Observables

This table provides evidence that after controlling for region-time and origination date-time fixed effects a household's synthetic loan-to-value ratio (SLTV) greater than 100% is a valid instrument to look at the effect of negative home equity on household labor supply, based on observable characteristics. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing-location of moving, and varies at the region-time-cohort level. Column 1 regresses a dummy equal to 1 if a household's current loan to value is greater than 100% on a dummy which equals 1 if the household's SLTV is greater than 100%, after including MSA x time, and origination date x time fixed effects. This is the 1st stage estimate of an IV regression. Column 2 is the same as 1, but the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the household's income at the time of mortgage origination, is the dependent variable. Column 3 is the same as column 1, but includes raw monthly deposit inflows as the dependent variable, without any normalization. Column 4 is the same as column 1, but monthly gross reported income at origination is the dependent variable. Column 5 is the same as column 1, but credit score at origination is the dependent variable. Column 6 is the same as column 1, but initial mortgage interest rate at origination is the dependent variable. All standard errors are clustered at the MSA level. P-Values: * 10%; ** 5%; ***1%.

	1st Stage	Reduced Form		@Origination Placebo tests		
	(1)	(2)	(3)	(4)	(5)	(6)
	LTV>1	%ΔDep	$Dep	$ Mo. Income	Credit Score	Int. Rate
SLTV>1	0.623***	-4.42***	-436.8***	-64.8	1.03	-0.0003
	(0.028)	(0.775)	(128.5)	(151.9)	(1.66)	(0.0003)
MSA x Time FE	Y	Y	Y	Y	Y	Y
Orig. Date x Time FE	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.587	0.072	0.045	0.045	0.152	0.727
Observations (mil)	5.375	5.375	5.375	5.375	5.375	5.375

Table III. Robust to Selection on Unobservables

This table provides evidence that after controlling for region-time, origination date-time, and household fixed effects a household's synthetic loan-to-value ratio (SLTV) greater than 100% is a valid instrument to look at the effect of negative home equity on household labor supply, focusing on tests that reveal differences in difficult to observe or unobservable characteristics. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing-location of moving, and varies at the region-time-cohort level. Column 1 regresses a dummy equal to 1 if a household's mortgage at origination was "Alt-A" or a "Liar Loan" on a dummy which equals 1 if the household's SLTV is greater than 100%, after including MSA x time, and origination date x time fixed effects. Column 2 is the same as column 1, but a dummy variable equal to one if the mortgage has no documentation at origination is the dependent variable. Column 3 is the same as column 1, but monthly gross verified income at origination is the dependent variable. Column 4 is the same as column 1, but the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the household's income at the time of mortgage origination, is the dependent variable and the regression includes household fixed effects. Column 5 is the same as column 4, but excludes any observations where a household actually has negative home equity. All standard errors are clustered at the MSA level. P-Values: * 10%; ** 5%; ***1%.

	@Origination Placebo tests			HH FEs	Placebo No Neg Eq
	(1)	(2)	(3)	(4)	(5)
	Alt-A	No Income Docs	Verified $ Mo. Income	%ΔDep	%ΔDep
SLTV>1	0.0002	-0.004	-6.1	-1.37***	0.08
	(0.0037)	(0.013)	(122.5)	(0.42)	(0.61)
MSA x Time FE	Y	Y	Y	Y	Y
Orig. Date x Time FE	Y	Y	Y	Y	Y
HH FE	N	N	N	Y	Y
Sample	All	All	All	All	Eq>0
Adjusted R^2	0.110	0.107	0.056	0.480	0.529
Observations (mil)	5.375	5.375	4.144	5.375	4.753

Table IV. Negative Home Equity and Labor Supply

This table shows the average change in household income associated with negative household home equity using variation in the timing of home purchase as an instrument for the probably of having negative equity. The instrument is a dummy variable equal to one if a household's synthetic loan-to-value ratio (SLTV) is greater than 100% after controlling for MSA-time, origination date-time, and household fixed effects. Column 1 shows the results of running the two-stage least squares procedure of regressing raw monthly deposit inflows as the dependent variable, without any normalization, on instrumented negative home equity. Column 2 is the same as column 1 but the dependent variable is the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the household's income at the time of mortgage origination. Column 3 is the same as column 2, but includes zip-time instead of MSA-time fixed effects. Column 4 is the same as 2, but includes time varying non-parametric household-level controls. These include deciles for origination income and property value, mortgage original interest rate by percentage buckets, and original credit score in bins of 50 all interacted with time fixed effects. Column 5 is the same as 2, but includes MSA x time x origination year fixed effects. Column 6 is the same as 2, but instead of origination date x time fixed effects it includes graduation year x MSA x time fixed effects among the subset of borrowers with outstanding student loans. All standard errors are clustered at the MSA level. P-Values: * 10%; ** 5%; ***1%.

