Household Liquidity Constraints and Labor Market Outcomes: Evidence from a Danish Mortgage Reform[*]

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Abstract

This paper studies the effects of household liquidity constraints on individual labor market outcomes by exploiting a mortgage reform in Denmark in 1992, which for the first time allowed homeowners to borrow against housing equity for non-housing purposes. We find that following the reform, liquidity-constrained homeowners extracted housing equity, increased debt levels, and had higher earnings growth and lower employment rates. In contrast, the reform had small and opposite effects on the earnings and employment rates of homeowners with high liquid asset holdings. Consistent with models of job search with risk aversion, the option to borrow against housing equity allows liquidity-constrained individuals to search for high-wage jobs. The results imply that relaxing household liquidity constraints during recessions could potentially increase earnings and output in the longer run through labor market search.

JEL codes: G21; E44; E24; J60; D14; R20

1   Introduction

A significant fraction of households are severely liquidity constrained. In the United States, for example, approximately a quarter of households are unable to come up with $2,000 to cope with an unexpected need (Lusardi, Schneiderm, and Tufano 2011).[1] This makes them very fragile to unexpected income shocks. The view that liquidity constraints are particularly severe during a recession has important implications for the design of stabilization policies (Eberly and Krishnamurthy 2014).[2] While the impact of liquidity constraints on consumption is well known (Gross and Souleles 2002; Agarwal, Liu and Souleles 2007; Leth-Petersen 2010), much less is known about how liquidity constraints affect labor supply and earnings. In this paper, we exploit a unique mortgage reform in Denmark to provide causal estimates of the effects of liquidity constraints on workers' labor market outcomes.

Estimating the effects of liquidity constraints on labor market outcomes is challenging because assets and earnings are both endogenously determined. Additionally, studies using exogenous variations often have a modest impact on the amount of credit access, or have confounding effects that make it hard to isolate the effect of liquidity constraints. For example, credit reports also affect the credit checks and therefore employment opportunities (Herkenhoff, Phillips and Cohen-Cole 2018). Debt relief programs and changes in housing prices affect both short-run liquidity constraints and long-run debt overhang. Therefore, some studies rely on structural models to quantify the effects of liquidity constraints on labor market outcomes (Kaplan 2012; Ji 2020).

In this paper, we overcome these challenges using the Danish mortgage reform in 1992 as a natural experiment. The reform allowed homeowners in Denmark, for the first time, to borrow against their housing equity for purposes other than financing the underlying property. The resulting increase in available home equity was large, equivalent to over one year's disposable income for the median treated individual in our sample. Since the notion of home equity finance

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[1]An additional 19% of households could only come up with $2,000 by pawning or selling possessions or taking out a payday loan (Lusardi, Schneider, and Tufano 2011).

[2]For example, there are debates about whether policies that replenish the liquid balances of households, such as reductions in mortgage payments that are concentrated in the periods of crises, would be more effective than debt write-downs that reduce mortgage payments over the entire duration of the mortgage contract (Ganong and Noel 2020; Dobbie and Song 2020). It is also argued that policies that prevent households from refinancing their debt during times of economic distress can significantly inhibit efforts aimed at curtailing the costs of recessions (DeFusco and Mondragon 2020).

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did not exist in Denmark prior to this reform and the reform itself was passed within three months, the reform was unexpected for individuals and therefore unrelated to house purchase decisions before 1992. We document that differences in the timing of individuals' home purchase relative to the reform led to systematic cross-sectional variation in the intensity of the reform's treatment across homeowners, even after controlling for detailed life-cycle and demographic characteristics. That is, homeowners who bought their homes shortly before 1992 had paid down less of their mortgage and hence had less home equity available to borrow against compared to homeowners who bought their homes well before the reform. We then combine the household balance sheets data with detailed matched employer-employee datasets to study the impact of the expanded credit access on workers' employment and earnings.

We find that the reform led to more housing equity extraction and higher debt levels for individuals with more housing equity, who also experienced faster wage and earnings growth after 1992. Individuals with equity to value ratio (ETV) higher than 0.25 in 1991 experienced an increase in debt of 13% of annual income and a 1.0% increase in wages after the reform compared to individuals with ETV lower than 0.25 in 1991.[3]

To isolate the reform's effects on liquidity constraints, we compare the effects on individuals with liquid assets[4] less than one month's disposable income in 1991, and individuals with higher levels of liquid assets in 1991. While liquidity-constrained individuals with high ETVs experienced an increase in debt levels by 16% of annual income and an increase in wages by 2.1% following the reform, non-liquidity-constrained individuals with high ETVs experienced an increase in debt levels by 10% of annual income and a decrease in wages by 0.2%. Furthermore, among individuals affected by the reform, the employment rate of liquidity-constrained individuals decreased after the reform, while the employment rate of non-liquidity-constrained increased slightly after the reform. The positive effect on earnings is biggest for younger workers and workers without vocational training.

Our identification relies on the assumption that individuals with more housing equity and individuals with less housing equity would have followed parallel wage trends absent the reform

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[3]0.25 is the median of ETV in 1991. Since the maximum loan-to-value ratio allowed is 80%, only individuals with ETVs higher than 0.2 can extract housing equity after the reform. Comparing individuals with ETVs above 0.2 vs individuals with ETVs below 0.2 yields similar results.

[4]Liquid assets are non-housing assets like bank deposits, cash, stocks and bonds.

conditional on observed pre-reform characteristics including demographics, total wealth, occupation and location. In other words, we assume that conditional on observables, the timing of the housing purchase is uncorrelated with changes in employment and wages after 1992. To confirm that the variation in ETVs is driven by the timing of house purchase, we also estimate an IV difference-in-differences where we instrument for ETV in 1991 using the number of years between the house purchase date and the reform. The instrument has a strong first stage and similar second-stage estimates as the OLS regressions.

We conduct several robustness tests of our identification assumption. First, we show that individuals with more housing equity and less housing equity had similar wage trends before 1991, both for liquidity-constrained and non-liquidity constrained groups. Second, we show that our results are robust to inclusion of industry-specific and income-level-specific trends as well as a linear pretrend. Third, we conduct a placebo test using the period prior to the reform, and show that individuals with more housing equity in 1985 and individuals with less housing equity in 1985 had similar wage and employment growth during the period 1986-1990 when controlling for the observed characteristics in 1985.

Our findings that relaxing liquidity constraints leads to higher wages and lower employment rate are consistent with models of job search with risk-averse workers. First, for unemployed workers, providing liquidity raises the reservation wages, and therefore workers stay in unemployment for longer and wait for better job offers. We show that for workers who are unemployed in 1991, having access to housing equity increases unemployment durations, reduces reemployment hazard rates, and increases reemployment wage. Second, for employed workers, having the extra liquidity buffer through home equity loans helps workers smooth negative income shocks and encourages workers to take more risks and search for high-wage jobs. We find that workers who recently become unemployed and experience negative income shocks are more likely to borrow against housing equity, suggesting that the extra credit from housing equity indeed allows workers to insure against negative labor market shocks. After the reform allows home equity loans, households with more housing equity are more likely to switch jobs and move to high-wage firms.

We consider several alternative explanations for our findings. First, although access to housing equity leads to higher rates of entrepreneurship, the effect is small compared to the wage gains, and

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excluding self-employed workers yields similar results. Second, we show that financial distress and housing lock are unlikely to explain our results, as we find similar results after excluding workers who had negative housing equity at any time during the period. Finally, we do not find a positive effect of access to housing equity on within-job-spell wage changes except for a small positive effect for college-educated workers, suggesting that productivity changes due to higher consumption and lower anxiety is not the primary driver of our baseline results.

Several other papers have studied how home equity loans affect labor market outcomes. Two papers look at the same 1992 reform in Denmark: Jensen, Leth-Petersen and Nanda (2015) find that access to housing equity increases entrepreneurship; and Markwardt, Martinello and Sándor (2014) find that the home equity loans partially substitute for unemployment benefits. Kumar and Liang (2018) study a similar reform in Texas in the 1990s and find that access to housing credit led to a lower labor force participation rate. Our paper is the first one to study how the ability to borrow against housing equity affects workers' employment and earnings and use individual-level data to link labor market shocks to equity extractions.

Our paper contributes to a growing body of work studying the relationship between household debt and liquidity and labor market outcomes. A line of research explores the relationship between housing wealth and labor supply (e.g., Bernstein 2019; Mulligan 2009; Donaldson, Piacentino, and Thakor 2019; Fontaine, Jensen, and Vejlin 2020) and productivity (Bernstein, McQuade, and Townsend 2019). While these papers focus on changes in housing wealth, our paper focuses on the additional liquidity from borrowing against housing equity. Our paper is closest to Herkenhoff, Phillips and Cohen-Cole (2019), who show that more access to revolving unsecured debt (e.g. credit cards) leads to longer unemployment durations and higher reemployment wages among the unemployed workers. We find similar results for the unemployed workers. In addition, we also find a positive effect on wages for the employed workers. We provide the first causal estimates of liquidity constraints on earnings of all workers using an unexpected and large liquidity shock – the option to borrow against housing equity provided an increase in credit access comparable to at least one year of disposable income for more than 50 percent of the households in our sample, which is a much bigger shock to liquidity than unsecured credit.

Since only homeowners can borrow against housing equity, our paper contributes to under-

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standing the impact of liquidity constraints for wealthy "hand-to-mouth" households, which are households with little liquid wealth despite owning illiquid assets. Kaplan, Violante and Weidner (2014) document that over 20% of US households are wealthy hand-to-mouth. Boar, Gorea and Midrigan (2020) estimate that four-fifths of homeowners in the US are liquidity constrained. While recent research tries to disentangle the effect of liquidity from wealth by varying short-term debt payments and long-term debt obligations separately (Ganong and Noel 2020; Dobbie and Song 2020), our setting allows us to isolate the effect of liquidity constraint. Our results suggests that in addition to reducing financial distress and stabilizing employment and consumption (Agarwal et al. 2017; Auclert, Dobbie and Goldsmith-Pinkham 2019), debt relief policies that relax the liquidity constraints of borrowers could also lead to higher earnings.

Finally, our paper is related to the literature on how unemployment benefits and other loans (e.g. payday loans) affect employment and wages. While home equity loans must be repaid or defaulted upon, unemployment benefit is a transfer to households, and therefore also have moral hazard effects in addition to liquidity effects (Chetty 2008). Similar to home equity loans, payday loans also offer insurance against negative shocks (Morse 2011). However, in contrast to our results, payday loans with high interest rates often have high default rates and lead to increased difficulty in paying debts (Melzer 2011; Carrell and Zinman 2014). This is because the interest rates for home equity loans in Denmark are lower than bank loans, and the default rate is very low due to full recourse and a loan-to-value ceiling. The contrast between home equity loans and high-interest payday loans highlights the importance of the contractual form of credit policies intended to alleviate liquidity constraints (Zingales 2015).

