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Negative Home Equity and Household Labor Supply

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ABSTRACT

Using U.S. household-level data and plausibly exogenous variation in the location-timing of home purchases with a single lender, I find that negative home equity causes a 2% to 6% reduction in household labor supply. Supporting causality, households are observationally equivalent at origination and equally sensitive to local housing shocks that do not cause negative equity. Results also hold comparing purchases within the same year-metropolitan statistical area that differ by only a few months. Though multiple channels are likely at work, evidence of nonlinear effects is broadly consistent with costs associated with housing lock and financial distress.

FOLLOWING THE HISTORIC DECLINE IN HOUSE prices during the recent financial crisis, more than 15 million U.S. mortgages, or approximately one-third 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

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

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levels for years after the crisis. Although these declines may have been driven by common factors, previous work (e.g., Mian and Sufi (2014), Verner and Gyöngyösi (2020)) suggests a causal link between employment and housing wealth whereby house price shocks altered 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 a significant relationship between negative home equity and labor supply exists it could improve our understanding of household financial decision-making as well as provide potentially important implications for marcoprudential policies.

The primary contribution of this paper is to provide the first causal empirical estimates of the effect of negative home equity on overall household labor supply. Using U.S. household-level data and plausibly exogenous variation in the location-timing of home purchases with a single lender, I find that instrumented negative equity is associated with a 2% to 6% reduction in household income. These results are consistent with recent papers indicating that negative home equity could negatively affect labor supply. In particular, prior evidence suggests that a reduction in home equity could reduce entrepreneurship (Adelino, Schoar, and Severino (2015), Schmalz, Sraer, and Thesmar (2017)), innovation and effort (Bernstein, McQuade, and Townsend (2021)), employment opportunities among impoverished households (Bos, Breza, and Liberman (2018)), labor mobility (Ferreira, Gyourko, and Tracy (2010), Bernstein and Struyven (2019)), job search (Brown and Matsa (2020)), and labor income among bankrupt households (Dobbie and Song (2015)). Although these results are suggestive, the effects on labor supply are unclear. First, many of these channels are likely to impact only a subset of homeowners, such as entrepreneurs, innovators, or bankrupt/impoverished households,[2] while others, such as housing lock and job search, are subject to debate.[3] Moreover, other channels such as wealth effects may predict the exact opposite pattern. For example, prior evidence suggests that exogenous increases in wealth, via lottery winnings (Imbens, Rubin, and Sacerdote (2001), Cesarini et al. (2017)) or inheritance windfalls (Joulfaian and Wilhelm (1994)), reduce labor supply. These channels would predict an increase, rather than a decrease, in labor supply from negative home equity, due to a reduction in housing wealth. The different predictions of potential channels mean that the net effect negative home equity

[2] For example, Bos, Breza, and Liberman (2018) focus on households that 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, for example, being unable to use a credit card to buy a suit, seem unlikely to extend to the average U.S. homeowner.

[3] Housing lock refers to households being unable to move due to the financial constraints associated with negative equity. Since the effectively nonrecourse nature of mortgages in the United States, the effect of housing lock on mobility is unclear. Empirical evidence has been mixed, with papers such as Schulhofer-Wohl (2012) and Mumford and Schultz (2014) finding no evidence of reduced mobility. Modestino and Dennett (2013) further point out that while nonpecuniary costs of immobility could be large, few households have to move for employment in a given year, so the effect on aggregate labor supply may be limited.

on labor supply is an empirical question. The findings in this paper of a positive relationship between housing wealth and labor supply that is nonlinear for households with negative home equity suggests that labor market disruptions associated with housing market frictions have significant economic impacts on labor supply.[4] In particular, nonlinear effects are most consistent with housing lock and financial distress as mechanisms driving the observed relationship between negative home equity and household income.

Empirical identification of the effect of negative home equity on labor supply faces a number of challenges. First, few data sets 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 identification.[5] Moreover, 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 (e.g., local labor demand shocks), and reverse causality could be a problem since wealthier households are likely to invest more in home improvements.

In this paper, I overcome these challenges using a new transaction-level data set with comprehensive information on assets, liabilities, and deposits for all customers of a major U.S. financial institution from 2010 to 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 allow me to determine which households have negative home equity. Since I observe actual deposits rather than reported values, estimated effects represent actual changes in deposit behavior rather than changes in household reporting in response to eligibility criteria.[6]

To overcome identification issues, I exploit plausibly exogenous variation in home equity from the interaction between the location and timing of home purchases, relative to households in the same region, as an instrument for the probability that a household has negative home equity. Under this empirical strategy households are exposed to identical time-varying local house price shocks but differ in their home equity based on when they purchased their home relative to their neighbors. Since variation in the timing of home

[4] The findings in this paper are also related to recent findings in Sodini et al. (2017) that home ownership 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 renting to owning, 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 driven at least in part by some of the aforementioned frictions that exist for homeowners with a significant amount of mortgage debt.

[5] For example, Cunningham and Reed (2013) use AHS data, but have only 652 household-year observations with negative equity over a nine-year period, which is a very small sample for something as noisy as self-reported household equity and labor income.

[6] Chetty, Friedman, and Saez (2013) show that in the context of household response to the earned income tax credit (EITC), individuals manipulate self-employment reported income.

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 instrumental variables. First, I show that for low levels of expected loan-to-value (LTV), 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 nonparametric household-level controls for household characteristics that could be related to local demand shock sensitivity.

A growing body of evidence (e.g., Mian and Sufi (2009), Palmer (2015)) suggests that lending standards may have changed in the run-up to the financial crisis, leading to potentially different sensitivities to local demand shocks for households who bought earlier versus later. 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 in different regions, and I compare them with households that 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 metropolitan statistical area (MSA), relative to their neighbors, but not earlier or later overall. This flexible set of controls means that any observed relationship between instrumented home equity and labor income cannot be spuriously driven by changes in nationwide lending standards by *MyBank* or the entry of subprime lenders during the boom.

A 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. For example, if *MyBank* increased lending more to low-credit quality or subprime borrowers in areas that subsequently experienced larger house price declines, that could potentially confound causal interpretation of the observed relationship. Although 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 this would show up in the form of lower income, lower credit scores, or higher interest rates at the time of initial origination of the loan. Given the relatively strong power observed for most of these tests, it is unlikely that any substantive difference in the observable characteristics of these borrowers is correlated with the instrument, which works against substantive differences in the "hard information" lenders used at origination across the 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, which works against differential use of "soft information" across these regions as well.

Although it seems unlikely, it remains possible that unobservable differences in these households exist that make 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 continues to be associated with a decline in labor income. I next take advantage of the fact that most of the proposed theories for why reductions in home equity can reduce labor supply are based on frictions that are nonlinear when households have negative home equity. In a placebo test in which I exclude all observations for which a household actually has negative home equity, I show in reduced form that changes in the instrument, which 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, which are likely to be correlated with local demand shocks, have no differential effect on household income. Consistent with this result, nonlinear forms of the analysis show no relationship between instrumented home equity and income, even for large variation in instrumented home equity, except for cases in which properties are likely to have negative equity. Placebo results further show that these households are unlikely to differ even on unobservables that make them more sensitive to local demand shocks, except through the treatment of negative home equity. Supporting this causal interpretation, I show that my results hold when comparing households that bought in the same MSA and year but at different times of the year, just a few months apart. The results also hold among the subsample for which I can observe student loans and control for the approximate date they enter the labor market.