			Zip FE	X_{it} Controls	Orig Yr x MSA FE	Grad Yr FEs
	(1)	(2)	(3)	(4)	(5)	(6)
	$Dep	%ΔDep	%ΔDep	%ΔDep	%ΔDep	%ΔDep
LTV>1	-298.1***	-3.47***	-3.77***	-4.94***	-2.20**	-5.63**
(IV: SLTV>1)	(61.3)	(1.18)	(1.13)	(1.03)	(0.89)	(2.97)
Region x Time FE	Y/MSA	Y/MSA	Y/ZIP	Y/MSA	N/A	Y/MSA
Orig. Date x Time FE	Y	Y	Y	Y	N/A	N
HH FE	Y	Y	Y	Y	Y	Y
HH Time Varying Controls	N	N	N	Y	N	N
MSA x Time x Orig Yr FE	N	N	N	N	Y	N
MSA x Time x Grad Yr FE	N	N	N	N	N	Y
F-Stat	2440.4	2440.4	2109.6	2304.8	110.25	126.1
Adjusted R^2	0.380	0.490	0.529	0.492	0.620	0.550
Observations (mil)	5.375	5.375	5.271	5.219	5.219	0.665

Table V. Robust to Income "Hiding"

This table explores the drivers of the negative effect of mortgage loan-to-value (LTV) on labor supply and show it is not driven by "hiding" of deposits with other institutions. Just as in the main specifications Column 1 regresses the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the household's income at the time of mortgage origination, on an instrumented dummy equal to one if current mortgage loan to home value is greater than 100%, MSA x time, origination date x time, and household fixed effects. A dummy which equals 1 if my synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. In this case though cases with 100% decline in deposits are completely excluded from the analysis. Column 2 is the same as column 1 but excludes any changes larger than 25%. Column 3 is the same as column 1, but does not exclude any deposits and the dependent variable is a dummy equal to 1 if the household receives any social security checks. These are defined as direct deposits received on the 3rd of the month, or the 2nd, 3rd, or 4th Wednesday that are not explained by regularly scheduled labor related direct deposits. Column 4 restricts the sample to the subset of borrowers with credit cards and associated credit bureau data. For this subset, I don't rely on mortgage data, so I know the approximate timing and zip code of origination (see text for more detail), but not the actual loan-to-value at or income at origination. I therefore regress the $ amount of deposits per month on the percent of mortgages in that estimated zip code x origination year x time with negative home equity, after including MSA x time, origination date x time, and household fixed effects. Column 5 is the same as column 4, but restricts the analysis to only households with mortgages not serviced or owned by *MyBank*. All standard errors are clustered at the MSA level. P-Values: * 10%; ** 5%; ***1%.

	(1)	(2)	(3)	(4)	(5)
	%ΔDep	%ΔDep	%GetSS	$Dep	$Dep
LTV>1	-3.35***	-0.07	0.91***		
(IV: SLTV>1)	(1.12)	(0.55)	(0.32)		
%NegEq				-48.8***	-65.0***
(Region x Cohort x Time)				(10.4)	(15.0)
Region x Time FE	Y	Y	Y	Y	Y
HH FE	Y	Y	Y	Y	Y
Orig. Date x Time FE	Y	Y	Y	Y	Y
Normalization	Orig Inc	Orig Inc	No	N/A	N/A
Dep/Mo Constraint	>$0	>-25%	N/A	N/A	N/A
Mortgage Servicer/Owner	All	All	All	All	Not *MyBank*
Orig Location/Date Method	Actual	Actual	Actual	Derived	Derived
Adjusted R^2	0.621	0.430	0.548	0.344	0.348
Observations (mil)	4.794	3.888	5.375	20.113	15.018

Table VI. Mortgage Mods, Negative Equity, and Labor Supply

This table shows how much of the relationship between income and current household mortgage loan to property value (LTV), can be explained by the household debt overhang channel. Column 1 regresses the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the household's income at the time of mortgage origination, on an instrumented dummy equal to one if the synthetic loan to value ratio (SLTV) measure is greater than 100%, MSA x time, origination date x time, and household fixed effects. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. This is also interacted with a dummy variable equal to one if a household ever receives a mortgage modification. Column 2 is the same as 1, but interacts instrumented negative home equity with the level of excess modifications per mortgage in a given MSA. This modification rate is the number of mortgages ever modified from 2010-2014 divided by the number of all outstanding mortgages over the same time period. The excess modification rate is the rate in a given MSA minus the average rate for all MSAs in the sample, divided by the standard deviation of these excess rates. Column 3 is the same as 2, but the excess modification rate per MSA is based on only those mortgages that have negative home equity at some point during the life of the mortgage. Column 4 is the same as 2, but use the excess ever 60+ day delinquency rate instead of modification rate by MSA for the interaction. Column 5 is the same as 2, but includes both the excess ever modified and ever 60+ day delinquency rates per MSA in the interactions. Column 6 is the same as 2 but instead of interacting negative SLTV with excess modifications, it interacts with a dummy variable equal to 1 if the state has judicial foreclosure requirements. The sample in this case is restricted to only households with 50 miles of a state border where the bordering states have different judicial foreclosure laws. The definition of state foreclosure laws comes from RealtyTrac. All standard errors are clustered at the MSA level. P-Values: * 10%; ** 5%; ***1%.