The rest of the paper is organized as follows. Section 2 describes the institutional details of the mortgage reform. Section 3 presents a conceptual framework to illustrate how liquidity constraints affect earnings. Section 4 describes the data used and the empirical strategy. Section 5 presents the main results and Section 6 explores the mechanisms. Section 7 concludes.

2   The 1992 Mortgage Reform in Denmark

We study the Danish mortgage reform, which took effect on 21 May 1992. The most important element of this reform is that it enabled homeowners, for the first time, to borrow against their

home for purposes other than financing the underlying property. The May 1992 bill introduced a limit of 60% of the house value for loans for non-housing purposes, but already in December 1992, this limit was further increased to 80%.

Until 2007, mortgage banks specializing in mortgage loans were the exclusive providers of mortgage debt in Denmark. The granting of loans was solely on the basis of the value of housing collateral, which was not true for loans from commercial banks. It was usually the case that the interest payments were lower for loans obtained from mortgage banks compared to commercial banks.[5]

Another aspect of the reform was that the maximum maturity of mortgage loans was prolonged from 20 to 30 years. This option also provided home owners with more liquidity by reducing the monthly installments on the loan while spreading these out over a longer time horizon.

A third element of the reform was a possibility of refinancing mortgage loans. This made it possible for borrowers to lower the cost of the loan when the market interest rate falls. While the other two parts of the reform impacted the access to credit, this part of the reform provided house owners with the option to lock in low market interest rates in order to obtain lower monthly payments on their mortgages and an overall gain in wealth.

The reform was implemented with short notice and passed through parliament in three months. The short period from its introduction to implementation is useful for our empirical strategy since individuals have little time to strategically take advantage of the reform. Appendix Figure 1 plots the unemployment rate and real housing price in Denmark around the reform. The reform was introduced during the 1992 recession when unemployment reached over 10% and implemented was right before the Danish economy and housing price started to grow rapidly, so the lessons from

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[5]In Denmark, a mortgage loan for housing is solely funded through the issuance of bonds sold on the stock exchange. The issued mortgage credit bonds would match the repayment profile and maturity of the loan granted. Thus, mortgage banks could not raise funding by deposits to fund their mortgage lending. Once the bank had screened potential borrowers based on the valuation of their property and their financial positions, all borrowers who were granted a loan at a given point in time faced the same interest rate. This was feasible because of the detailed regulation of the mortgage market. First, mortgage banks were subject to solvency ratio requirements monitored by the Financial Supervision Authority, and there was a legally defined threshold of limiting lending to 80% of the house value when the loan was issued. Second, there was a unique identification number for each plot of land in Denmark, to which all relevant information about owners and collateralized debt were recorded. Third, mortgage loans had a preferential status over any other loan and if debtors could not maintain their loans, creditors could demand a sale of the property. Finally, mortgage banks accumulated a buffer through contributions from all borrowers, which were used as a buffer to cover loan defaults. The combination of the regulation around mortgage lending and protection afforded by the title registration system and the buffer to cover loan defaults implied that the loans offered by mortgage banks were very safe, justifying lending based solely on the value of collateral.

this reform may shed light on other similar policies during recoveries.

In this paper, we focus on the the first two elements of the reform which provided homeowners access to extra credit. The option to borrow against housing equity provided an increase in access to credit comparable to at least one year of disposable income for more than 50% of the households in the sample (Leth-Petersen 2010). To isolate the credit access effect of the reform, we will focus on households with high level of equity-to-value ratios and credit-constrained households, who are most likely to be affected by the expanded credit access of the reform. We will discuss the detailed empirical design in Section 4.4.

Mortgage loan delinquencies and defaults have traditionally been low in Denmark. The LTV ceiling of 80% on new mortgage loans limits lender losses in the event of a default. In addition, mortgage loans are full recourse in Denmark and borrowers remain personally liable for any short-fall between the sale value of a repossessed property and the outstanding amount of the loan.[6] Therefore, borrowers have strong incentives to keep payments and avoid forced sales.

3   Conceptual Framework

Our analysis of the impact of liquidity constraints on labor market outcomes was informed by existing theories of job search. Since most of these models look at unemployment insurance and do not directly apply to expansion of credit access to the unemployed, we will briefly discuss their implications and outline alternative hypotheses.

A first category of models shows that unemployment insurance extends liquidity to unemployed workers and increases the reservation wages (Mortensen 1977; Chetty 2008). As a result less liquidity-constrained unemployed workers have longer unemployment durations, but the effect on wages is ambiguous (Nekoei and Weber 2017; Price 2018). The ambiguous effects on wages is a result of two opposing forces: workers are only willing to accept higher wages, but longer unemployment durations also depress wages over time. Herkenhoff, Phillips and Cohen-Cole (2019) shows theoretically that expanding the credit access of unemployed workers have similar predictions as providing higher unemployment benefits. In this class of models, it is therefore an empirical

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[6]A mortgage loan is declared in default after 3.5 months of non-payment, and forced sale procedures are initiated unless alternative workout procedures are agreed with the borrower. It typically takes no more than nine months from the declaration of default until a forced sale is finalized.

question whether more credit access raises the wages of unemployed workers.

A second category of models shows that higher unemployment insurance raises the workers' outside option and encourage workers to search for high-wage jobs. To see this, consider a simple static model similar to Acemoglu and Shimer (2000). Suppose there are a large continuum of jobs, indexed by their "specificity" $\alpha \in [0,1]$. Each job produces $y(\alpha)$ when filled. A job with higher α produces more output, so y is an increasing function. However, a high α job is also harder to fill for the employers. Workers do not know before applying for the job whether they will be a good fit. High α jobs require a better match between the firm and its employee, so the probability that a random worker possesses the skills and abilities required for a job of specificity α is given by the decreasing function $M(\alpha)$.

A worker consumes her wage w when employed and b when unemployed. Workers and firms get together via search. Jobs are posted at the beginning of the period. Each worker then decides where to apply for a job. After the matching stage, the pair learns whether the worker has the requisite skills. If she does not, both remain unmatched. If she does, the pair produce $y(\alpha)$, and wages are determined by bargaining.

In equilibrium, the worker maximizes her expected utility with respect to α:

$$\max_{\alpha} M(\alpha)u(w(\alpha;b)) + (1 - M(\alpha))u(b) \tag{1}$$

where wage $w(\alpha;b)$ is determined by Nash bargaining:

$$\max(u(w(\alpha;b)) - u(b))^{\beta}(y(\alpha) - w(\alpha;b))^{1-\beta} \tag{2}$$

An increase in the utility when unemployed $u(b)$ increases wages by increasing the specificity of jobs α that workers search for. Going beyond this simple model, a relaxation of credit access allows risk averse agents to smooth consumption over time and increases the utility when unemployed. Since workers are better insured against unemployment, they are more willing to search for jobs that pay high wages but have a lower probability of employment.[7]

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[7]In a general equilibrium, an increase in workers' access to credit could also change the equilibrium job composition, e.g. by creating more high-wage jobs (Acemoglu and Shimer 1999; Acemoglu 2001). While we are not exploring the general equilibrium effects of the mortgage reform in this paper, this implies that comparing workers affected by the reform and workers not affected by the reform might understate the overall positive wage effects of

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An alternative, yet not mutually exclusive, class of models examine the relationship between workers' job mobility and market incompleteness (Hawkins and Mustre-del-Rio 2016; Cubas and Silos 2020). Since job switching is often associated with more volatile earnings and unemployment spells, workers who are less credit constrained are more likely to switch to better occupations and better jobs when facing adverse shocks.

These models predict that expanding credit access to workers would increase their unemployment durations and unemployment risks and therefore reduce the employment rate. While the effect on wages in ambiguous in the first class of models, other models in general predict a wage increase following a relaxation of credit constraints. In the next sections, we will examine the effects of the reform on the wage and employment outcomes of homeowners. We will also test the mechanism more specifically in Section 6.

It is worth noting that another strand of literature studies the effect of debt on labor market outcomes (Donaldson, Piacentino, and Thakor, 2019; Luo and Mongey 2019; Ji 2020). While liquidity and debt are often correlated, the mortgage reform allows us to causally identify the effect of liquidity by unexpectedly relaxing the liquidity constraints for some homeowners, and therefore our results should be interpreted as the effect of liquidity constraints conditional on debt levels.

4   Data and Research Design

4.1   Data

We combine several registers from Statistics Denmark to create a matched employer-employee panel dataset covering all population in Denmark from 1981 to 2000.

The first part of the dataset is regarding wealth and income of the households. The wealth information exists because Denmark had a wealth tax until 1997. The data on assets and liabilities can be divided into a number of categories. Assets are divided into six different categories: housing assets, shares, deposited mortgage deeds, cash holdings, bonds, and other assets. Housing assets are defined as the cash value of property as set by the tax authorities. Tax assessed house values are a bit different from market values, and we scaled them with the aggregate ratio of actual house

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the reform.

prices to tax assessed values at the municipality level. We define liquid assets as the total value of non-housing assets. Liabilities are available under four categories: mortgage debt, bank debt, secured debt, and other debt. Mortgage debt is recorded as the market value of the underlying bonds at the last day of the year. House value, cash holdings, mortgage debt, and bank debt are reported automatically by banks and other financial intermediaries to the tax authorities for all Danish taxpayers and are therefore considered to be very reliable. The remaining components of wealth are self-reported, but subject to being audited by the tax authorities.

The second part of the dataset contains individuals' labor market histories. The data are collected from government registers in the last week of November each year, providing detailed data on the labor market status of individuals, including the unemployed and those who do not participate in the labor force. The data contains detailed information on annual wage income, hourly wage, occupation, and unemployment benefits and durations. Each employed worker is matched to her establishment. Establishments are unique physical work locations, such as an office, store, or factory, and each establishment has a unique identifier that is consistent over time. The database links an individual's ID with a range of other demographic characteristics such as their age, gender, educational qualifications, marital status and number of children.

Since we are exploiting a mortgage reform for our analysis, we focus on individuals who are homeowners in 1991 (the year before the reform). We also focus on those who are between the age of 25 and 55 in 1991, to avoid interference from retirement decisions. In 1991, about 46% of the population between age 25 and 55 are homeowners. Individuals who are living with their parents and those living in a communal or common household are omitted from the sample. This leaves a sample of 762,039 individuals, who are followed from 1987 to 1997.