One final concern that I address is that households with *MyBank* mortgages and negative equity could be systematically hiding income from the institution they owe money. I only capture deposit inflows at *MyBank*, it is possible however, that households with mortgages at *MyBank* could be closing accounts or reducing payroll inflows at that institution to appear less able to pay and receive more assistance. To alleviate this concern, throughout the analysis I use multiple restrictions to ensure that households in the panel have active retail accounts, taking advantage of the inflow and level information available 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 a mortgage not owned or serviced by *MyBank*. In this case the household has no incentive to hide deposits. I find that negative equity continues to reduce income. Overall, the results are consistent with income shrouding playing little role in the observed decline in deposits, and shows the results represent actual declines in overall household income.

The paper is organized as follows. In Section I, I begin by describing the unique household-level financial information from a major U.S. financial institution used in this paper. In Section II, I describe the empirical challenges for identification in more detail and the methods I employ to overcome them. I discuss the empirical findings in Section III. In Section IV, I discuss potential mechanisms that could explain the observed relationship between home equity and household labor supply. Section V concludes the paper.

I. Data Description and Validation

The majority of my data come 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 and state-level judicial foreclosure law information. The data provider for this study is a major U.S. financial institution, which I refer to as *MyBank*, with transaction-level client account information on more than one-fourth of all U.S. households over the five years from 2010 to 2014.[7] For the purposes of this study, I focus on households with sufficient *MyBank* relationships to estimate income and mortgage information, and analyze income decisions at the monthly household level. Income is estimated using retail account deposit information and mortgage information comes from credit bureau data (only available for households with *MyBank* credit card accounts) or *MyBank* mortgage account data. In Section I of the Internet Appendix, I detail how combining household information from multiple *MyBank* accounts alters the sample size.[8]

For each mortgage account, I have detailed information on mortgage type (e.g., fixed-rate, 30-year), characteristics at origination including date, reported income, credit score, interest rate, and appraised LTV, as well as ongoing monthly mortgage performance, characteristics, and actions, including delinquency status, current LTV updated using internal Lender Processing Service (LPS) MSA-level house price index (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 IA.3 in the Internet Appendix, I show that the time series of delinquency rates for *MyBank* mortgage data closely matches the levels and trends seen in national Federal Reserve economic mortgage data over the past five years.

The largest population of households with a *MyBank* relationship are credit card customers, by a substantial margin. This is not surprising as households often have only one mortgage lender but multiple credit cards. For each credit card account and month, *MyBank* pulls credit bureau data on the associated

[7] According to census.gov, over the period from 2009 to 2013, out of 116 million *MyBank* client accounts cover more than 31 million U.S. households (see Table IA.I in the Internet Appendix for details), or 27% of all U.S. households. Coverage is lower when looking at individuals, likely because dependents often do not have separate *MyBank* accounts and some households with multiple adults may choose to list only one person in the account information. *MyBank* has consistently been one of the five largest U.S.-based banks, with borrowers across all 50 states. Mortgages originated by *MyBank* are often securitized and sold off, but origination details remain available for analysis in my sample.

[8] The Internet Appendix is available in the online version of this article on *The Journal of Finance* website.

customer liabilities. For the purposes of this paper, these monthly frequency credit bureau data are the only information used from the credit card accounts. The credit bureau data include comprehensive data on all customer liabilities across all lenders including mortgages, autoloans, student loans, home equity lines of credit, credit cards, and installment credit, as well as monthly updated credit scores. For each credit category the data include information on the balance, monthly payments, and initial balance.[9]

Retail accounts include any checking or savings accounts. The raw data include 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 (e.g., "ACH direct deposit"). The data set includes billions of transactions over the period 2010 to 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 (teller and automated teller machine), and other deposit types including mobile remote deposit capture deposits. Since some of these accounts are not used to deposit the majority of income, I restrict analysis to households with active accounts that appear to contain the majority of their income.[10,11]

To confirm the validity of using deposits as an income measure, I compare the average annual income based on my deposit data at the zip code level with those reported by the IRS Statistics of Income (SOI) over the period 2010 to 2013.[12] In Figure IA.I in the Internet Appendix, you can see a strong correlation between these measures of income. Specifically, regardless of the income measure used and the subsample explored, I find that zip code level correlations between the income measure and the IRS SOI are high, ranging from 0.736 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 that receive social security

[9] Maturities and interest rates on these liabilities are estimated and validated for the subset of data for which 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 the implied interest rate and maturity from a selection of discrete interest rates and maturities that exist in the data for each set of consecutive months. If less than 75% of estimated interest rates and maturities for a given product do not match or I have fewer than 20 observed estimates, I do not include them in the sample. Even with sufficient information these could have floating rates, nonstandard amortization schedules, or unusual pre-payment behavior that would confound clean identification of the underlying maturities and rates.

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

[11] To be included in the panel, households must have at least 12 months of deposits across all accounts $\geq$\$100 and $\leq$\$25,000, and mean and median level of deposits across all accounts $\geq$\$500 and $\leq$\$25,000.

[12] For the purposes of income validation, I use publicly available zip code level income data from the IRS SOI for 2010 to 2013. These data are 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.

or disability checks. After excluding regularly scheduled job-related deposits, I assign any remaining direct deposits that are paid on either the third of each month or the second, third, or fourth 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 closely matches the mean of $1,268/month I find per social security recipient in my sample. This result validates not only the data overall, but also the method of extracting social security payments that I employ.

In the majority of my analyses, I focus on households with retail deposits at *MyBank* that allows me to observe their income, and mortgages at *MyBank*, which allows me to observe their home equity. This focus results in an initial sample of approximately 200,000 households that represent approximately 7.8 million household-month observations. Unless stated otherwise, I focus on households with income at origination, loan origination date, and additional information, which reduces the sample to approximately 5.4 million household-month observations. In a robustness check, I also consider households with *MyBank* retail and credit card accounts but with mortgages originated and serviced by any lender, which increases the sample to about 20.1 million household-month observations. For more details on sample construction, see Table IA.I in the Internet Appendix.

I analyze a broad range of characteristics for each *MyBank* subsample in Table I andin more detail in Table IA.I in the Internet Appendix. From the tables we can see that the median household income for households with mortgages is about $5,000 to $6,000 per month and, as expected, the majority of household liabilities are mortgage related. The median income, nonhousing financial assets, mortgage leverage, and mortgage interest rate are similar to self-reported information collected by the Survey of Consumer Finances for households with an active mortgage balance of at least $1,000 in 2010, which is consistent with the representative nature of *MyBank*'s national coverage and lends support to the external validity of the conclusions of this paper. For more details on the comparison, see Table IA.III in the Internet Appendix.

The *MyBank* mortgage data include information on reported income at origination, which allows me to test the validity of data matches across lines of business as well as provide another check on the quality of my deposit-based income measure. In Figure IA.2 in the Internet Appendix, I plot the cumulative distribution function of income at origination and income based on deposits for a matched 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. These results lend credibility to the internal matches across *MyBank* lines of business and validate my income measure across the income distribution.