	(1)	(2)	(3)	(4)	(5)	(6)
	%ΔDep	%ΔDep	%ΔDep	%ΔDep	%ΔDep	%ΔDep
LTV>1	-2.72**	-2.94**	-4.14***	-3.95***	-4.02***	8.57
(IV: SLTV>1)	(1.30)	(1.19)	(1.08)	(1.08)	(1.08)	(6.67)
LTV>1 x Ever Mod	-5.68***					
(IV: SLTV>1 x Ever Mod)	(1.78)					
LTV>1 x MSA Excess Mods		-2.18**	-1.98**		-1.81*	
(IV: SLTV>1 x MSA Excess Mods)		(1.00)	(1.00)		(1.12)	
LTV>1 x MSA Excess DQ				-0.96	-0.45	
(IV: SLTV>1 x MSA Excess DQ)				(1.02)	(1.09)	
LTV>1 x Judicial State						-13.76**
(IV: SLTV>1 x Judicial State)						(5.87)
Region x Time FE	Y	Y	Y	Y	Y	Y
HH FE	Y	Y	Y	Y	Y	Y
Cohort x Time FE	Y	Y	Y	Y	Y	Y
MSA Excess Rate	N/A	Mod /MTG	Mod /Neg Eq	N/A	Mod /Neg Eq	N/A
Within X miles of Border	N/A	N/A	N/A	N/A	N/A	50
Adjusted R^2	0.478	0.480	0.479	0.478	0.480	0.611
Observations (mil)	5.375	5.375	5.375	5.375	5.375	0.146

Figure I. LTV vs. Income: Identification Based on Timing of Moving

This figure shows the relationship between income and current household mortgage loan to property value (LTV) after controlling for household specific factors and local demand shocks. This figure shows the coefficients of regression where I regress the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households' income at the time of mortgage origination, on dummies for various ranges of current (LTV) ratios, where house price is computed using original property value and changes in LPS MSA-level house price indices used by *MyBank* internally, MSA x time, origination date x time, and household fixed effects. In this figure the x-axis indicator dummies for each household-month that appears in a given 10% LTV bucket and the right hand side are the co-efficients from the regression (bold line). LTVs of 30-40% are the omitted group for comparison. 95% confidence intervals computing standard errors clustered at the MSA level, are shown in the shaded regions. P-Values: * 10%; ** 5%; ***1%.

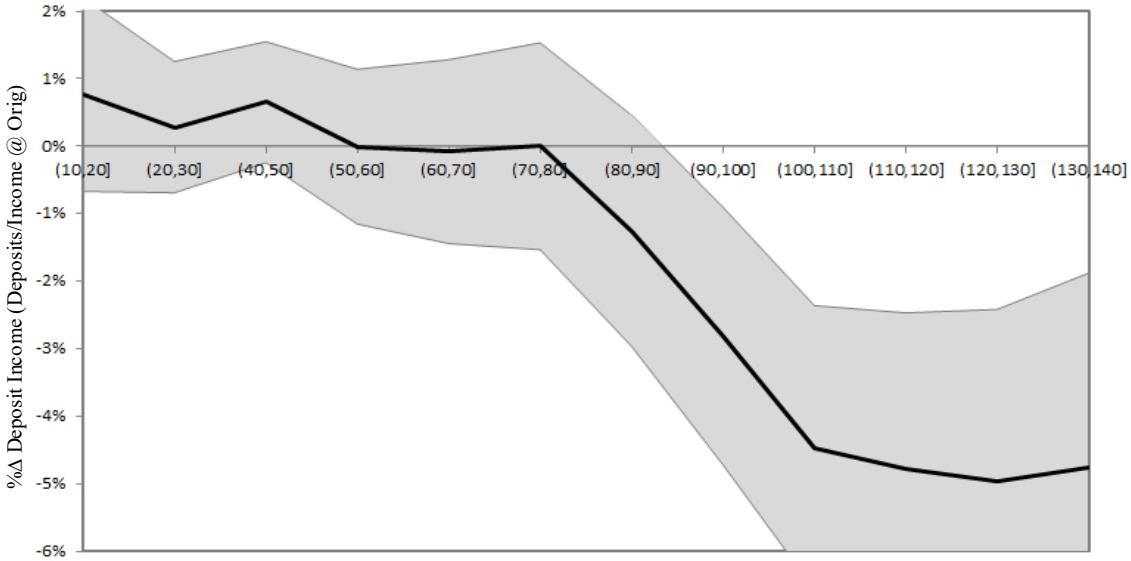


Figure II. LTV vs. Income: Identification Based HPI IV Reduced Form

This figure shows the average change in household income associated with negative household home equity using variation in the timing of home purchase as an instrument for the probably of having negative equity. This figure shows the coefficients of regression where I regress the % change in deposits, where the numerator is the monthly deposit inflows and the denominator is the households income at the time of mortgage origination, on dummies for various ranges of MSA-level house price index changes since mortgage origination, where house price is computed using original property value and changes in LPS MSA-level house price indices used by *MyBank* internally, MSA x time, origination date x time, and household fixed effects. In this figure the x-axis are indicator dummies for each household-month that appears in a given 10% HPI change bucket and the right hand side are the co-efficients from the regression (bold line). HPI changes of 20-30% are the omitted group for comparison. 95% confidence intervals computing standard errors clustered at the MSA level, are shown in the shaded regions. P-Values: * 10%; ** 5%; ***1%.