4.2 Summary Statistics

Table 1 summarizes the statistics of variables on demographics, earnings, and balance sheets for all homeowners in 1991. Housing equity constitutes the majority of assets for most of the homeowners. Most people in Denmark are paid their December salary a few days before the end of the year, and asset holdings are summarized for tax purposes at the end of the year. The median individual has very little liquid assets: the median level liquid asset is less than the median monthly income.

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On the right panel of Table 1, we split the sample by the median of equity to value (ETV) ratio in 1991. The reform allowed individuals to borrow up to a maximum of 80% of the home value. Therefore individuals with ETV lower than 0.25 will be able to extract no or very little housing equity for other purposes. The high-ETV group is older than the low-ETV group since older people are more likely to buy houses at an earlier time. Nevertheless, the other demographic characteristics (gender, marital status, children, education) of high-ETV group is very similar to the low-ETV group, and both groups also have similar wages and unemployment.

At the bottom part of Table 1 we calculate the potential amount of housing equity that was unlocked by the reform as housing equity in 1991 minus 20 percent of the housing value (it takes the value of zero if ETV is less than 0.2). It shows that the amount of equity unlocked was substantial. The reform unlocked an average value of 78,500 DKK (about 12,000 USD) in housing equity. The average amount of housing equity unlocked for people with ETV below 0.25 in 1991 is very little, while the average amount of housing equity unlocked for people with ETV above 0.25 in 1991 is 163,400 DKK, which is close to one year's income. Therefore the reform provided a large positive liquidity shock, but essentially only to the homeowners with ETV above median (0.25) in 1991 with little effect on homeowners with low ETV in 1991.

4.3 Identifying Housing Equity Extraction

We follow Bhutta and Keys (2016) to identify housing equity extractions in the data. We define equity extractions as instances when a borrower's outstanding mortgage debt increases by more than 5 percent over a one year period, with a minimum increase of 5,000 DKK. Since we do not observe the trade line information for each mortgage held, we further require that the borrower do not move over the one year period to exclude second mortgages and new mortgages. This increase in mortgage debt can come from borrowing against housing collateral, or changes in the maturity of the mortgage.

Figure 1 shows the fraction of homeowners in each year who have positive equity extractions. Before 1992 the fraction is around 1%, and these may be false positives of new mortgages (e.g. summer houses). After 1992, the fraction of borrowers with an increase of at least 5% in total mortgage balance has risen sharply to over 5% per year. Between 1993 and 1996, the average

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fraction of homeowners extracting equity is 11.8%, which is close to the fraction in Bhutta and Keys (2016). In 1994, almost 23% of homeowners borrowed against their housing equity.

How does ETV affect equity extraction? Figure 2 (a) shows that the probability of extracting housing equity between 1992 and 1996 is increasing in the ETV in 1991. Borrowers with ETV higher than 0.6 in 1991 are twice more likely to extract their housing equity than households with ETV lower than 0.2 in 1991. Note that the probability of extracting equity is not zero even for households with ETV lower than 0.2 in 1991, since housing prices grew rapidly since 1991 and higher housing prices led to higher ETVs for homeowners. Figure 2 (b) plots the total share of housing equity extracted by the borrower against ETV in 1991. The share of housing equity extracted is the amount of increase in outstanding mortgage debt normalized by the average housing price over the one year period, and we sum up all the shares for years 1992-1996. Borrowers with low ETV in 1991 extracted little equity, while borrowers with ETV higher than 0.6 extracted about 20% of their housing equity.

4.4 Empirical Strategy

The reform allowed individuals, for the first time, to borrow against their housing equity for non-housing purposes. Our research design exploits cross-sectional variation in the exposure to the reform's treatment across individuals. As shown in Figure 2, individuals with higher ETV at 1991 are more likely to borrow against housing equity and are able to extract more housing equity after the reform. We therefore divide all individuals into two groups based on whether their ETV in 1991 is higher than the median (0.25). We then use a difference-in-differences approach to compare the differential responses of the liabilities, income and employment of the two ETV groups to the reform. Given that the reform was first introduced in May of 1992 and data are recorded as of November, we include 1992 in our post-reform period and measure individual attributes as of 1991.

Our baseline specification is as follows:

$$y_{it} = \beta Post_t \times \mathbf{1}(ETV_{91} > 0.25)_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it} \tag{3}$$

where y_{it} is the debt or labor outcome for person i at year t, $Post_t \times \mathbf{1}(ETV_{91} > 0.25)$ equals one if person i had ETV greater than 0.25 in year 1991 and year t is 1992 or later. The key coefficient

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is β, which measures the high-ETV group's response to the reform relative to the low-ETV group, who were affected little by the reform by construction.

Since there is an almost linear relationship between ETV in 1991 and housing extraction (Figure 2), in an alternative specification, we also interact the post-reform dummy with the level of ETV in 1991:

$$y_{it} = \beta Post_t \times ETV_{91,i} + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it} \tag{4}$$

We include person fixed effects in all regressions. Standard errors are clustered at the municipality level. We also account for the differential response of individuals at different points in the life cycle, wealth, and working in different industries and living in different municipalities by including an interaction between these individual covariates measured in 1991 and year fixed effects. Specifically, we include in X_i^{1991} indicators for the individuals' gender, education level, marital status, children, age, decile of total household wealth,[8] the municipality of residence, and the occupation category. We interact each of these characteristics with year dummies, ϕ_t, to control for different trends in debt accumulation and earnings across people with different observable characteristics. Thus we are comparing two "identical" individuals (in terms of their age, gender, educational background, wealth, marital status and children) who work in the same occupation and live in the same municipality, but one who bought the home some years before the other.

One concern is that part of the variation in ETVs in 1991 comes from difference choices of mortgage payment plans (how much to pay as down payment and how much to repay monthly) and local housing price changes, which could be potentially correlated with individuals' expected future earnings. To investigate this possibility, we propose using the year of housing purchase as an instrument for the ETV in 1991. Figure 3 shows that the timing of housing purchase strongly predicts the ETV in 1991. The average ETVs range from 0.3 to 0.7 if houses were purchased before 1982, and are 0.25 or even lower[9] for houses purchased within 10 years of the reform. We estimate

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[8]The asset levels would affect workers' attitude towards risk. For example, with constant relative risk aversion, richer workers have lower absolute risk aversion. As a result, they are more willing to accept riskier jobs, compared to poorer workers.

[9]The average ETV in 1991 is below 0.2 for houses bought between 1985 and 1990 because housing prices have been declining between 1985 and the reform in 1992.

the first stage:

$$Post_t \times \mathbf{1}(ETV_{91} > 0.25)_i = \beta_1 Post_t \times \mathbf{1}(TimePurchase < 1982)_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \epsilon_{it} \quad (5)$$

where $\mathbf{1}(TimePurchase < 1982)_i$ is a dummy variable equal to 1 if person i purchased the house before 1982. The control variables are the same as in equation (3). If the timing of housing purchase predicts ETV in 1991, we would expect β_1 to be positive and highly statistically significant. The second stage takes the predicted ETV from equation (5) and looks at the effect on debt and labor outcomes (we run this using 2SLS to obtain the correct standard errors):

$$y_{it} = \beta_2 \widehat{Post_t \times} \mathbf{1}(ETV_{91} > 0.25)_i + \theta X_i^{1991} \times \phi_t + \alpha_i + u_{it} \quad (6)$$

where we are using the timing of house purchase as an instrument to see the effect of liquidity constraints driven by differences in ETV in 1991 on individual debt and labor outcomes.

Our identifying assumption is that, conditional on the observed covariates in 1991, the timing of the housing purchase is uncorrelated with changes in employment and wages after 1992. The fact that the mortgage reform was unexpected indicates that the reform did not directly impact the decision to purchase houses before 1992. Table 1 shows that individuals with high ETV are older and have less debt, but have similar marital status, children, education, and income as individuals with low ETV. Although age is an important determinant of the timing of housing purchase, even for people with the same age there is a lot of variations in the timing of housing purchase.[10] Potential threats to identification would be unobserved shocks that affect both the timing of housing purchase and the changes in employment and wages after 1992. For example, individuals who purchased houses more recently may have experienced a recent divorce, which may also affect their income. In such case, the incomes of different ETV groups would have started to diverge before the 1992 reform, and we can use the pre-trend to assess the validity of the identifying assumption. We also perform a placebo test using the pre-reform years: if the timing of housing purchase is systematically correlated with future income growth, we would also expect to see similar patterns even without the reform.

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[10]For example, housing purchases can be driven by life events (Bernstein and Struyven 2017; Bernstein and Koudijs 2020) or beliefs about future changes in housing prices (Bailey et al. 2018).

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To further isolate the effects of the reform on individuals' liquidity constraints, we compare the effects of the reform on individuals with high level of liquid assets and individuals with low level of liquid assets. Since the key element of the reform is to relax individuals' liquidity constraints by allowing them to borrow against housing equity, it should have little effect on individuals who already have a large buffer of liquid assets. We define an individual as having low liquidity if her average level of liquid assets is less than her average monthly income between 1986 and 1990.[11] By this definition, almost 50% of all the individuals in our sample have low liquidity before the reform.

To estimate the differential effect of the reform on the high-liquidity and low-liquidity households, we estimate the following triple-differences specification:

$$y_{it} = \beta Post_t \times \mathbf{1}(ETV_{91} > 0.25)_i + \gamma Post_t \times \mathbf{1}(ETV_{91} > 0.25)_i \times \text{LowLiquidity}_i +$$
$$\delta Post_t \times \text{LowLiquidity}_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it} \tag{7}$$

where LowLiquidity_i is an indicator for having less liquid assets than one month's disposable income between 1986 and 1990. β is the effect of the reform on the high-ETV group relative to the low-ETV group among high-liquidity individuals, and $\beta + \gamma$ is the effect of the reform high-ETV group relative to low-ETV group among low-liquidity individuals. The difference γ measures the differential response of the credit-constrained individuals relative to the unconstrained individuals to the increased credit access.

5 Results

5.1 Effects of the Reform on Borrowing

To verify that the mortgage reform impacted the homeowners, we first look at the effects of the reform on equity extraction and the overall liabilities. Columns 1 to 3 of Panel A in Table 2 show

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[11]We use the years prior to the reform so that differences in liquidity is less likely to be driven by reverse causality. We also use alternative measures including liquid asset to income ratio in 1991 and the *maximum* liquid asset to income ratio in 1986-1990, and get similar results. Liquid asset holding is not a perfect indicator of constrained status (Jappelli 1990). For the test implemented here a sufficient requirement is that the high liquid asset group is not constrained. It is not required that households with low liquid assets are all restricted, only that some households in the low liquid asset group are affected by constraints.