Table I

Summary Statistics

This table presents summary statistics for *MyBank* data. To be included in the panel, households must have at least 12 months with deposits across all accounts ≥\$100 and ≤\$50,000 and a mean and median level of deposits across all accounts ≥\$500 and ≤\$25,000. For direct deposits, households must have at least 12 months of direct deposits ≥\$100 and ≤\$25,000, a mean and median level of direct deposits across all accounts ≥\$500 and ≤\$25,000, and ≥75% of all deposits must be via the direct deposit channel. All data are winsorized at 99th percentile. This sample includes only those households that have retail and mortgage accounts at *MyBank* over the period 2010 to 2014

	Mean	Median	*SD*	Number of Obs (Millions)	Number of HHs (Millions)
Households w/ *MyBank* Retail Accounts and *MyBank* Mortgage 2010 to 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
Subprime	10.2%			7.835	0.200
Jumbo	19.3%			7.835	0.200
Stated income	13.3%			7.835	0.200
Single-family residential	88.7%			7.835	0.200
Is fixed rate	83.9%			7.835	0.200

II. Empirical Method

To provide evidence on 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, 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 and region r that originated their mortgage in month c, I regress household income, y_{icrt}, on a dummy variable that equals 1 if the household's LTV ratio, LTV_{it}, is greater than l_k and less than h_k for k LTV buckets, region × time fixed effects, γ_{rt}, and cohort (month of mortgage origination) × 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 are likely to invest more in home maintenance. Since I compute changes in house prices at the MSA level, the inclusion of MSA × time fixed effects precludes the possibility that the results are driven by variation in local demand shocks or individual variation in home investment. The cohort × time fixed effects imply that the analysis is not confounded by changes over time in the nationwide composition of borrowers at *MyBank* or the entry of subprime borrowers during the boom. I also include multiple LTV indicator buckets to see if, as many theories of labor market frictions arising from housing wealth would predict, declines in income occur only for high LTV ratios.

Despite the inclusion of all of 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 household's current LTV into three distinct components—(i) house prices changes, (ii) changes in the balance of the mortgage, and (iii) origination LTV—as follows:

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

Since households with improved income are more likely to prepay their mortgage, reducing their LTV, prepayment poses an empirical challenge for identification. To circumvent this concern, 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):

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

Equation (3) varies across mortgages based on the age of the loan but does not depend 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 such as LTV at origination are only a concern when interacted with a time-varying factor, as is the case here. In particular, if individuals with high LTV at origination 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 of households. Using equation (4), I get a "synthetic" LTV, or *SLTV*, which varies only 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:

$$\text{S}LTV_{rct} \equiv LTV_c \times \frac{\left(1+\%\Delta\text{Synth}Loan_{ct}\right)}{(1+\%\Delta HPI_{rct})}. \tag{4}$$

After including all controls in equation (1), variation in SLTV will be driven almost entirely by how fortunate the timing of house purchase was for a household in 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. Although LTV, which is an endogenous variable, is driven by time-varying household-specific factors, such as the origination LTV that individuals choose, the instrument for LTV, namely, SLTV will not have any variation coming from individual-level decisions.

To formalize the instrumental variable approach, I run the two-stage least squares (2SLS) regression:

$$\begin{aligned} U_{it} &= \alpha + \gamma_{rt} + \phi_{ct} + \delta_1 \cdot 1_{\{SLTV_{rct}\geq 100\}} + X_{it}'\beta + \eta_{icrt} \\ y_{icrt} &= \alpha + \gamma_{rt} + \phi_{ct} + \delta_2 \cdot \hat{U}_{it} + X_{it}'\beta + \epsilon_{icrt}, \end{aligned} \tag{5}$$

where a household that has negative home equity (i.e., is underwater) is denoted by $U_{it} \equiv 1_{\{LTV_{it}\geq 100\}}$.[13] Since SLTV is computed using region-level house

[13] I run this regression using the first stage as a linear probability model and using negative SLTV as the instrumental variable. For robustness, I use multiple LTV bucket indicators in the first stage but not probit or linear-linear models. As noted in many papers (e.g. Greene (2002)), probit estimates are inconsistent in a fixed effects panel regression as are purely linear models when the underlying treatment effect varies nonlinearly.

prices since origination, the remaining variation comes from the region × origination date interactions. In other words, a buyer is instrumented to have negative home equity via SLTV not because they bought early or later overall or in a region that saw worse declines, but because they bought at a specifically bad time in that region relative to those buying at that time in other regions. After household fixed effects are included, the resulting variation comes from whether a household is more likely to have positive versus negative home equity based on the timing of their home purchase within a given region. This approach takes advantage of not only the timing of home purchase, but also the nonlinearity of the treatment effects. The necessary assumption for the exclusion restriction is that after controlling for all fixed effects, SLTV affects income only via the probability the house has negative home equity. I find support for this assumption 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 robust to a wide range of variations of specification (6), including the primary specification, which controls flexibly for time-invariant characteristics via household fixed effects, compares just those households that bought their homes within the same MSA and year, separated by only a few months.

III. Results

A. 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 2SLS regression of specification (6). In particular, I use a dummy variable equal to 1 if SLTV is greater than 1, after controlling for region-time and origination date-time fixed effects. As detailed in Section II, SLTV is a measure of home equity based on the timing of home purchase in a given MSA that does not 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 × time and origination date × time fixed effects if the SLTV is greater than 1 a mortgage is a statistically significant 62.3 percentage points more likely to 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

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 examine the effect of negative home equity on household labor supply based on observable characteristics. SLTV is an instrument for LTV that does not depend on household-specific factors, except the timing-location of a move, and that varies at the region-time-cohort level. Column (1) regresses a dummy equal to 1 if a household's current LTV is greater than 100% on a dummy equal to 1 if the household's SLTV is greater than 100%, after including MSA × time, and origination date × time fixed effects. This gives the first stage estimate of an IV regression. Column (2) is the same as column (1), but uses the percent change in deposits as the dependent variable, 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 (1), but uses raw monthly deposits as the dependent variable, without any normalization. Column (4) is the same as column (1), but uses monthly gross reported income at origination as the dependent variable. Column (5) is the same as column (1), but uses the credit score at origination as the dependent variable. Column (6) is the same as column (1), but uses the initial mortgage interest rate at origination as the dependent variable. All standard errors are clustered at the MSA level. p-Values: *10%, **5%, and ***1%

	First 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 × Time FE	Y	Y	Y	Y	Y	Y
Orig. Date × 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

reported at origination, as well as raw observed household income per month. These results are suggestive of a potential causal link between negative home equity and labor supply. The remaining requirement for causal interpretation is satisfaction 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 in a way that predicted future changes in house prices and differences in local demand sensitivity of the sample borrowers, that would confound causal interpretation of these findings. Since the specifications in Table II do not include household fixed effects, I can test explicitly for evidence of observable differences in these borrowers at the time of origination correlated with the instrument. In Table II, columns (4) to (6), I show that negative STLV does not 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 find no evidence that *MyBank* was lending to borrowers whose income was more sensitive, ex ante, to house price movements. In particular, borrower-reported income at origination was not lower, the credit agency determination of credit quality or sensitivity to future economic shocks was not higher, and even a proxy for the bank's internal measure of risk—the mortgage interest rate charged to the borrower—did not differ for these households. The finding of no differences in borrower characteristics and in particular no differences in origination interest rates are especially helpful for causal interpretation in light of evidence from Hsu, Matsa, and Melzer (2014). Di Maggio and Kermani (2016) and Hsu, Matsa, and Melzer (2018) find that unemployment insurance can be a stabilizing force for housing markets that reduces defaults and Hsu, Matsa, and Melzer (2014) present evidence that this can induce lenders to provide easier credit in states with larger unemployment benefits. The authors find that for a "$3,600 increase in maximum UI benefits, [they] estimate that interest rates for first-lien mortgage loans decline by about 10 basis points." This makes sense if lenders are aware ex ante of regional variation in employment risk for mortgage default and incorporate such variation into the costs of borrowing. My finding of a fairly precise null for the effects of the proposed instrument on origination interest rates suggests that any effects of the instrument are unlikely to be driven by systematic differences in labor market conditions or regulations at the time of origination, while the inclusion of region $\times$ time fixed effects alleviates concerns about ex post differences in local labor market outcomes.