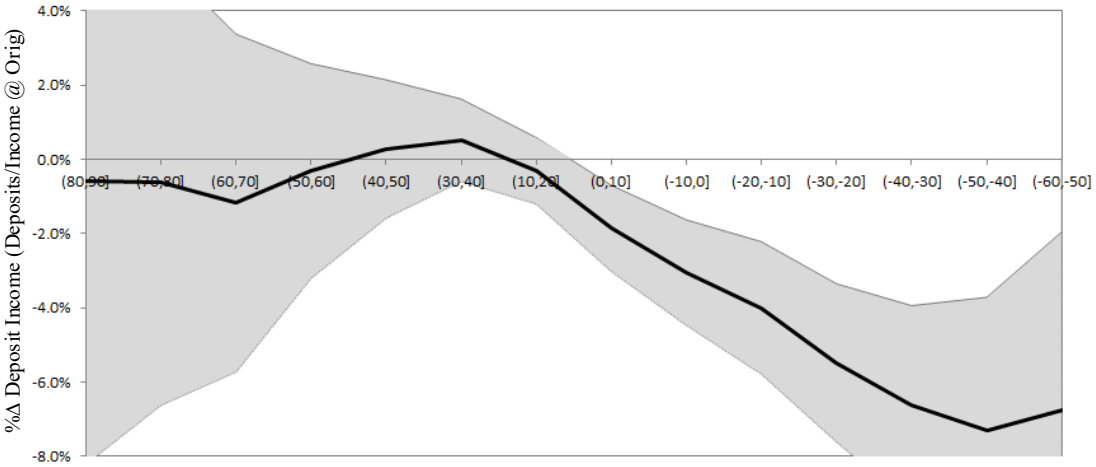


Figure III. Modification and Delinquency Rates vs. LTV

This figure shows how delinquency and modification rates vary with a household's mortgage loan to home value (LTV) ratio by 10% LTV buckets over the time period 2010-2014. Each unit of observation is at the household month level. The black line represents the % of households with a LTV ratio in a given month with the 10% range that will receive a mortgage modification within the next year. The red dashed line is the percent who are ever at least 60 days past due on any mortgage interest payments. P-Values: * 10%; ** 5%; ***1%.

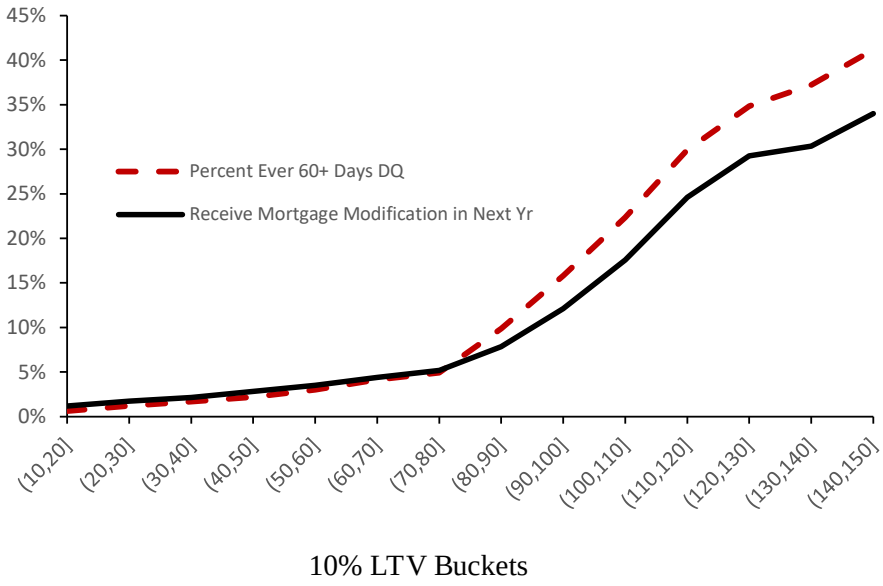
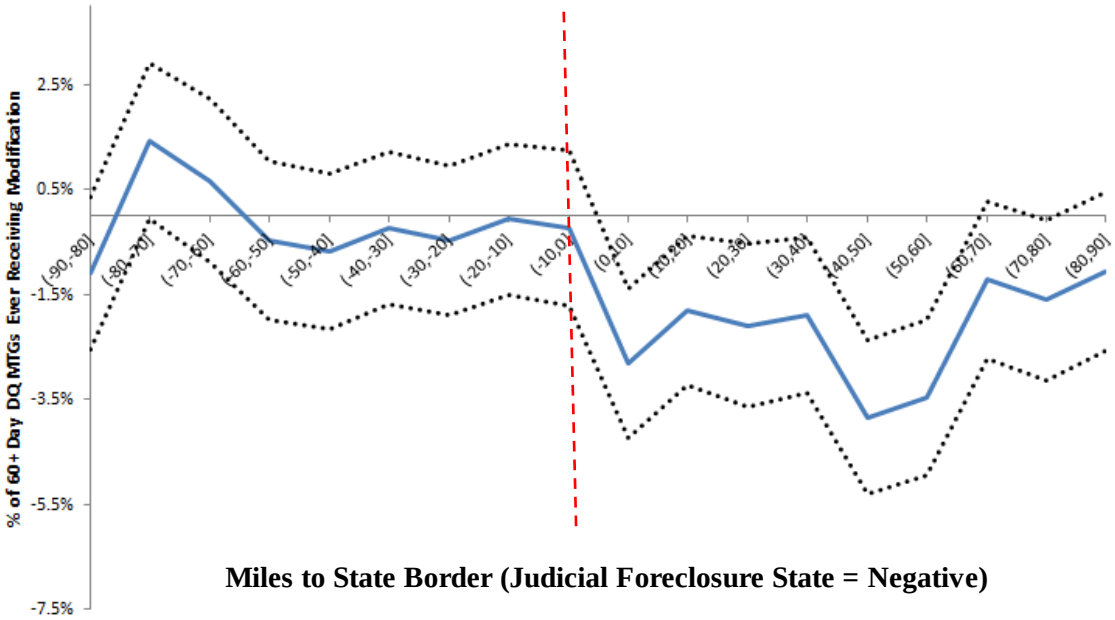