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results from difference-in-differences regressions of measures of borrowing on indicators for high- and low- ETV groups after 1992 (Equation (3)). The unit observation is person-year. Following the mortgage reform, individuals with high ETVs are more likely to extract housing equity and extract a larger share of their housing equity, confirming the findings in Figure 2. In column 3, we use total liabilities divided by average annual income as the dependent variable. Total liabilities include mortgage, bank debt and other secured and unsecured debt, and average income is the average annual income during the period 1987-1997. High-ETV individuals increased their debt level substantially after the reform: individuals with ETVs higher than 0.25 in 1991 increased their total debt level by 13.3% of their annual earnings more than individuals with ETVs lower than 0.25 in 1991. This indicates that the increased borrowing of housing equity did not simply replace other forms of debt, such as bank loans. The positive effect of ETV on total debt levels after 1992 is consistent with Leth-Petersen (2010).

Next, we study how the effects differ by whether the individual is liquidity constrained or not. If the reform increased the level of debt because it relaxes the credit constraint, it should have less impact on the borrowing for individuals who have a lot of liquid assets and are not credit constrained. Columns 4 to 6 of Table 2 show the triple-differences estimates (Equation (7)). First, the triple-interaction terms of low liquidity, high ETV and post 1991 have positive and significant effects for all three measures, indicating that individuals with little liquid assets borrow more against housing equity and increase their debt more after the reform. Second, among individuals with a high level of liquid assets and thus not liquidity constrained, those with high ETVs also borrow more against housing equity, but the change in total debt level is smaller. For example, households with high liquidity and ETV higher than 0.25 increased their total debt by 10.4% of annual earnings, while households with low liquidity and ETV higher than 0.25 increased their total debt by 16.4% of annual earnings.

In Panel B of Table 2 , we use the continuous measure of ETV in 1991 as the treatment variable (Equation (4)) and get similar results. A one-standard-deviation increase in ETV of 1991 increases debt level by 11% of an annual salary. The effect on borrowing is larger for liquidity-constrained individuals than non-constrained individuals.

These results indicate that the reform indeed relaxed the credit constraints for individuals' with

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high ETVs. Homeowners with higher ETVs borrowed against their housing equity and increased overall debt levels, and the effect is larger for credit-constrained individuals.

5.2 Effects of the Reform on Wages and Employment

How does the relaxation of credit constraint affect wages and employment? Table 3 shows the results from our baseline regressions using measures of wages and employment as dependent variables. In column 1, we use log annual wage as dependent variable. Following the reform, individuals with ETVs higher than 0.25 in 1991 experienced a wage gain of 0.96% relative to individuals with ETVs lower than 0.25 in 1991. In column 2, we use normalized earnings as dependent variable where we divide annual earnings by the average annual earnings from 1987 to 1997. This measure takes into account individuals with zero earnings.[12] We find that high-ETV individuals experienced a 0.95% increase in earnings. In column 3, the dependent variable is an employment indicator, which equals to one if the individual has positive earnings and zero otherwise. The employment rate of the high-ETV group increased by 0.08%, but the difference is not statistically significant.

Columns 4 to 6 of Table 3 present results for triple-differences specification (equation (7)). Among credit-constrained individuals, an ETV of greater than 0.25 leads to a 2.1% increase in wages. On the other hand, for non-constrained individuals, the effect of high ETVs on wages is negative and not statistically significant from zero. This suggests that the higher earnings experienced by the individuals with high ETVs are due to the relaxation of borrowing constraint for liquidity-constrained individuals. In line with the stylized model in section 3, we find a negative effect of the reform on the employment rate for the liquidity constrained workers. For the non-liquidity-constrained workers we find a positive effect on the employment rate.[13]

In Panel B of Table 3, we use continuous ETV as the treatment variable. A one-standard-deviation (0.3) increase in ETV in 1991 increases wages by 0.7 percent on average, and increases

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[12]The normalized earnings are winsorized at 1st and 99th percentile. Results are similar when normalizing earnings by the average earnings before the reform (1987-1991).

[13]The third part of the reform, which introduces the option to refinance, is a positive shock to the wealth of homeowners. This shock to the wealth is larger for low-ETV households, who have more debt outstanding and can gain more by taking advantage of the lower interest rate. Since we would expect a positive wealth shock to reduce the labor supply (Cesarini et al. 2017), this can potentially explain the positive employment effects we find for the high-ETV individuals relative to the low-ETV individuals among the high-liquidity individuals. In principle, the positive employment effect could also be a general equilibrium effect: the lower employment rate of the liquidity-constrained individuals makes it easier for the non-liquidity-constrained individuals to find jobs due to less competition for (lower wage) jobs.

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wages by 1.4 percent for liquidity-constrained individuals.

How big is this effect? The estimates in column 5 indicates that the earnings of liquidity-constrained individuals with ETVs higher than 0.25 increase by 1.8% after the reform. Assuming that the earnings growth remain the same afterwards, and that careers last 20 years and discount rate is 5 percent, an 1.8% earnings increase implies an increase in present discounted value equal to 23% of the annual earning, which is larger than the increase in the amount of borrowing by these individuals (16% of the annual earning from column 6 of Table 2).

To test whether the wages of the high-ETV group and the low-ETV group would have followed parallel trends without the reform, we estimate the treatment effects on wages over time as follows:

$$y_{it} = \alpha_i + \sum_{\tau=1987}^{1997} \beta_\tau \mathbf{1}(ETV_{91} > 0.2)_i \times D_t(\tau) + \theta X_i^{1991} \times \phi_t + \varepsilon_{it} \tag{8}$$

where $D_t(\tau)$ is equal to one if $t = \tau$. β_τ is the effect of high ETV on wages in year τ, and year 1991 is chosen as the base year. Figure 4 plots the coefficients β_τ. The effects are insignificant from zero before 1991, and becomes positive and significant after 1993 (two years after the reform). Interestingly, the wage gap between high-ETV and low-ETV individuals keeps widening over time. One potential reason is that working at high-wage jobs has persistent positive effects on workers' careers (Oreopoulos, von Wachter and Heisz 2012).

To confirm that the effect is driven by liquidity constraint, we estimate the same regression separately for low-liquidity individuals and high-liquidity individuals and plot the coefficients in the bottom figure of Figure 4. For both groups, individuals with high ETVs have similar wage trends as individuals with low ETVs before 1991, implying that conditional on controls individuals with different levels of ETVs follow similar counterfactual wage trends. Following the reform, having higher ETV has no effect on wages for the individuals with a lot of liquid assets, while higher ETV leads to higher wage growth for individuals with little liquid assets, suggesting that being able to borrow against housing equity leads to higher wage growth for liquidity-constrained individuals.

5.3   IV Results

Panel A of Table 4 reports the first stage of the IV regression, where we instrument for ETV in 1991 using the timing of the home purchase. Having purchased the house before 1981 strongly

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predicts an ETV above 0.25 in 1991. The first-stage F-statistics are above 100 in all regressions.

Panel B shows the 2SLS estimates. Consistent with the OLS estimates, people with ETV higher than 0.25 in 1991 are more likely to extract housing equity and increase the overall debt levels more, and experience higher earnings. The effect on earnings is positive and larger in magnitude than OLS: high-ETV individuals earn 1.2% higher wages and 1.6% higher earnings. In Appendix Figure 2, we plot the reduced form effect of whether the housing purchase is before 1981 on wage income over time, and find that individuals buying houses before 1981 and individuals buying houses after 1981 followed parallel trends in wages prior to the reform conditional on observables.

Panel C reports the triple-difference estimates, where interactions of the post-1992 dummy and the high-ETV dummy and the triple interactions of the post-1992 dummy, the low-liquidity dummy and the high-ETV dummy are instrumented by the post-1992 dummy (and the low-liquidity dummy) interacted with the predicted 1991 ETV based on the timing of housing purchase. Consistent with the OLS estimates, liquidity-constrained individuals with high ETVs in 1991 had higher debt levels, higher earnings and a lower employment rate after the reform, while non-constrained individuals with high ETVs in 1991 had only a small increase in debt levels, no significant change in earnings and a higher employment rate.

In general, the IV estimates of the effects on wages are bigger than the OLS estimates. There could be a number of reasons for the difference. First, ETV in 1991 may be measured with error, which would attenuate the OLS, but not the IV estimates. Second, it is possible that individuals, who expect higher wage growth in the future, choose lower down payment and monthly payments at the beginning and have lower ETVs. However, it is important to recognize that the OLS and IV coefficients are not statistically different. This suggests the difference between the point estimates could also be driven by estimation error. The most important insight from this section is that the IV analysis confirms the positive effect of access to housing equity on wages.

5.4 Heterogeneous Effects

We examine the heterogeneity of treatment effects by demographic characteristics in Table 5. Each column is a separate regression for all individuals in a demographic group, and the dependent variable is log wage.

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Columns 1 to 3 show that workers with basic education and workers with college education benefited the most from the reform. Liquidity constrained individuals in both groups experience a wage gain of around 3% after the reform, while workers with vocational education only experienced a modest wage gain of less than 1%. This might be due to the fact that workers with vocational training have more rigid union wage structure and lower income volatility (Dahl, le Maire and Munch 2013).

Columns 4 and 5 show that women have slightly larger wage responses to the reform than men, although the difference is not statistically significant. The last two columns show that younger workers experienced larger increases in earnings following the reform compared to older workers. One reason might be that younger workers are more liquidity constrained. Bhutta and Keys (2016) find that the equity extraction of young homeowners are more responsive to house price growth since they are more likely to be collateral constrained. Another reason might be that working for low-paying firms have negative long-term career consequences for young workers (Oreopoulos, von Wachter and Heisz 2012).

5.5   Robustness

5.5.1   Unobserved heterogeneity

As discussed in Section 4, the main identification challenge is that there is unobserved heterogeneity between people who bought the house earlier and people who bought the house later. This leads us to include a rich set of controls in the baseline specification, so that we are effectively comparing individuals with the same age, gender, education level, wealth level and family status. In this section, we explore the possibility that there are other unobserved shocks that affect both the timing of house purchase and labor market performance after the reform.

Labor market shocks

One possible threat to the validity of our design is that individuals in different jobs have different income shocks, which could be correlated with the decision to purchase homes. In particular, different industries may have different cyclicality, which could lead to different housing purchase decisions and different labor market performance during and after recessions. Our dataset has

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detailed information about the industry each individual works in, which allows us to control for industry-by-year fixed effects at a very granular level.[14] In addition, we also control for deciles of income level interacted with year fixed effects to absorb differences in income shocks across the income distribution.

We report the results in Appendix Table 1. The estimated coefficients remain similar with the inclusion of the additional labor market controls, suggesting that industry-specific income shocks as well as shocks by income level do not drive our results.