One limitation of looking at self-reported characteristics, such as income at origination, is that borrowers might be misreporting these values systematically (Mian and Sufi (2017)). To alleviate that concern, in Table III, columns (1) to (3), I show that these borrowers are not any more likely to originate a mortgage that is Alt-A ("liar loans") or without income documentation, and that focusing on verified income only still reveals no income difference at the time of origination.

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 examine 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 LTV that does not depend on household specific factors, except the timing-location of a move and that 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 equal to 1 if the household's SLTV is greater than 100%, after including MSA × time and origination date × time fixed effects. Column (2) is the same as column (1), but uses a dummy variable equal to 1 if the mortgage has no documentation at origination as the dependent variable. Column (3) is the same as column (1), but uses monthly gross verified income at origination as the dependent variable. Column (4) is the same as column (1), but uses the percent change in deposits as the dependent variable, where the numerator is monthly deposit inflows and the denominator is the household's income at the time of mortgage origination and the regression includes household fixed effects. Column (5) is the same as column (4), but excludes observations for which a household has negative home equity. Column (6) is the same as column (5), but includes the interaction between negative SLTV and demeaned monthly percent changes in aggregate deposits by MSA. All standard errors are clustered at the MSA level. p-Values: *10%, ** 5%, and ***1%

	@Origination Placebo Tests			HH FEs	Placebo No Neg. Eq.	
	(1)	(2)	(3)	(4)	(5)	(6)
	Alt-A	No Income Docs	Verified $ Mo. Income	%ΔDep	%ΔDep	%ΔDep
SLTV > 1	0.0002	–0.004	–6.1	–1.37***	0.08	0.08
	(0.0037)	(0.013)	(122.5)	(0.42)	(0.61)	(0.61)
SLTV > 1 × %ΔMSA Dep						0.17
						(0.24)
MSA × Time FE	Y	Y	Y	Y	Y	Y
Orig. Date × Time FE	Y	Y	Y	Y	Y	Y
HH FE	N	N	N	Y	Y	Y
Sample	All	All	All	All	Eq > 0	Eq > 0
Adjusted R^2	0.110	0.107	0.056	0.480	0.529	0.531
Observations (mil)	5.375	5.375	4.144	5.375	4.753	4.753

Despite finding no evidence of differences on important observable characteristics at origination, it is possible that the borrowers differed on some unobservable qualities that makes them more sensitive to local demand shocks. To address this concern, I first show in Table III, column (4), that negative SLTV continues to be associated with a decline in income after including household fixed effects to control flexibly for any time-invariant component in any unobservable differences. However, these specifications still do not address differences in unobserved household sensitivity to time-varying shocks. To address that concern, I use the fact that the theories likely to explain a positive relationship between housing wealth and labor supply predict that the effects concentrate among households with negative home equity. In Table III, column (4), I show that when 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, house price changes become unrelated to household income. In other words, these households are not more sensitive to local demand shocks in general, but rather only when such shocks lead them to have negative home equity. The nonlinearity of this sensitivity is also confirmed in Figure 1, which depicts the relationship between nonlinear categorical dummies for LTV and the percent change in household income since origination, after controlling for MSA $\times$ time, origination date $\times$ time, and household fixed effects.

As can be seen in Figure 1, 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 nonlinearly in household income, and remains low for households with negative equity.[14] Figure 2, Panel A, 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. The direct nonlinear effects for the first stage and reduced form are presented explicitly in Figure 3.

As can be seen, there is no change in income for large changes in SLTV, if SLTV is relatively low. Only when SLTV approaches 100%, does income begin to fall. This happens to coincide almost exactly with when we see increases in the probability a household has negative home equity—the first stage. Again, for cases in which households are unlikely to have negative home equity but have large relative variation in housing wealth, there is no change in household income. Only when housing wealth declines are likely to lead to an increased probability of negative home equity is there a nonlinear and persistent decline in household income. This is made more clear in Figure 2, Panel

[14] 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 have negative home equity if they tried to sell their home. We would therefore expect some decline in income even for households that have some small amount of positive equity.

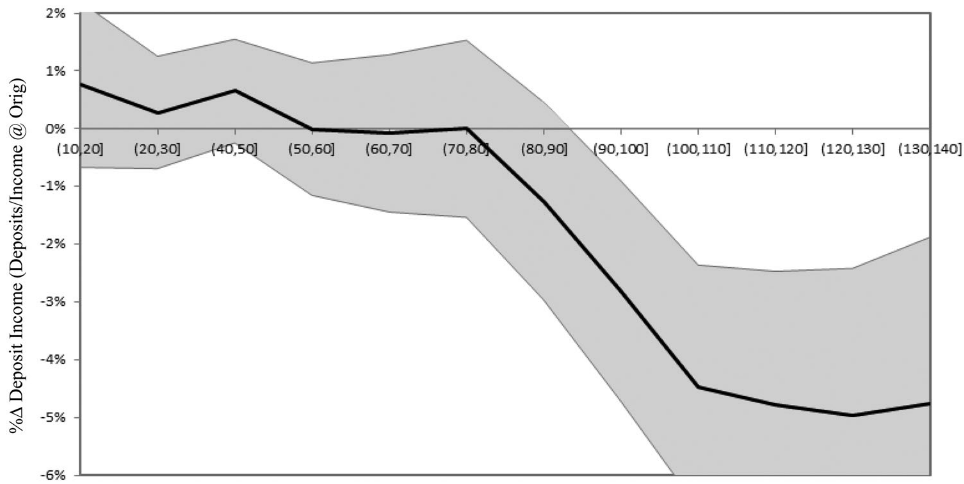

Figure 1. LTV versus income: variation primarily from timing of purchase. This figure depicts the relationship between income and current household mortgage loan to property value (LTV) after controlling for household-specific factors and local demand shocks. This figure shows the coefficients of a regression in which I regress the percent 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 dummies for various ranges of current (LTV) ratios, where house price is computed using original property value and changes in Lender Processing Service (LPS) MSA-level house price indices used by *MyBank* internally, and MSA × time, origination date × time, and household fixed effects. In this figure, the x-axis corresponds to indicator dummies for each household-month that appears in a given 10% LTV bucket and the y-axis are the coefficients from the regression (bold line). LTVs of 30% to 40% are the omitted group for comparison. 95% confidence intervals based on standard errors clustered at the MSA level are shown in the shaded regions. *p*-Values: *10%, ** 5%, and ***1%.

B, which focuses on households with positive home equity. Again, for large and even negative changes in home equity, I find no evidence of a reduction in income. In an additional and related placebo test in Table III, column (6), I find that among households with positive home equity, those with higher SLTV are not associated with a greater sensitivity of income to changes in local unemployment rates. The results provide further evidence that the estimated effect sizes are not contaminated by reverse causality of job losses or local conditions to housing. Taken together, the results above provide compelling evidence that negative SLTV leads to declines in household income, through the increased probability of negative home equity and a causal link from negative home equity to reduced household labor supply.