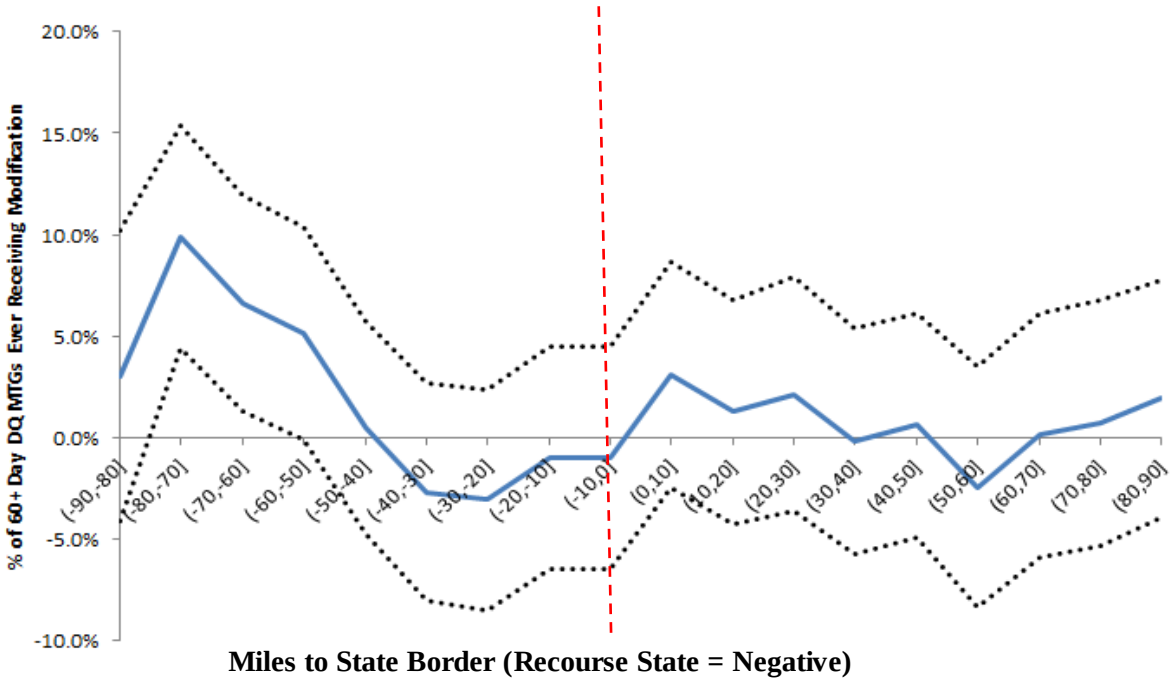
Figure IV. Mortgage Modifications and State Foreclosure Laws RD

This figure shows how mortgage modification rates discontinuously change at state borders with different foreclosure requirements. To be included in the sample it must be a MyBank mortgage, be at least 60+ days delinquent and at within at least 100 miles of a state border with differing foreclosure laws. Plotted in figure A are the coefficients of regressing mortgage modifications in a given month for mortgages 60+ days delinquent on fixed effects of each 10-mile bucket of distance from a state border for states with different laws concerning judicial foreclosure requirements (negative indicates a state with judicial foreclosure requirements), after controlling for 50 square mile fixed effects of the MSA of a mortgage. Figure B is the same as A, but focuses on recourse vs. non-recourse states, where negative values now refer to states with the possibility for recourse following mortgage foreclosures. Blue lines indicate all estimated coefficients, while the dashed black lines indicate 95% confidence intervals. The definition of state judicial foreclosure laws comes from RealtyTrac. The definition of state foreclosure laws comes from RealtyTrac. P-Values: * 10%; ** 5%; ***1%.