Linear pre-trend

A related concern is that the expectation of income growth is correlated with the timing of house purchase. For example, if people expecting faster income growth in the future purchase homes sooner, then it could explain the positive correlation between higher ETV and higher income growth. Nevertheless, the lack of pre-trend prior to the reform suggests that high-ETV and low-ETV individuals have similar income growth prior to the reform.

To further address this concern, we estimate a variation of the baseline specification including a linear trend to absorb differences differences in income trends between the high-ETV and low-ETV individuals:

$$y_{it} = \beta Post_t \times \mathbf{1}(ETV_{91} > 0.25)_i + \theta X_i^{1991} \times \phi_t + \delta t \times \mathbf{1}(ETV_{91} > 0.25)_i + \alpha_i + \varepsilon_{it} \qquad (9)$$

The regression allows for a linear pre-trend, such that our estimated effects are relative to a linear time-trend. Consistent with the insignificant pre-trend in the event study, Appendix Table 2 shows that we get similar estimates with the inclusion of the linear pretrend.

Placebo test

To further test whether individuals with high ETVs would have parallel trends in wages and employment as individuals with low ETVs, we conduct a placebo test using only years before the mortgage reform. If there are systematic differences between high-ETV individuals and low-ETV individuals as well as between high-liquidity individuals and low-liquidity individuals that are not

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[14]We use an industry breakdown with 60 industries in total.

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specific to year 1992 and explain our findings, then we should also observe similar effects in other years when there is no reform and people cannot borrow against housing equity. We pick the year 1986 as the year of the placebo reform so that we can observe five years 1986-1990 without the actual reform being announced.

We estimate the following specification:

$$y_{it} = \beta Post86_t \times ETV_{85,i} + \theta X_i^{1985} \times \phi_t + \alpha_i + \varepsilon_{it} \tag{10}$$

where $Post86_t$ is an indicator for years after 1986, and $ETV_{85,i}$ is the ETV in 1985.

We divide the period into a pre-period (1982-1985), and a post-period (1986-1990), and test whether individuals with higher ETVs in 1985 had higher wage growth between 1986 and 1990. All observable characteristics are measured in 1985. We also divide individuals into high-liquidity and low-liquidity groups based on their liquid assets between 1982 and 1985 and apply the triple-differences specification as in Equation (7).

Appendix Table 3 presents the results of the placebo test. All estimates of the effects of high ETV and the interaction terms are statistically insignificant from zero and tend to be much smaller in magnitude than our baseline results. This suggests that liquidity-constrained individuals with high ETVs had faster wage growth after 1992 precisely because the reform allowed home equity loans and relaxed their credit constraints.

5.5.2 Alternative measures of liquidity

In this section, we examine the robustness of our results to alternative measures of liquidity of households. Our baseline measure uses the ratio of liquid assets to monthly income, where liquid assets includes cash, bank deposits, stocks and bonds. We consider two alternative measures. First, we consider a narrower measure which excludes stocks and bonds from the calculation of liquid assets as stocks and bonds could be subject to transaction costs. Second, we follow Kaplan, Violante and Weidner (2014) and use an indicator for whether an individual is hand-to-mouth in 1991 as the measure for liquidity. In particular, a person is hand-to-mouth if the liquid assets minus liquid liability is lower than half of per-pay-period income minus the credit limit. We take the pay frequency to be two weeks and credit limit to be one month of income. By this definition,

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around 30% of homeowners in Denmark are hand-to-mouth in 1991.

Appendix Table 4 reports the results from the triple-difference regressions using these two alternative measures of liquidity constraints. Similar to our main results, liquidity-constrained individuals are more likely to extract housing equity and increase their debt levels more. Liquidity-constrained individuals also have higher wages and lower employment rates following the reform, while for non-constrained individuals the effects on wages are insignificant and there is a positive effect on the employment rate.

6 Mechanisms

In this section, we explore the mechanisms of how expanding credit access leads to higher earnings. As shown in our conceptual framework, a relaxation of credit constraints increase the reservation wage and value of unemployment, and allows individuals to wait longer for better jobs, as well as search for more highly-paid jobs. We first start by describing which individuals borrow from housing equity, and show that access to housing equity are indeed used to insure against negative labor income shocks. Then, we consider the labor market outcomes of unemployed individuals, and show that individuals with more housing equity stay in unemployment for longer and get higher reemployment wages. Finally, we look at job switching behavior to examine the job search channel.

6.1 Who Borrows Against Housing Equity?

We start our analysis by looking at the determinants of equity extraction. If the additional borrowing from housing equity provides insurance against negative labor market shocks, we would expect to see more borrowing when individuals experience negative labor market shocks. For example, Kaplan (2012) finds that workers are more likely to move back home to live with their parents when they lose their jobs.

We estimate a linear probability model of the propensity to extract housing equity:

$$\text{Extract}_{ict} = \beta_1 \text{IncomeShock}_{it} + \gamma X_i^{1991} + \alpha_{ct} + \epsilon_{ict} \tag{11}$$

where Extract$_{ict}$ is an indicator variable for housing equity extraction, IncomeShock$_{it}$ is a measure of income shocks to person i in year t. The vector X_i^{1991} includes individual-level covariates including ETV in 1991, the level of liquid assets in 1991, and decile of total wealth in 1991. We also include municipality-year fixed effects to account for different housing price trends at the municipality level. The unit of observation is person-year, and we only include observations for homeowners after 1992.

Table 6 presents the results. In column 1, we measure income shocks in year t using percent growth in labor income from year $t-1$ to year t. Individuals that experienced a negative income shock are more likely to borrow against housing equity. For example, a 20% reduction in income leads to a 0.56 percentage point rise in equity extraction. This corresponds to an increase by 5% relative to the average extraction rate of 11 percent across all years after 1991. Column 2 includes person fixed effects to account for unobserved heterogeneity across homeowners in their propensity to extract equity that may be correlated with labor market outcomes. We find that homeowners are more likely to extract equity after being hit with negative income shocks, although the estimate is lower.

In columns 3 and 4, we test whether homeowners are more likely to extract housing equity when becoming unemployed. We find that unemployed workers are 4 percentage points more likely to extract housing equity, an increase of 36% relative to the average probability of extraction.

In the last four columns, we test whether homeowners whose employers experience negative shocks are more likely to extract housing equity. The shocks to employers cannot be diversified or avoided and is a measure of uninsurable risk to the income of workers (Fagereng, Guiso and Pistaferri 2018). In columns 5 and 6, the independent variable is an indicator for mass layoffs at the worker's establishment, where a mass layoff is defined as a reduction in employment by over 30% in an establishment with 50 or more employees. In columns 7 and 8, the independent variable is average wage changes for all incumbent workers at the worker's establishment from year $t-1$ to year t. The coefficients show that workers experiencing mass layoffs and wage cuts at their employers are more likely to extract housing equity.

These results show that workers experiencing negative income shocks are more likely to extract housing equity. One important point is that regardless of realized borrowing, the potential to

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borrow affects job search decisions no matter whether the home equity is actually extracted. Workers know that if their buffer stock of liquid assets is depleted, they can borrow, and this affects their job search decisions even if they never borrow. Nevertheless our results suggest that borrowing against housing equity provides an important buffer against negative labor market shocks to homeowners. As shown in Appendix Figure 3, the percentage of workers experiencing negative income shocks peaked around 1993, and the liquidity buffer provided by the home equity loans was particularly important in the first few years after the reform came into effect.

6.2 Effects on Unemployed Workers

Extra credit from housing wealth allows unemployed households to augment today's liquid asset position by borrowing against future income. Chetty (2008) shows that increases in unemployment benefits or severance payments lead to longer unemployment durations, especially for liquidity constrained households. Herkenhoff, Phillips and Cohen-Cole (2019) find that better access to consumer credit increases unemployment durations and wages conditional on finding a job.

To examine how the borrowing against housing equity affect the job search behavior of unemployed workers, we compare unemployment durations and reemployment wages of workers who are unemployed in 1991 right before the mortgage reform and have different levels of housing equity. We consider only people who became unemployed before the reform so that our estimates only reflect the effect of the reform conditional on being unemployed, and do not contain the effect of the reform on the selection into unemployment. In particular, we estimate the following equation for the workers unemployed in 1991:

$$D_i = \gamma \mathbf{1}(ETV_{91} > 0.25)_i + \pi \mathbf{1}(ETV_{91} > 0.25)_i \times \text{LowLiquidity}_i + \theta \text{LowLiquidity}_i + \beta X_i + \varepsilon_i \quad (12)$$

where D_i is the unemployment duration of individual i, control X_i include pre-unemployment wage, age fixed effects, municipality fixed effects, and dummies for year entering unemployment. The coefficients of interest are γ, which is the effect of having positive housing equity on unemployment duration, and π, which is the differential effect of having positive housing equity for liquidity-constrained individuals relative to non-liquidity-constrained individuals.

We first estimate equation (12) using OLS including only people who have re-entered the labor

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market by 2005. Columns 1 and 2 of Table 7 show that having positive housing equity on average increases unemployment durations by 0.14 years, or 7 weeks. Liquidity-constrained households increased unemployment durations by 0.18 years, or 9.5 weeks, while non-liquidity-constrained households increased their unemployment durations by 0.06 years, or 3.2 weeks.

In columns 3 and 4, we estimate a Cox proportional hazard model, which accounts for censoring of workers who never reentered the labor market. We specify the log hazard to be the linear function on the right hand side of equation (12) plus a constant. The coefficient is negative and significant, indicating that high-ETV individuals have a lower hazard rate and longer unemployment durations. The effect is more pronounced among liquidity-constrained workers.

In columns 5 and 6, we consider how access to housing equity affects reemployment wages. The dependent variable is the change in log wage between pre- and post-unemployment jobs. On average, the access to housing equity has insignificant positive effect on reemployment wages. However, the effect is opposite for liquidity-constrained and non-liquidity-constrained individuals: liquidity-constrained households with high ETVs experienced a 5.7% higher reemployment wages, whereas non-constrained households with high ETVs experienced 1.9% lower reemployment wages.