B. Negative Home Equity and Household Labor Supply

In Section III.A, I provide evidence that strongly supports the validity of negative SLTV as an instrument for negative home equity. Using that instrument,

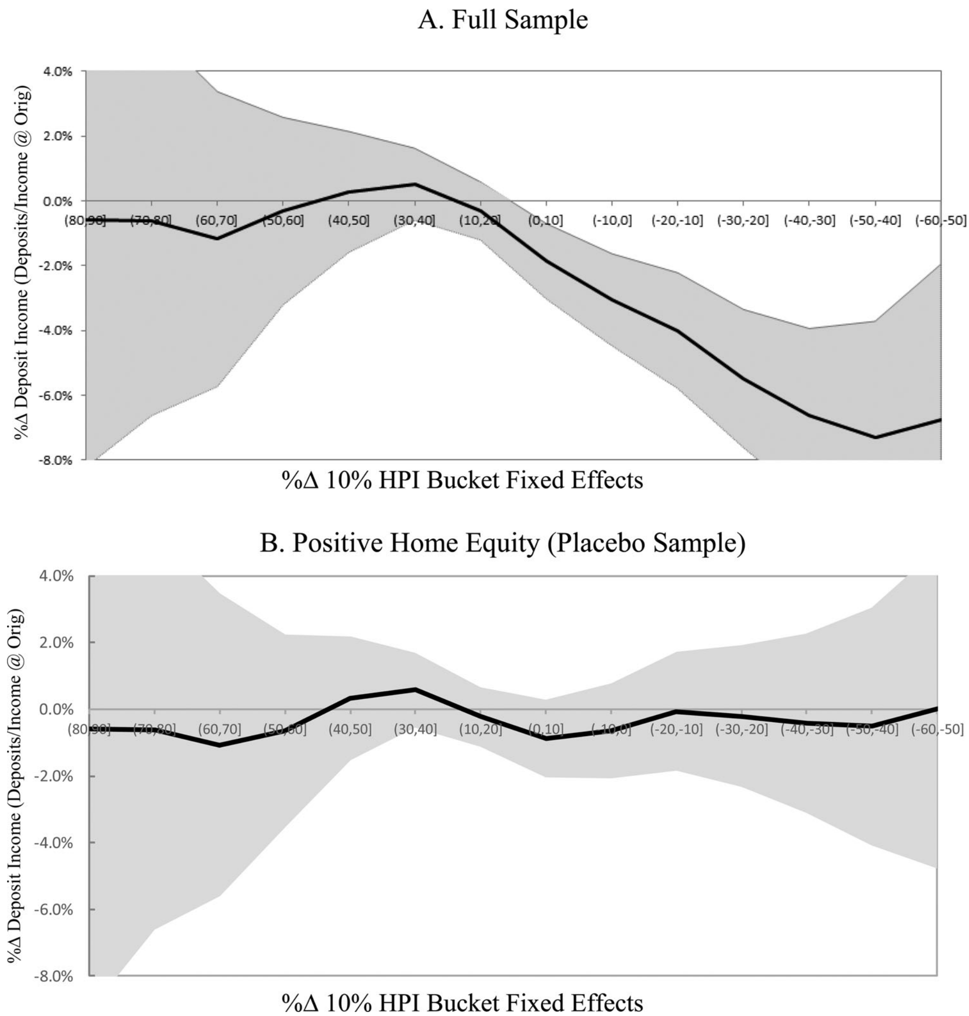

Figure 2. LTV versus income: identification-based HPI IV reduced form. This figure depicts the average change in household income associated with negative household home equity using variation in the timing of home purchase as an instrument for the probability of having negative equity. Panel A shows the coefficients of a regression in which I regress the percent change in deposits, where the numerator is monthly deposit inflows and the denominator is the household's income at the time of mortgage origination, on dummies for various ranges of MSA-level house price index changes since mortgage origination, where the house price is computed using the original property value and changes in LPS MSA-level house price indices used by *MyBank* internally and MSA $\times$ time, origination date $\times$ time, and household fixed effects. In this figure, the x-axis corresponds to indicator dummies for each household-month that appears in a given 10% HPI change bucket and the y-axis are the coefficients from the regression (bold line). HPI changes of 20% to 30% are the omitted group for comparison. 95% confidence intervals based on standard errors clustered at the MSA level, are shown in the shaded regions. Panel B is the same as Panel A, but only includes households with positive home equity as a placebo test. *p*-Values: *10%, **5%, and ***1%.

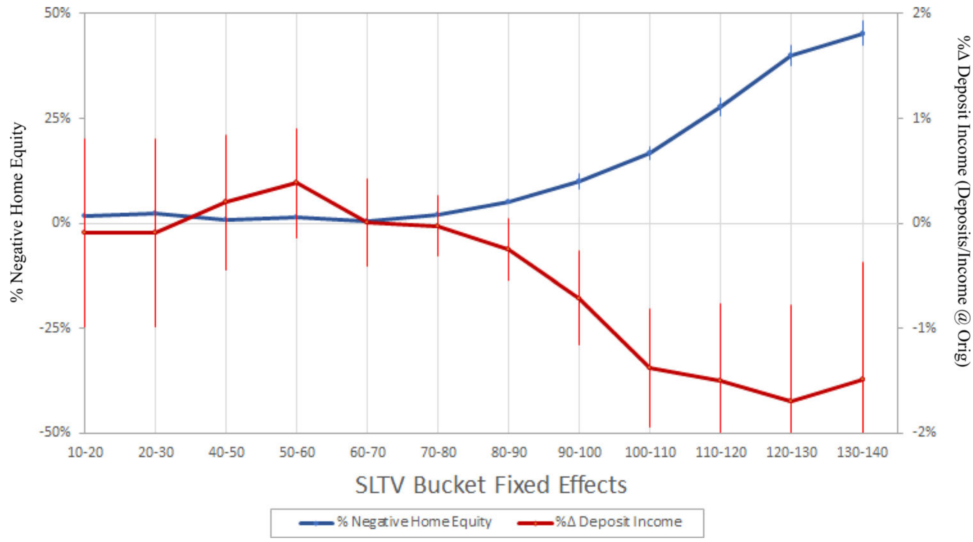

Figure 3. SLTV, negative home equity, and income. This figure shows the relationship between both income and negative home equity and current household mortgage "synthetic" loan-to-value (LTV) after controlling for household-specific factors and local demand shocks. This figure shows the coefficients of a regression in which I regress the percent change in deposits, where the numerator is monthly deposit inflows and the denominator is the household's income at the time of mortgage origination, or a dummy for negative home equity, on dummies for various ranges of current SLTV ratios, where the house price is computed using the original property value and changes in LPS MSA-level house price indices used by *MyBank* internally and MSA $\times$ time, origination date $\times$ time, and household fixed effects. In this figure, the x-axis corresponds to dummies for each household-month that appears in a given 10% SLTV bucket and the y-axis are the coefficients from the regression (bold line). 95% confidence intervals computing standard errors clustered at the MSA level are shown in the shaded regions. *p*-Values: *10%, **5%, and ***1%. (Color figure can be viewed at wileyonlinelibrary.com)

in Table IV, columns (1) and (2), I show that instrumented negative home equity is associated with a $298 per month or 3.47% decline in household labor supply.

Although the previous section provides consistent support for a causal interpretation of this estimate, it focuses primarily on the lack of observable differences in the borrowers correlated with negative SLTV. By contrast, in the remainder of Table IV, I increase confidence in the causal interpretation by showing that the 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 $\times$ time fixed effects instead of MSA $\times$ time fixed effects, and again I find similar declines in labor supply, suggesting that selection within-MSA is not driving the observed results. In column (4), I also include a large range of nonparametric household-specific

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 1 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) reports results of running the 2SLS regression using 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 percent 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 column (2), but includes time-varying nonparametric household-level controls. The controls include deciles for origination income and property value, original mortgage 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 column (2), but includes MSA × time × origination year fixed effects. Column (6) is the same as column (2), but instead of origination date × time fixed effects it includes graduation year × MSA × 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%, and ***1%

			Zip FE	X_{it} Controls	Orig Yr × 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 × Time FE	Y/MSA	Y/MSA	Y/ZIP	Y/MSA	N/A	Y/MSA
Orig. Date × 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 × Time × Orig Yr FE	N	N	N	N	Y	N
MSA × Time × Grad Yr FE	N	N	N	N	N	Y
F-stat	2,440.4	2,440.4	2,109.6	2,304.8	110.25	126.1
Adjusted R^2	0.380	0.490	0.529	0.492	0.620	0.550
Observations (millions)	5.375	5.375	5.271	5.219	5.219	0.665

time-varying controls that might be expected to be correlated with labor demand sensitivity. These include deciles for origination income and property value, original mortgage interest rate by percentage buckets, and original credit score in bins of 50 all interacted with time fixed effects. The results show a 4.9% decline in household income that is again consistent with the overall findings and suggest that the results are not driven by nonlinear selection at origination.