A. Mortgage Modifications and State Judicial Foreclosure Requirements



B. Mortgage Modifications and State Recourse Laws



Technical Appendix for Online Publication

Appendix A: Panel Data Construction

The data provider for this project is a major U.S. financial institution, who I refer to as *MyBank*, with transaction-level client account information on more than 1/4th of all U.S. households over the 5 years from 2010-2014. For the purposes of this project I focus on households with sufficient *MyBank* relationships to estimate income and mortgage information and analyze income decisions at a monthly household level. Income is estimated using retail account deposit information and mortgage information is either derived from credit bureau data (only available for households w/ *MyBank* credit card accounts) or *MyBank* mortgage account information. In table A1 I detail the effect on sample size and household characteristics when multiple *MyBank* accounts are combined at a monthly frequency.

Table AI. Effect of Panel Data Construction on Sample Size

Merging is done at HH-level. To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$25k, a mean and median level of deposits across all accounts >=$500 & <=$25k. To be "active" a HH must have at least $200 aggregated across all accounts in a month or at least $100 in deposits across all accounts. For direct deposits and assigned to jobs direct deposits the same restrictions apply as with deposits, but for direct deposits and assigned direct deposits only respectively, and >=75% of all deposits must be via the channel of interest. 1st row includes no filters, but all others that include retail include the filter.

	Median Ann. Deposits	Median MTG Bal	# HH-Mo Obs (mil)	# Acct (mil)	# Cust (mil)	# HHs (mil)
MyBank Retail Acct (*Raw*)	$23,556					
MyBank Retail Acct	$37,166					
MyBank Credit Card Acct		$152,268				
MyBank Mortgage		$116,255				
MyBank RTL & MTG	$63,780	$170,726	7.83	1.40	0.70	0.20
MyBank RTL & CC & Any MTG	$66,301	$222,626	24.42	4.84	1.99	0.62
MyBank RTL & CC & No MTG	$39,982	$0	30.13	6.22	2.43	0.96
MyBank RTL, CC, MTG	$73,011	$177,631	4.36	1.32	0.49	0.13
MyBank RTL, CC, & Non-*MyBank* MTG	$67,506	$228,569	16.58	4.30	1.75	0.54
MyBank RTL & CC & Non-*MyBank* & Direct Deposit Req.	$72,587	$224,421	5.52	1.14	0.45	0.17
MyBank RTL & CC & Non-*MyBank* & Assigned Direct Deposit Req.	$63,837	$210,748	0.88	0.15	0.06	0.03

Appendix B: Additional Tables/Figures

Table BI. Additional Summary Statistics

To be included in the panel all households must have at least 12 months with deposits across all accounts of >=$100 & <=$50k and a mean and median level of deposits across all accounts of >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel. All data winsorized at 99th percentile. Group A look at only households that have retail and credit card accounts at *MyBank* and a mortgage with any lender. Group B examines only the subset of households with mortgages either owned or serviced by *MyBank* from 2010-2014.

	Mean	Median	Std. Dev	#Obs (mil)	#HHs (mil)
B. Households w/ *MyBank* Retail & Credit Card Accounts & Any Bank Mortgage 2010-2014					
Retail Data					
Income (All)	$7,856	$5,525	$8,547	24.42	0.622
Income (Dir. Dep.)	$6,632	$5,358	$5,305	7.81	0.195
Savings	$33,440	$9,782	$58,140	24.42	0.622
Bank Card/Credit Bureau Data					
All Liabilities	$294,600	$258,600	$204,585	21.74	0.568
MTG Balance	$250,900	$222,600	$165,344	20.94	0.554
MTG Interest Rate	6.96%	6.75%	3.33%	21.60	0.565
Has Autoloan	30.4%			21.74	0.568
Has *MyBank* MTG	32.1%			24.42	0.622
Bal Used/Available All Credit	21.9%	7.0%	29.3%	20.49	0.550
FICO Bank Credit Score	768	782	73.1	21.74	0.568
C. Households w/ MyBank Mortgage					
Mortgage Data (@ origination)					
MTG Balance (000s)	169.7	139.5	113.0		
MTG Interest Rate (%)	5.88	5.75	1.30		
Income @ Origination	7,054	5,730	5,025		
Combined Loan-to-Value	73.1	77.47	19.9		
Is Fixed Rate	91.2%				

Table BII. *MyBank* Summary Stats vs. Survey of Consumer Finance

To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$50k and a mean and median level of deposits across all accounts >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel. All data winsorized at 99th percentile. This sample includes only households that have retail and mortgage accounts at *MyBank* from 2010-2014. Data from Survey of Consumer Finance (SCF) comes from 2010 and includes all households with a primary mortgage outstanding balance of at least $1,000 (13,580 households).