The increase in unemployment durations has two opposing effects on reemployment wages (Chetty 2008; Nekoei and Weber 2017; Price 2018). On the one hand, when provided enough liquidity, unemployed workers are able to search more patiently and wait longer for better job matches. On the other hand, having more liquidity could also distort search incentives and reduce search efforts, which lead to worse jobs following unemployment. Our results suggest that the former effect dominates for liquidity-constrained workers. Our results for liquidity-constrained individuals are similar to Herkenhoff, Phillips and Cohen-Cole (2019), who find that an increase in unused revolving debt of one year's income leads to an increase in unemployment durations by 0.11 years and an increase in reemployment wages by 6%.[15]

Since unemployed workers only account for about 10% of the sample, the higher reemployment wages of unemployed workers can only explain less than half of the positive wage effects of the

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[15]In Denmark, the unemployment system is two-tiered. It is voluntary to pay a fee to be a member of an unemployment insurance fund and be insured against unemployment. If not eligible for unemployment insurance benefits, the worker might still be able to receive mean-tested social assistance benefits. Markwardt, Martinello and Sándor (2014) find that people with more housing equity are less likely to take up UI benefits. Our estimates reflect a combination of the effect of an increase in liquidity provision through housing equity and the effect of a decrease in the take-up of UI benefits.

reform.[16] In the next section, we explore the effects of the reform on the wages of employed workers.

6.3 Effects on Employed Workers

As we have discussed in the conceptual framework in Section 3, a relaxation of credit constraints could also increase the wages of employed workers by encouraging workers to switch to better-paid jobs and better-paid firms. A better insurance against income risks also encourages workers to search for high-wage jobs and switch firms or careers when experiencing negative shocks.

In Table 8, we rerun the baseline specification restricting to only workers who were employed with positive wages at the time of the reform in 1992. We find a slightly smaller, yet positive and significant effect on wages for the group of employed workers. For example, for workers with low liquidity, having access to housing equity raises wage levels by 1.9% and total earnings by 1.7%. We also find a positive effect of access to housing equity on the employment rate for high-liquidity individuals and a negative effect on the employment rate for low-liquidity individuals, although the effects are half as large as the baseline results using the entire sample and only significant at the 10% level.

We then explore the channels through which access to housing equity affects the wages of employed workers. We first look at whether workers are more likely to switch jobs after the reform allowed them to borrow against housing equity. Table 9 shows the difference-in-differences estimates as in Equation (7). The dependent variable in column 1 is an indicator variable that equals one if the worker switches employer. Liquidity-constrained individuals with ETV higher than 0.25 are 0.5% more likely to switch jobs after the reform, while non-constrained individuals do not significantly change their job switching behavior.

We then split job mobility to upward and downward movements. A worker "moves up" if the wage at a new employer is higher than the previous wage, and "moves down" if the wage at a new employer is lower. Columns 2 and 3 show that liquidity-constrained individuals with high ETVs are 0.8% more likely to move up, and 0.3% less likely to move down. The differences are

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[16]For example, if we consider the triple-differences estimates in column 6 of Table 7, the effect on the reemployment wages of low-liquidity high-ETV individuals is 5.7%. When multiplying by the unemployment rate of 1992 (12%), this explains roughly 20-30% of the positive wage effects in column 4 of Table 3.

not statistically significant for non-constrained individuals. This suggests that workers who can borrow against housing equity move to better jobs and avoid falling off the job ladder.

Next, we directly test whether access to housing credit allows people to move to better firms and occupations. We measure the wage level of firms using two approaches. The first is the coworkers' average wage within the establishment. The second measure is establishment wage fixed effects (Abowd, Kramarz and Margolis 1999). We estimate a two-way fixed effects model as in He and le Maire (2019) for all workers (including non-homeowners) and all establishments for the period 1980-2000, and use the estimated establishment fixed effects as a measure for the establishment-specific wage premium. Columns 4 and 5 of Table 9 show that workers with high ETVs move to firms that pay higher wages after the mortgage reform. Liquidity-constrained individuals with high ETVs in 1991 are employed in establishments that pay 0.4% higher average wages to coworkers and 0.3% higher wage premiums.

Finally, in column 6 we consider whether workers move to better occupations following the reform. The dependent variable is the average real wage of the occupation a worker belongs to. We find that non-constrained workers with high ETVs move to occupations with lower wages after 1992, and liquidity-constrained workers with high ETVs move to occupations with relatively higher wages, although the positive effect is not statistically significant.[17]

6.4 Discussion of Alternative Mechanisms

6.4.1 Entrepreneurship

One alternative explanation for our findings is that the option to borrow against housing equity encourages workers to start up their own businesses and earn more. Schmalz, Sraer and Thesmar (2012) shows that an increase in the value of housing collateral leads to higher probability of becoming an entrepreneur. Jensen, Leth-Petersen and Nanda (2015) studied the same mortgage reform as our paper, and find that homeowners with high ETVs in 1991 are more likely to become entrepreneurs. In unreported results, we find that individuals with high ETVs in 1991 have a 0.1%

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[17]The relaxation of credit constraint may also lead to better jobs by encouraging workers to invest more in human capital accumulation. Similar to firms cutting investment when financially constrained, individuals may also invest less in human capital when credit constrained (Sun and Yannelis 2016; Fos, Liberman and Yannelis 2017). In unreported results we find that there is a small increase in probability of training for individuals with high ETVs after the reform, but the effect on the duration of training is insignificant.

higher probability of becoming self-employed, which is consistent with Jensen, Leth-Petersen and Nanda (2015). However, the effect on entrepreneurship rate is too small to explain the increase in earnings for all workers – for entrepreneurship to explain all of the increase in earnings, the earnings of the entrepreneurs would have to be more than 5 times higher than the earnings in other jobs.

To further investigate how much of the earnings increase is due to entrepreneurship, we re-ran our baseline regressions excluding individuals who were self-employed between 1992 and 1997. Results are shown in Appendix Table 5. After excluding entrepreneurs from the sample, we still find a similar earnings increase among individuals who had high ETVs in 1991 and were liquidity-constrained. Therefore an increase in the rate of entrepreneurship cannot explain the positive effect of credit access on earnings.

6.4.2 Financial distress

Another alternative explanation is that individuals with low ETVs are more likely to experience financial distress, which could depress their earnings. In particular, when the ETV is negative, households may engage in "strategic" default (Mayer et al. 2014), and the default would cost time and energy and increase stress, which might hurt job performance or reduce job search among the unemployed (Dobbie and Song 2015; Bernstein 2019).[18] Negative housing equity may also prevent households from moving geographically and searching for jobs widely, known as the "housing lock" (Brown and Matsa 2017).

In Appendix Table 6, we exclude individuals who have ever had negative ETV between year 1991 and 1997, which is nearly 20% of the sample. Even among the group of individuals who always had positive housing equity during the sample period, we find almost the same effects as with the baseline sample. Therefore, negative ETVs and the resulting financial distress is unlikely to drive our baseline results.

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[18]Direct effects on credit scores from distress could hurt labor market outcomes because of employer screening (Bos, Breza and Liberman 2015), though Dobbie et al. (2020) show that removal of bankruptcy flags do not significantly affect labor income.

6.4.3 Productivity

A related channel through which liquidity could affect labor market decisions is worker productivity. Bernstein, McQuade, and Townsend (2019) show that a decline in the housing wealth is associated with lower productivity for innovative workers. One explanation put forth by the authors is that declines in housing wealth could cause reductions in consumption (Mian, Rao and Sufi 2013) and specifically decrease spending on labor-augmenting goods and services (Aguiar, Hurst and Karabarbounis 2013). For example, if innovative workers with high future productivity and access to home equity loan are more likely to pay for home services that may free up additional time they can engage in working and innovating, that may be more likely to increase their productivity. Having a liquidity buffer through housing equity may also reduce workers' level of anxiety and stress (Engelberg and Parsons 2016) and boost their productivity at work.

In Appendix Table 7, we include person-establishment fixed effects (i.e. job spell fixed effects) to study the effect of liquidity constraints on wages within jobs. If additional credit access raises productivity, we should expect wages to go up for workers staying in their current jobs. However, we find that homeowners with high ETVs have slightly lower within-job wage changes after 1992. In column 2, we interact ETV with liquidity, and find that there is a positive, yet small and statistically insignificant effect on wages within jobs for liquidity-constrained individuals. Splitting the sample by education levels shows that this positive effect on wages is entirely driven by college-educated workers. Therefore, while the productivity channel might explain a small part of the wage gains of college-educated workers, it does not seem to be the primary driver of our baseline results.

7   Conclusion

Housing assets constitute the majority of wealth for most households, but they are highly illiquid, and many individuals are liquidity constrained despite owning a large amount of housing wealth (Boar, Gorea and Midrigan 2020). In this paper, we exploit a natural experiment in Denmark, which allowed homeowners to borrow against housing equity, and find that the expanded credit access increased earnings and job quality for liquidity-constrained individuals.

While it has been well established that policies relaxing household liquidity constraints (e.g. debt relief, stimulus checks) can help stabilize consumption and employment in recessions through

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an aggregate demand channel (Agarwal et al. 2017; Ganong and Noel 2020; Auclert, Dobbie and Goldsmith-Pinkham 2019), our results show that providing liquidity to households can have an additional positive effect on earnings and output through a labor market search channel. Our setting focuses on the period around the 1992 recession in Denmark, and our results can potentially apply to other episodes with high unemployment and liquidity-constrained households. Further understanding how other policies that target the liquidity constraints of both wealthy and poor hand-to-mouth households affect their labor market outcomes, and how it interacts with the aggregate demand channel, will be an area for fruitful future research.

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Figure 1: Share of homeowners extracting equity by year
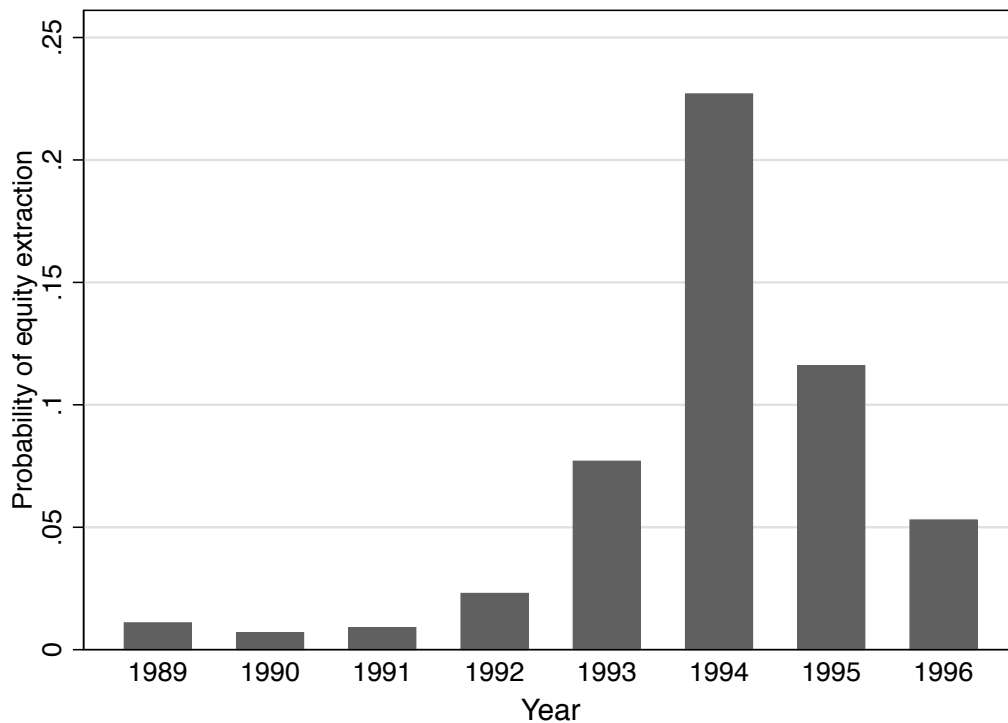
Notes: This figure shows the share of homeowners extracting housing equity in Denmark by year. Following Bhutta and Keys (2016), we define extraction of housing equity as instances when a borrower's outstanding mortgage debt increases by more than 5 percent over a one year period, with a minimum increase of 5,000 DKK. Since we do not observe the trade line information for each mortgage held, we further require that the borrower do not move over the one year period to exclude second mortgages and new mortgages.