In Table IA.III in the Internet Appendix, I rerun all of these analyses after excluding household fixed effects. The results are similar to those with household fixed effects, further showing that omitted time-invariant covariates are unlikely to be confounding causal interpretation of my findings. However, this may come as a surprise when looking at the reduced form estimates for SLTV $>$ 100% without household fixed effects in Table II and with them in Table III. The reduced-form estimates are substantially larger in Table II, which raises the question as to why the 2SLS estimates are so similar. The answer becomes more clear when examining Figure 3. As I describe in more detail above, Figure 3 shows the nonlinear first stage and reduced form as a function of SLTV. As can be seen, there is no change in income for large changes in SLTV if SLTV is relatively low. Only when SLTV approaches 100% does income begin to fall. As SLTV continues to rise after that point, income continues to fall but is matched by the increase in the probability of having negative home equity in the first stage. In fact, if one multiplies the first-stage and reduced form estimates, a relatively smooth effect obtains as SLTV rises in the effect of instrumented negative home equity on income. The reason is that an increased probability of having negative home equity tends to vary closely with the amount of negative home equity. The intensive and extensive margins are very hard to disentangle, not just for the econometrician, but also for borrowers and lenders. Prior to selling a property all parties only have some noisy measure of the true value of a property. Therefore, as the household becomes more underwater by my estimates, they are more likely to actually be underwater if a transaction occurs and at the same time are more likely to have a larger amount of negative home equity if they do have negative home equity. It is worth noting that these results represent the combined effects of an increased probability of having negative home equity and increases in the amount of negative home equity conditional on being underwater. This is important since most predictions from the channels discussed in Section I suggest that the latter, rather than the former, may be the driving force behind the observed increases in the magnitude of household income effects as SLTV rises.

Unfortunately, the data do not indicate when workers enter the labor force. It is possible that those who buy earlier or later in specific markets also entered the labor market in systematically different ways and may be more likely to be laid off. To partially mitigate this concern, in column (5) of Table IV, I show that instrumented negative home equity is associated with a 2.2% decline in labor supply, even after controlling for the origination year interacted with the MSA and time. Identification is therefore, 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 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. Although I do not see the year an individual enters 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.[15] In column (6), I use this same sample of households with information on approximate college graduation date, but now include fixed effects for MSA × time × college graduation year. This specification allows me to control flexibly for the amount 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 control, 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 the analysis up to this point is that I measure deposits at only one institution and, in particular, I use deposits from the same institution as the household's mortgage lender. If households hide or shift deposits away from their lender when they have negative equity, the reduction in deposits among households with negative equity my simply represent a movement of deposits to another institution rather than an actual decline in overall deposits from income. With this concern in mind, throughout the analysis I use multiple restrictions to ensure that households in the panel have active retail accounts, taking advantage of the inflow and level information that I have for all deposit accounts at *MyBank*, and results are robust to all choices of filters and measures of income. Specifically, in column (2) of Table IA.V in the Internet Appendix, I show that the results are robust to including only direct deposits instead of all deposits as the measure of income.[16]

To address this concern more directly, in Table V, column (1), I show that the results are unchanged after excluding cases in which households deposit $0 into their accounts.

This finding suggests that the results are not driven by households systematically leaving the bank. This is despite the fact that Table V, column (2), shows that the results are driven entirely by declines of greater than 25% in household income. Taken together, the evidence suggests that households make large extensive-margin changes in labor income, for example, by increasing search duration. Moreover, the evidence suggest that the results are not likely to be driven by households systematically moving deposits into retirement savings accounts prior to depositing. Shifts in this kind of long-run savings behavior would lead to small pervasive changes in deposit inflows rather than large concentrated reductions of the magnitude observed. This view finds further

[15] 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 provides credibility as the average origination date of student loans is highly correlated with the timing of college graduation.

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

Table V

Robust to Income "Hiding"

This table explores the drivers of the negative effect of mortgage LTV on labor supply and shows that it is not driven by "hiding" deposits with other institutions. Just as in the main specifications, column (1) regresses the percent 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 1 if current mortgage LTV is greater than 100%, and MSA × time, origination date × time, and household fixed effects. A dummy equal to 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 LTV that does not depend on household-specific factors except the time of a move and that varies at the region-time-cohort level. In this case, however, observations with a 100% decline in deposits are excluded from the analysis. Column (2) is the same as column (1) but excludes 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 social security checks, which are defined as direct deposits received on the third of the month, or the second, third, or fourth 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 do not rely on mortgage data, so I know the approximate timing and zip code of origination (see the text for more details), but not the actual LTV or income at origination. I therefore regress the dollar amount of deposits per month on the percentage of mortgages in the estimated zip code × origination year × time with negative home equity, after including MSA × time, origination date × time, and household fixed effects. Column (5) is the same as column (4), but restricts the analysis to households with mortgages not serviced or owned by *MyBank*. All standard errors are clustered at the MSA level. p-Values: *10%, **5%, and ***1%

	(1) %ΔDep	(2) %ΔDep	(3) %GetSS	(4) \$Dep	(5) \$Dep
LTV > 1	-3.35^{***}	-0.07	0.91^{***}		
(IV: SLTV > 1)	(1.12)	(0.55)	(0.32)		
%NegEq				-48.8^{***}	-65.0^{***}
(Region × Cohort × Time)				(10.4)	(15.0)
Region × Time FE	Y	Y	Y	Y	Y
HH FE	Y	Y	Y	Y	Y
Orig. Date × 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

support from the results in Table V, column (3). Despite the overall reduction in deposit income shown previously, in column (3), I find that households are actually more likely to receive social security or disability checks. This finding again suggests that households are more likely to either 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 suggests that there is not a systematic shift of deposits away from *MyBank* in response to the instrumented negative home equity.

Notwithstanding, in robustness tests I rerun my analysis focusing on *MyBank* retail customers with a mortgage from another lender. Since I do not rely on detailed mortgage information in these tests, I use the zip code that households enter in their retail accounts as a proxy for the property's MSA and information from the credit bureau data on mortgage origination dates.[17] I then regress the dollar amount of deposits per month on the percentage of mortgages in that estimated zip code × origination year × time with negative home equity, after including MSA × time, origination date × time, and household fixed effects. Note that these regressions are reduced-form regressions since current LTV is not available in credit bureau data to run the first stage. This approach is likely to reduce the power of the regression, but the reduced-form regression reported 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 region × time, cohort × time, and household fixed effects. This result holds in column (5) when analyzing households with mortgages at any lender or for the households for which *MyBank* does not service or own 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, the above results suggest that hiding income is unlikely to explain the reduction in monthly deposit inflows seen for households with negative equity.