	SCF Median (2010)	*MyBank* Median	*MyBank* Std. Dev
Households w/ *MyBank* Retail & *MyBank* Mortgage 2010-2014			
Retail Data			
Income (All)	$5,083	$5,315	$8,439
Income (Dir. Dep. w/ Filter)	--	$5,172	$5,226
Savings	$7,850	$10,100	$60,626
Mortgage Data			
Current Loan-to-Value (%)	58.6	58.0	31.5
MTG Interest Rate	5.39	5.38	1.23
Is Fixed Rate	87.4%	83.9%	

Table BIII. Additional Robustness

This table shows that the negative effect of mortgage loan-to-value (LTV) on labor supply is robust to the choice of measuring income and the household. Just as in the main specifications Column 1 regresses the % change in deposits on an instrumented dummy equal to one if current mortgage loan to home value is greater than 100%, region x time, origination date x time, and household fixed effects. A dummy which equals 1 if a household's synthetic loan to value ratio (SLTV) measure is greater than 100% is used as an instrument for the likelihood that a household has negative home equity. SLTV is an instrument for loan-to-value that does not depend on household specific factors, except the timing of moving, and varies at the region-time-cohort level. As in the primary specifications the numerator is still the monthly deposit inflows, but in this case the denominator is the households average monthly deposit inflows over the entire sample period. Column 2 is the same as column 1, but includes direct deposits instead of all deposits and normalizes by the income at origination just as in the main specification. Column 3 is the same as column 1 but the dependent variable is the log of all monthly deposit inflows, with nothing in the denominator. For households with 0 deposits in a given month, but with a still active account $1 was included instead. Column 4 is the same as the main specification, but households are defined based on an internal *MyBank* identifier instead of using those people with a shared mortgage liability. All standard errors are clustered at the MSA level. P-Values: * 10%; ** 5%; ***1%.

	(1) %ΔDep	(2) %ΔDirDep	(3) log(1+Dep)	(4) %ΔDep
LTV>100 (IV: SLTV>100)	-4.08*** (0.76)	-5.28*** (1.26)	-3.69** (1.89)	-3.38*** (1.15)
Region x Time FE	Y	Y	Y	Y
HH FE	Y	Y	Y	Y
Cohort x Time FE	Y	Y	Y	Y
Adjusted R^2	0.030	0.397	0.572	0.475
Define HH	Shared MTG	Shared MTG	Shared MTG	Internal
Denominator	Mean Dep	Orig Income	N/A	Orig Income
Observations (mil)	5.375	5.375	5.375	5.670

Table BIV. Mortgage Mods and State Foreclosure Laws RD

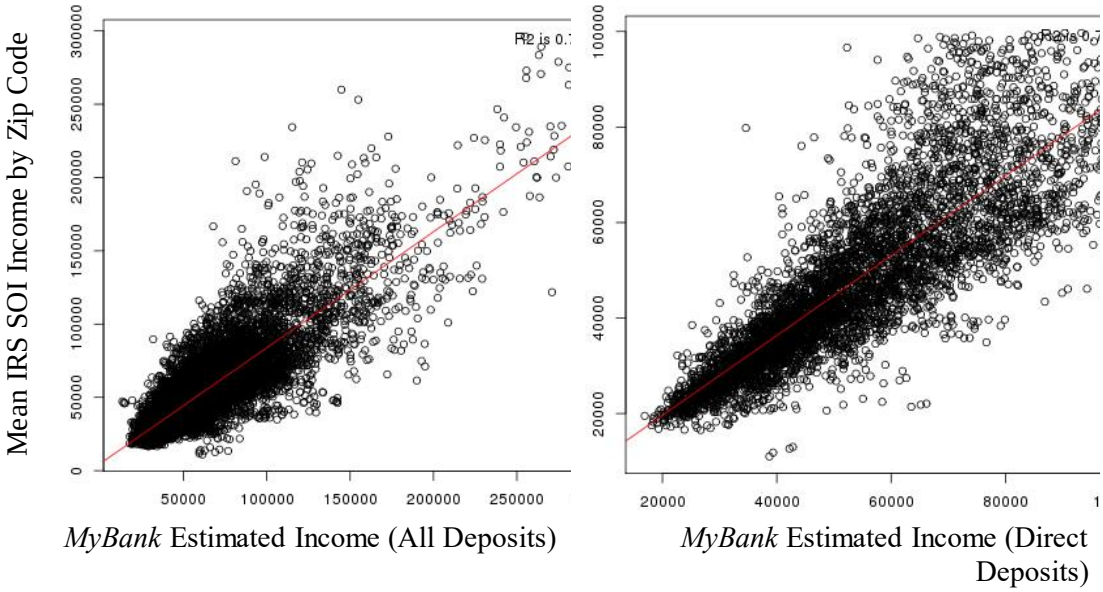
This table examines how mortgage modification rates vary discontinuously along state borders with differing foreclosure laws. The sample includes a cross-section, not time series component, of any MyBank mortgage account, not household, within 50 miles of a state border with differing foreclosure laws from 2010-2014. In column 1 I regress a dummy variable equal to one if the mortgage ever receives a modification on a dummy variable equal to one of the state has judicial foreclosure requirements, after controlling for 100 square mile fixed effects around the border, distance to border (signed to be negative if state has judicial foreclosure requirements), distance to border squared, and distance to border cubed. Column 2 is the same as 1, but includes only those mortgages that are ever at least 60+ days delinquent. Column 3 is the same as 2, but includes only those mortgages that are ever at least 90+ days delinquent. Column 4 is the same as 2, but only includes those MSAs on state borders with differing recourse laws in foreclosure and a dummy variable for a state having recourse laws, instead of the judicial foreclosure dummy. Standard errors are clustered at the MSA level. P-Values: * 10%; ** 5%; ***1%.