Figure 2: Equity extraction by ETV in 1991
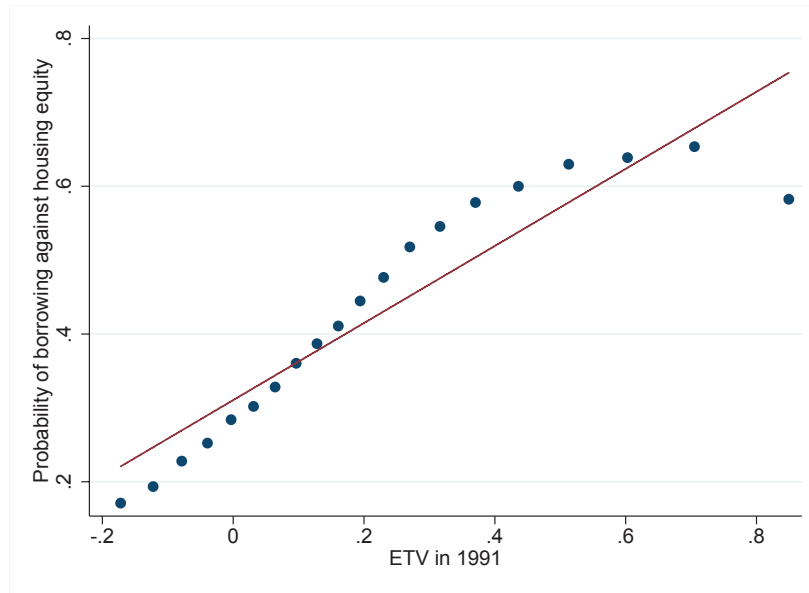
(a) Probability of equity extraction
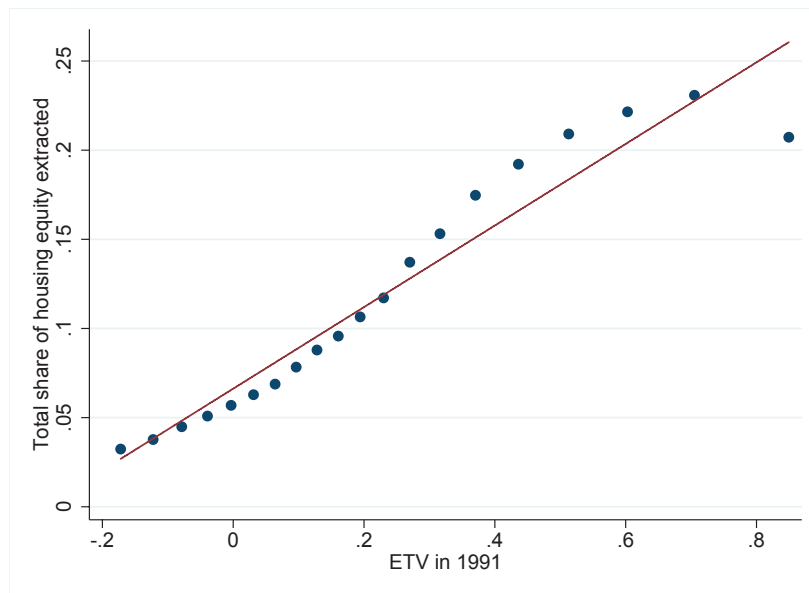
(b) Fraction of housing equity extracted

Notes: This figure shows the binned scatter of the probability of equity extraction and the share of housing equity extracted over the five-year period of 1992-1996 against the equity-to-value (ETV) ratio in 1991. Each dot contains the same number of individuals. The share of housing equity extracted is calculated as the amount of increase in outstanding mortgage debt normalized by the average housing price over the one year period, and we sum up all the shares for years 1992-1996.

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Figure 3: Timing of housing purchase and ETV in 1991
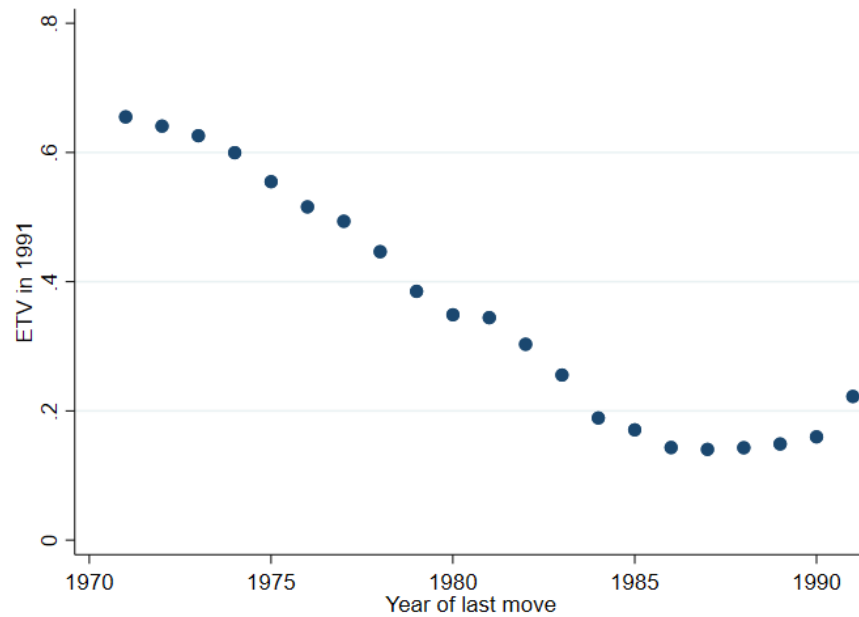
Notes: This figure plots the average ETV in 1991 for each year of housing purchase. The time of housing purchase is the most recent year that the person has moved address prior to 1991. The plot shows the first stage of the IV regression where ETV in 1991 is instrumented by timing of house purchase.

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Figure 4: Effects of reform on wages over time
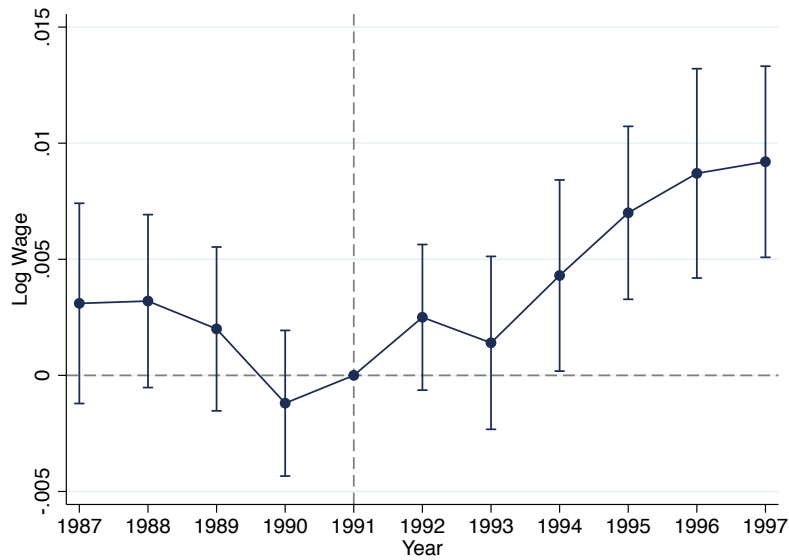
(a) All workers
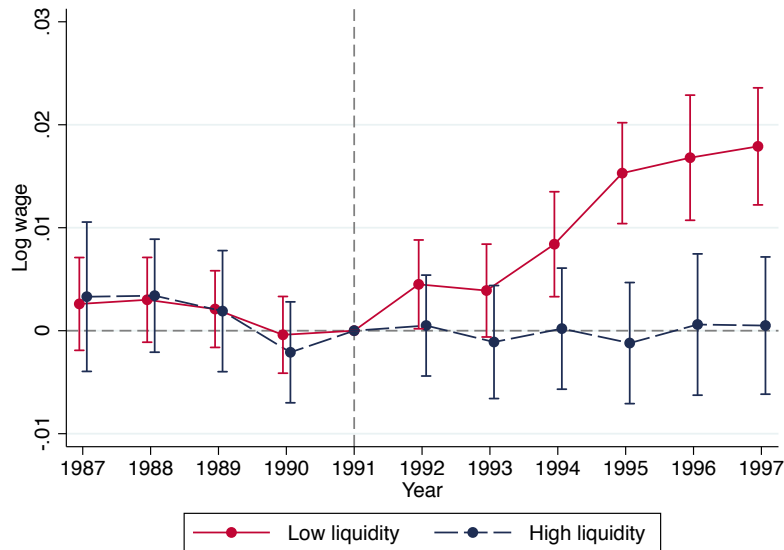
(b) By level of liquid assets

Notes: This figure shows the dynamic treatment effects of the mortgage reform on earnings of individuals with ETVs higher than 0.25 in 1991 over time, i.e. coefficients β_τ in equation (8). The dependent variable is log wage. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level. The bottom figure plots the treatment effects for low-liquidity individuals (individuals with liquid assets less than one month's disposable income in 1991) and high-liquidity individuals respectively.