In subsequent work, Gopalan et al. (2020), build on the analysis carried out in my paper by replicating the identification strategy developed in this paper using an entirely new data set based on verified employer income records, rather than deposits at *MyBank*. They also find a negative relationship between instrumented negative home equity and labor income, which provide additional confidence that results are not driven by income hiding at *MyBank* and support the external validity of the findings in this paper. I am unable to observe detailed employer information in my sample, but in their replication and extension Gopalan et al. (2020) can, which allows them to run additional robustness tests. They show that a matched set of renters living in the same zip code and working with the same job title at the same employer as a homeowner and assigned the same LTV as that homeowner observes no reduction in income for lower home equity. This provides another strong placebo test consistent with the causal interpretation presented in my paper.

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

Although a complete assessment of the macroeconomic implications of this labor supply response is beyond the scope of the current paper, it is worth highlighting just how many households were likely to have been affected in the recovery following the Great Recession. In Panel A of Figure IA.4 in the Internet Appendix, I plot separate estimates of the percentage of residential properties with near-zero or negative home equity by quarter over the sample period based on data from Zillow and CoreLogic. For both sources I find similar overall levels and trends, with the percentage of all households having negative home equity reaching a high of 30.7% in 2010, before falling considerably as markets recovered. Panel B further shows, that according to Federal Reserve Economic Data (FRED), the rate of homeownership as measured by owner occupancy per household was 67.1% at the beginning of 2010 before falling thereafter. Taken together, these results suggest 20% of U.S. households were directly affected by negative home equity in the first quarter of 2010, with this figure falling to as low as 8.4% by the end of 2014. If one-fifth of households experienced a reduction in labor income of 2.2% to 5.6%, this would certainly suggest the potential for substantial macroeconomic effects. According to CoreLogic, in the first quarter of 2010 negative home equity totaled over $820 trillion, making it the single largest category of unsecured household liability.[18]

The importance of understanding negative home equity is made even more clear when examining regional heterogeneity in treatment. In Panel C of Figure IA.4 in the Internet Appendix, I plot the proportion of properties with negative home equity for MSAs two standard deviations above ("high") and below ("low") the national MSA-level average. Although in the second quarter of 2011 about 22% of households in the median MSA had negative home equity, for "low" MSAs this was only 4% but for "high" MSAs, it was around 60%. For example, according to Zillow's estimates, in areas of Las Vegas as many as three-quarters of all households had negative home equity. Holding local economic conditions and selection constant, this would suggest that a household in a "high" underwater MSA was 56 percentage points more likely than an equivalent household in a "low" underwater MSA to have negative home equity in early 2010. The estimated effect of negative home equity on labor income from Table IV, column (1), is about $298 per month of treatment. Based on this estimated labor income treatment effect and assuming an income-to-consumption ratio of 0.85 as in Hurst et al. (2016), the expected loss in consumption for households in "high" underwater MSAs relative to households in "low" underwater MSAs is $137 per month, or $1,641 on an annualized basis. The annual effects in 2010 are comparable in size to the $1,860 per household cross-region transfers caused by a constant cross-region interest rate mortgage policies as documented in Hurst et al. (2016) and to the tax rebate checks authorized by the U.S. Congress during the 2001 and 2008 recessions, which tended to range from $500 to $1,000 per household. I

[18] Based on data from the New York Federal Reserve, as of the first quarter of 2010 the outstanding principal balance of student loans, credit cards, autoloans, and other liabilities was approximately $0.76, $0.76, $0.7, and $0.36 trillion, respectively.

caution, however, that while useful to gain a general understanding of the potential size of the microeconomic effects, the simple exercise above brings with it a myriad of important caveats. Moreover, this exercise ignores other effects of home equity, despite evidence that it is likely to affect consumption and in turn local labor demand (Mian, Rao, and Sufi (2013)). More broadly speaking, this exercise ignores all general equilibrium effects of negative home equity both overall and within the context of the estimated effects on labor income. For "high" underwater MSA areas, with such a substantial portion of the local labor market affected, it certainly seems reasonable to expect that these labor market distortions may have been more than just a microeconomic consideration. Quantifying the exact general equilibrium effects of such distortions outside the scope of this paper, however, since regardless of the channel any local general equilibrium effects are likely affected by responses of wages, inmigration, and firm competitiveness as illustrated in Donaldson, Piacentino, and Thakor (2019). In addition, as has been noted by Chetty, Olsen, and Pistaferri (2011) and Chetty (2012), macroeconomic estimated labor supply elasticities tend to exceed microeconomic estimates and typically cannot be easily recovered without the benefit of an underlying structural model.

IV. Discussion: Potential Mechanisms

Overall, the above results are consistent with negative home equity causing an average partial equilibrium labor income decline of 2.2% to 5.6% that is not driven by changes in labor demand. Although an exact comprehensive decomposition of the underlying channels is beyond the scope of this paper, though likely to be an important area of inquiry for future researchers, nonlinearities in the treatment effects would be consistent with housing lock and/or financial distress affecting household labor supply.

A. Housing Lock

Households that are financially constrained and have negative equity may be prevented from moving, a phenomenon known as housing lock. To extent that reduced mobility reduces labor market opportunities, this could lead to a reduction in income via longer periods of unemployment, worse labor market matches, or monopsony power on the part of employers aware of an employee's limited search ability.[19] Due to the relatively weak recourse nature of mortgages in the United States, the effect of housing lock on mobility is unclear. Empirical evidence has been mixed, with papers such as Schulhofer-Wohl (2012) and Mumford and Schultz (2014) finding no evidence of reduced mobility. In

[19] Technically, by far the most reasonable mechanism through which an employer could know that an employee has weaker wage bargaining due to negative home equity is through reduced outside offers or search, which would constitute a change in labor supply. In this setting, just as in Brown and Matsa (2020), negative home equity could reduce search, even among employed individuals, which leads to slower wage increases in bargaining even with their existing employer.

addition, Modestino and Dennett (2013) point out that while the nonpecuniary costs of immobility could be large, relatively few households have to move for employment in a given year, so the effect on aggregate labor supply may be limited.

By contrast, more recent research that employs the empirical design developed in this paper find significant reductions in moving rates among households with negative home equity in the Netherlands and the United States (Bernstein and Struyven (2019), Gopalan et al. (2020)). In addition, Brown and Matsa (2020) provide evidence that job seekers appear to engage in more geographically constrained search in more depressed housing markets, which could be consistent with mobility constraints due to housing lock affecting labor market search. Though they do not focus on mortgage liabilities, Di Maggio, Kalda, and Yao (2019) find evidence that the discharge of student loans improves borrower mobility and labor income, which provides further support to the view that debt plays a role in constraining employment search.

In the case of mortgage liabilities, these sorts of constraints are unlikely to bind when a household has positive home equity, since in theory the home could be sold without needing substantial additional resources. In contrast, negative home equity requires that the homeowner use financial assets to pay down the liability, default, or rent out the property to facilitate a move, which is often not feasible. These patterns imply nonlinear effects of home equity on mobility that start to increase near negative home equity, which is exactly what Bernstein and Struyven (2019) find, and an analogous prediction for labor income distortions. It should be noted, however, that since in the U.S. setting strategic default becomes more likely as negative home equity increases, it is possible that effects on mobility and income could asymptote as default incentives rise. Lenders in these settings may also have incentives to encourage short sales, which would force movement on the part of homeowners, potentially disrupting employment.