	(1) Mod (%)	(2) Mod (%)	(3) Mod (%)
Jud Foreclosure State	0.862***	3.592***	4.022***
	(0.132)	(0.632)	(0.689)
100 mile Lat/Long FEs	Yes	Yes	Yes
Dis. State Border Controls	Yes	Yes	Yes
Within X miles of Border	50	50	50
Max Days DQ	N/A	60+	90+
Adjusted R^2	0.0043	0.0071	0.0084
Observations (mil)	1.311	0.206	0.179

Figure BI. Validity of Income Measure

A. Zip-Code Level Mean Income IRS SOI vs. MyBank (2010-2013)

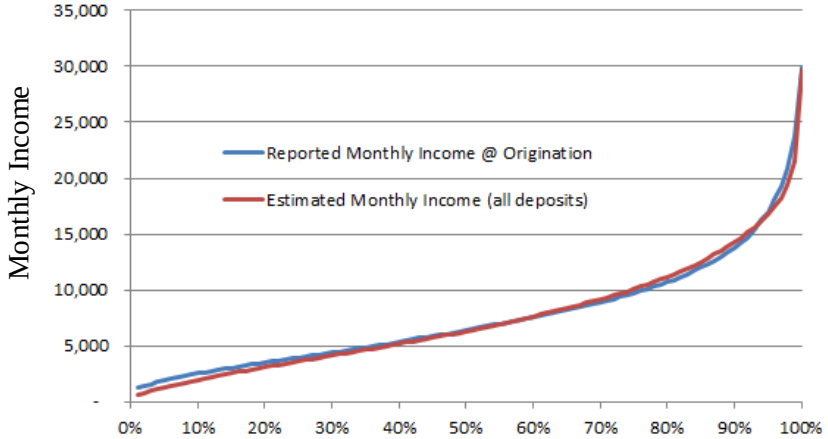
These figures compare the mean incomes by zip code from 2010-2013. To be included there must be at least 4,000 IRS SOI returns and at least 1,000 *MyBank* observations per zip-code year w/ filters applied. To be included in the panel all households must have at least 12 months with deposits across all accounts >=$100 & <=$25k, a mean and median level of deposits across all accounts >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel.



Correlations	All Deposits	All Direct Deposits	All Jobs
MyBank Retail Acct	0.832	0.886	0.911
MyBank RTL, CC, & Any MTG	0.838	0.777	0.736

B. Estimated Income vs. MyBank @ Origination Distribution

This figure compares the cumulative distribution of reported income at mortgage origination for *MyBank* mortgages with the estimated income based on retail deposits for all households in the same calendar year for all households with data available for both, who meet the filter requirements. To be included in the panel all households must have at least 12 months with deposits across all accounts and years >=$100 & <=$25k, a mean and median level of deposits across all accounts and years >=$500 & <=$25k. For direct deposits the HH must have at least 12 months of direct deposits >=$100 & <=$25k, a mean and median level of direct deposits across all accounts >=$500 & <=$25k and >=75% of all deposits must be via the direct deposit channel. The table below includes the pair-wise individual correlations for each household for all three measures of income.



Correlation	All Deposits	Direct Deposits	Job Direct Deposits
MyBank RTL & CC & Any MTG	0.378	0.511	0.449

Figure BII. Validity of Delinquency Measure

This figure compares a time series of mortgage delinquency rates for households with mortgage at *MyBank* using *MyBank's* internal mortgage data with national seasonally adjusted quarterly mortgage delinquency rates published by Federal Reserve Economic Data (FRED) from 2009-2014. Quarterly data from are interpolated between quarters to provided monthly estimates. The green and blue top lines for both FRED and *MyBank* represent the percent of all mortgages that are at least 30 days past due. The red bottom line represents all *MyBank* mortgages that are at least 90 days past due.