B. Financial Distress

Households with reduced wealth, and in particular low home equity, are much more likely to experience financial distress when facing an income or liquidity shock (Foote, Gerardi, and Willen (2008), Fuster and Willen (2017), Gerardi et al. (2018)). For households with low but positive home equity that experience an income or liquidity shock, they may need to access their home equity to avoid default. Indeed, even if market frictions preclude their ability to access that wealth for liquidity, households have a strong incentive to avoid default because in that case the lender would seize the house and they would lose all of their positive housing wealth. In the case of negative home equity, households appear to engage in "strategic" default, which may be a value-maximizing decision (Foote, Gerardi, and Willen (2008), Adelino, Schoar, and Severino (2013), Mayer et al. (2014)), but often is associated with costs in the form of energy, time, and stress, which could reduce worker productivity among the employed or job search among the unemployed (Deaton (2012), Currie and

Tekin (2015), Dobbie and Song (2015), Bos, Breza, and Liberman (2018), Engelberg and Parsons (2016)).[20] Di Maggio, Kalda, and Yao (2019) find substantial labor income effects of the discharge of student loans, but the particular set of borrowers examined were already not paying these loans. Although there could be any number of reasons borrowers who are not paying would respond to debt relief, when asked for an interpretation of this result Ben Miller, senior director for post-secondary education at the Center for American Progress, said he thought "it suggests there might be some sort of psychological benefit to this relief that goes beyond the household balance sheet."[21] This interpterion could be consistent with the effects of stress associated with financial distress. It is also plausible that such mechanisms could be important in the context of mortgage liabilities and home equity decisions more generally. Guiso, Sapienza, and Zingales (2013) and Bhutta, Dokko, and Shan (2017) provide evidence that the decision to default among underwater households is often one driven more by emotional and behavior factors than by specific costs or benefits, such as lender recourse. To the extent that these forms of distress have significant effects on labor markets, a nonlinear effect of home equity that is concentrated in households with negative equity would be consistent with this mechanism.

Another way in which financial distress could impact labor markets is through incentives caused by household protection under limited liability, which I refer to as the "household debt overhang" channel. As is well-known, 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)). As shown in theoretical work by Donaldson, Piacentino, and Thakor (2019), highly levered households face a similar debt overhang 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 expected liability repayment, this transfer to debt holders acts like an implicit tax that incentivizes households to reduce their labor supply. This does not mean that households purposefully leave existing employment, but could suggest, as in Donaldson, Piacentino, and Thakor (2019) that unemployed workers could be incentivized to prolong job search in an effort to find a better match. Empirically, Dobbie and Song (2015) find evidence consistent with households' labor decreases responding to such incentives. Using random assignment to judges, they show that bankruptcy protection leads to an increase in labor supply. The authors' proposed mechanism is that households with limited liability do not always expect to fully repay outstanding liabilities, and bankruptcy protection reduces the likelihood of a larger implicit tax from wage garnishment outside the bankruptcy system. This mechanism is functionally

[20] It could also be the case that direct effects on credit scores from distress could negatively impact labor market outcomes, but Dobbie et al. (2020) show that the removal of bankruptcy flags and subsequent increases in credit scores and access to credit do not substantially affect labor income.

[21] See https://finance.yahoo.com/news/student-debt-loans-cancel-forgive-142422120.html.

the same underlying premise as the debt overhang mechanism proposed in this paper and modeled in Donaldson, Piacentino, and Thakor (2019). In the context of home equity, Donaldson, Piacentino, and Thakor (2019) show that for sufficiently high house value, relative to the outstanding mortgage, there are no distortionary effects because a household can use their collateral to fully repay all liabilities. When collateral value is low, however, such as in cases of low or negative home equity, limited liability causes labor supply distortions, just as in the case of unsecured liabilities. Therefore, in their framework debt overhang would predict a nonlinear relationship between housing wealth and labor income, with no relationship between housing wealth and labor supply until home equity is sufficiently low.[22]

C. Wealth Effects

There is broad prior evidence that increases in nonhousing wealth can lead to reductions in labor supply (Joulfaian and Wilhelm (1994), Imbens, Rubin, and Sacerdote (2001), Cesarini et al. (2017)). This evidence is consistent with households establishing a reference level of wealth or liquidity that supports smooth consumption growth and working only at the level required to maintain it (Rizzo and Zeckhauser (2003)). By contrast, Bernstein, McQuade, and Townsend (2021) show that declines in housing wealth are associated with lower innovation among workers, consistent with reduced productivity. One channel for this response put forth by the authors is that declines in housing wealth could lead to reductions in consumption (Mian, Rao, and Sufi (2013)) and in particular to decreased spending on labor-augmenting goods and services (Becker (1965), Baxter and Jermann (1999), Aguiar, Hurst, and Karabarbounis (2013)). For example, if innovative workers with higher levels of wealth are more likely to pay for home services that free up additional time they can engage in work or even just think about inventions, that may be more likely to increase their productivity. This channel may also be consistent with the findings in He and Maire (2018) that the relaxation of liquidity constraints from extracting home equity have labor market consequences in Denmark. To the extent that labor-augmenting consumption is the prevailing channel, it is not clear ex ante that these effects should be asymmetric or nonlinear. However, Bernstein, McQuade, and Townsend (2021) demonstrate a nonlinear response of worker innovation to housing wealth shocks whereby increases in housing wealth have no effect while large declines, especially among those more likely to have low or negative home equity, drive the observed response. Housing wealth is the primary form of savings for many households (Campbell (2006)) and the marginal propensity to consume out of housing wealth has

[22] It is not clear, however, these ex ante costs of financial distress are of first-order importance in practice. For mortgage debt, unlike the setting of Dobbie and Song (2015), lenders either legally cannot or in practice do not obtain deficiency judgements and garnish wages. Although income-contingent renegotiations, such as mortgage modifications, could provide another observable channel, it is not obvious that households are aware of, and willing to alter their behavior in response to, such incentives.

sharp nonlinearities around negative home equity (Baker (2018), Ganong and Noel (2020)), so this result could be driven by nonlinearities in the response of consumption to housing wealth. That being said, in my setting the finding of no response of household income to large increases in housing wealth feels at least somewhat at odds with this interpretation and suggests that perhaps other mechanisms, many of which do contain starker nonlinearities in home equity, may provide more likely explanations.

D. Collateral Channel and Entrepreneurship

Another channel through which home equity could affect labor market decisions are entrepreneurial endeavors. Adelino, Schoar, and Severino (2015) and Schmalz, Sraer, and Thesmar (2017) show that due to information asymmetries between banks and entrepreneurs, collateral value from positive home equity is a critical driver of the entry and success of entrepreneurs and small business development. To the extent that entrepreneurship is an income-maximizing labor opportunity, a reduction in home equity would then be expected to reduce labor income. Although just like with many of the other channels discussed above this relationship would be expected to be nonlinear, the predicted pattern of the nonlinearity is likely to be quite different in this setting. For households with positive home equity we would expect a reduction in home equity to reduce collateral value and in doing so impact entrepreneurial income, but for households with negative home equity we would expect little to no effect for changes in home equity since their collateral already has no value for lenders to use. However, entrepreneurship may bring with it non-pecuniary benefits that act as a compensating differential in equilibrium. In that case it may be that even though home equity matters as collateral for entrepreneurship, it is not clear that it will necessary be an important driver of total household income.

V. Conclusion

In this paper, I provide the first empirical evidence of a causal effect of negative home equity on household labor supply. To do so, I use a new comprehensive data set with information on household-level liabilities, assets, and all deposit transactions for all customers of a major U.S. financial institution from 2010 to 2014 to exploit 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 origination cohort trends. I find that instrumented negative home equity leads to an average reduction in household labor income of 2.2% to 5.6%, even when comparing households that bought their homes in the same region and year. These results shed new light on the role that house price declines played in exacerbating employment declines following the crisis. Mian and Sufi (2014) examine how house price shocks affect equilibrium employment via local labor demand, but this paper is the first to demonstrate the role that house price declines play 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 suggest that it has a role to play in understanding how household balance sheets can exacerbate financial crises.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.