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Table 1 Summary Statistics

	All home owners			ETV<0.25	ETV>0.25
	Mean	Median	Std. Dev.		
Age	40.1	41.0	8.0	37.3	43.2
Female	0.34			0.39	0.30
Kids	0.66			0.67	0.66
Married	0.70			0.68	0.72
Basic education	0.30			0.30	0.30
Vocational training	0.44			0.43	0.45
College education	0.26			0.27	0.25
Experience	16.2	16.0	7.8	14.4	18.2
Annual wage (1000 DKK)	197.7	198.2	128.6	198.1	197.2
Hourly wage	133.8	130.0	87.3	134.0	133.6
Unemployment in 1991	0.10			0.09	0.11
Housing price in 1991 (1000 DKK)	411.0	355.9	230.6	368.0	458.3
Total asset in 1991 (1000 DKK)	525.0	410.1	1418	455.8	600.9
Liquid asset in 1991 (1000 DKK)	92.0	13.9	1320	70.0	116.1
Total liability in 1991 (1000 DKK)	380.8	312.8	742.3	452.5	302.0
Mortgage debt in 1991 (1000 DKK)	269.8	234.2	192.4	339.1	193.9
Bank debt in 1991 (1000 DKK)	81.3	38.4	624.1	80.9	81.7
Potential housing equity unlocked in 1991 (1000 DKK)	78.5	9.4	127.7	1.2	163.4
ETV IN 1991	0.30	0.25	0.30	0.05	0.56
Number of observations	8,382,429			4,383,379	3,999,050
Number of people	762,039			398,489	363,550

Notes: This table reports the summary statistics for our baseline sample of homeowners. Worker level information are from the income register and is available for the entire sample period (1987-1997). All monetary values are normalized to real 2010 Danish krones. All ages refer to the age of an individual as of November within a given year. The classification of education groups relies on a Danish education code that corresponds to the International Standard Classification of Education (ISCED). "Higher education" basically corresponds to the two highest categories (5 and 6) in the ISCED; i.e., the individual has a tertiary education. "Vocational education" is defined as the final stage of secondary education encompassing programs that prepare students for direct entry into the labor market. Workers with just a high school or equivalent education or less than that are classified as "basic education". Housing assets refer to the tax assessed valuation of the individual's property scaled with the ratio of market prices to tax assessed house values for house that have been traded in that municipality and year. Non housing assets include the individual's other assets including stocks, bonds and bank deposits. All medians are calculated as the average value of 10 observations around the median.

Table 2 Effects of Mortgage Reform on Borrowing

Dependent variable	(1) Equity Extraction	(2) Fraction of equity extracted	(3) Liability/ Income	(4) Equity Extraction	(5) Fraction of equity extracted	(6) Liability/ Income
	A. Treatment: Dummy for (ETV91>0.25)					
Post*1(ETV91>0.25)	0.0684***	0.0219***	0.1325***	0.0510***	0.0177***	0.1039***
	(0.0014)	(0.0004)	(0.0078)	(0.0012)	(0.0003)	(0.0106)
Post*1(ETV91>0.25) * Low Liquidity				0.0382***	0.0093***	0.0604***
				(0.0011)	(0.0002)	(0.0092)
	B. Treatment: ETV91					
Post*ETV91	0.1227***	0.0395***	0.3711***	0.0890***	0.0309***	0.3253***
	(0.0032)	(0.0009)	(0.0158)	(0.0024)	(0.0007)	(0.0199)
Post*ETV91* Low Liquidity				0.0806***	0.0207***	0.1059***
				(0.0024)	(0.0006)	(0.0154)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	8,531,288	8,531,288	8,133,236	8,531,288	8,531,288	8,133,236

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions. Equity extraction is defined as in Bhutta and Keys (2016). The share of housing equity extracted is calculated as the amount of increase in outstanding mortgage debt normalized by the average housing price over the one year period. Liabilities include mortgage debt, bank debt, secured debt, and other debt. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

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Table 3 Effects of Mortgage Reform on Wages and Employment

Dependent variable	(1) Log wage	(2) Normalized earnings	(3) Employment rate	(4) Log wage	(5) Normalized earnings	(6) Employment rate
A. Treatment: Dummy for (ETV91>0.25)						
Post*1(ETV91>0.25)	0.0096***	0.0095***	0.0008	-0.0016	0.0012	0.0027***
	(0.0013)	(0.0025)	(0.0006)	(0.0022)	(0.0041)	(0.0009)
Post*1(ETV91>0.25) * Low Liquidity				0.0229***	0.0174***	-0.0038***
				(0.0031)	(0.0046)	(0.0010)
B. Treatment: ETV91						
Post*ETV91	0.0230***	0.0233***	0.0025**	0.0017	0.0109	0.0046***
	(0.0023)	(0.0047)	(0.0011)	(0.0037)	(0.0071)	(0.0015)
Post*ETV91* Low Liquidity				0.0453***	0.0268***	-0.0047***
				(0.0054)	(0.0073)	(0.0017)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,595,214	8,200,343	8,531,288	7,595,214	8,200,343	8,531,288

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions. Normalized earnings are annual earnings divided by the average annual earnings from 1988 to 1996, which takes into account individuals with zero earnings. Employment rate is an indicator variable which equals one if the wage income is positive. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

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Table 4 IV Effects of Mortgage Reform

	(1)	(2)	(3)	(4)	(5)	(6)

A. First Stage

Dependent variable: Post*1(ETV91>0.25)

	(1)					
Post*1(move before 1981)	0.4311***					
	(0.0013)					

B. Diff in Diff

Dependent variable	Equity Extraction	Fraction of equity extracted	Liability/ Income	Log wage	Normalized earnings	Employment rate
Post*1(ETV91>0.25)	0.0728***	0.0241***	0.1158***	0.0121***	0.0159***	0.0024*
	(0.0010)	(0.0002)	(0.0088)	(0.0033)	(0.0057)	(0.0014)

C. Triple Diff

Dependent variable	Equity Extraction	Fraction of equity extracted	Liability/ Income	Log wage	Normalized earnings	Employment rate
Post*1(ETV91>0.25)	0.0576***	0.0194***	0.0261***	-0.0034	0.0028	0.0056***
	(0.0011)	(0.0003)	(0.0118)	(0.0041)	(0.0079)	(0.0018)
Post*1(ETV91>0.25)* Low Liquidity	0.0411***	0.0108***	0.1787***	0.0308***	0.0262***	-0.0066***
	(0.0014)	(0.0004)	(0.0122)	(0.0046)	(0.0080)	(0.0021)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	8,531,288	8,531,288	8,133,236	7,595,214	8,200,343	8,531,288

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from IV regressions, where the instrument is a dummy variable indicating whether the closing date is after 1981 interacted with the post-1992 dummy. Dependent variables are as defined in Table 2 and Table 3. The endogenous variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

Table 5 Heterogeneity of Wage Effects by Individual Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: log wage						
	Basic Education	Vocational Education	Higher Education	Male	Female	Age<40	Age>=40
Post*1(ETV91>0.25)	0.0002	0.0045*	-0.0100**	0.0004	-0.0039	-0.0037	0.0028
	(0.0037)	(0.0026)	(0.0041)	(0.0025)	(0.0035)	(0.0023)	(0.0026)
Post*1(ETV91>0.25) * Low Liquidity	0.0267***	0.0092***	0.0309***	0.0177***	0.0286***	0.0290***	0.0097***
	(0.0048)	(0.0028)	(0.0053)	(0.0035)	(0.0048)	(0.0032)	(0.0032)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	2,179,945	3,344,750	2,070,519	4,991,633	2,603,581	3,908,326	3,686,888

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions for each demographic group. Normalized earnings are annual earnings divided by the average annual earnings from 1988 to 1996, which takes into account individuals with zero earnings. All demographic characteristics are measured in 1991. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

Table 6 Determinates of Equity Extraction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Outcome variable is Extract={0,1}							
Income growth	-0.0278***	-0.0118***						
	(0.0009)	(0.0010)						
Unemployment			0.0402***	0.0471***				
			(0.0028)	(0.0040)				
Mass layoff					0.0086***	0.0041**		
					(0.0016)	(0.0018)		
Firm wage change							-0.0097***	-0.0046*
							(0.0019)	(0.0024)
Municipality*Year FE								
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	No	Yes	No	Yes	No	Yes	No	Yes
No. of observations	1,673,245	1,673,245	1,685,518	1,685,518	1,380,262	1,380,262	1,331,894	1,331,894

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from regressions on propensity to borrow against housing equity. The dependent variable is an indicator variable for extracting housing equity. Income growth is year-to-year percent wage change. Unemployment is a indicator variable for whether the individual has zero labor earnings. Mass layoff is an indicator variable that equals one if the employer reduces number of workers by over 30% over a one-year period. Firm wage change is average percent wage change for all incumbent workers. The regressions control for municipality-year fixed effects, and in columns 5 and 6 also control for individual fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

Table 7 Effects of Mortgage Reform on Unemployed Workers

Dependent variable	(1) Unemployment duration	(2) Unemployment duration	(3) Hazard rate	(4) Hazard rate	(5) Wage change between jobs	(6) Wage change between jobs
1(ETV91>0.25)	0.1363***	0.0632	-0.0972***	-0.0841***	0.0104	-0.0193**
	(0.0318)	(0.0487)	(0.0106)	(0.0156)	(0.0065)	(0.0078)
1(ETV91>0.25)* Low Liquidity		0.1192**		-0.0591***		0.0571***
		(0.0523)		(0.0207)		(0.0080)
Pre-unemployment wage	Yes	Yes	Yes	Yes	Yes	Yes
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	42,780	42,780	42,780	42,780	42,780	42,780

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from cross-sectional regressions on unemployed workers in 1991. Unemployment duration is measured in years. The wage change between jobs is the change in log wage between pre- and post-unemployment jobs. Columns 3 and 4 estimates a Cox proportional hazard model. The main right-hand-side variables are equity to value ratio in 1991 and ETV interacted an indicator for having liquid assets less than one month's disposable income in 1991. All regressions control for fixed effects of age, municipality and year of beginning unemployment, as well as the log wage before unemployment.

Table 8 Effect on Employed Workers

Dependent variable	(1) Log wage	(2) Normalized earnings	(3) Employment rate	(4) Log wage	(5) Normalized earnings	(6) Employment rate
Post*1(ETV91>0.25)	0.0076***	0.0067***	0.0006	-0.0031	-0.0028	0.0013*
	(0.0013)	(0.0013)	(0.0005)	(0.0023)	(0.0022)	(0.0007)
Post*1(ETV91>0.25)* Low Liquidity				0.0218***	0.0196***	-0.0015*
				(0.0032)	(0.0027)	(0.0008)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,024,978	7,629,624	7,629,624	7,024,978	7,629,624	7,629,624

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions on the sample of homeowners who were employed at the time of the reform in 1992. Normalized earnings are annual earnings divided by the average annual earnings from 1988 to 1996, which takes into account individuals with zero earnings. Employment rate is an indicator variable which equals one if the wage income is positive. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

Table 9 Effects of Mortgage Reform on Job Switching

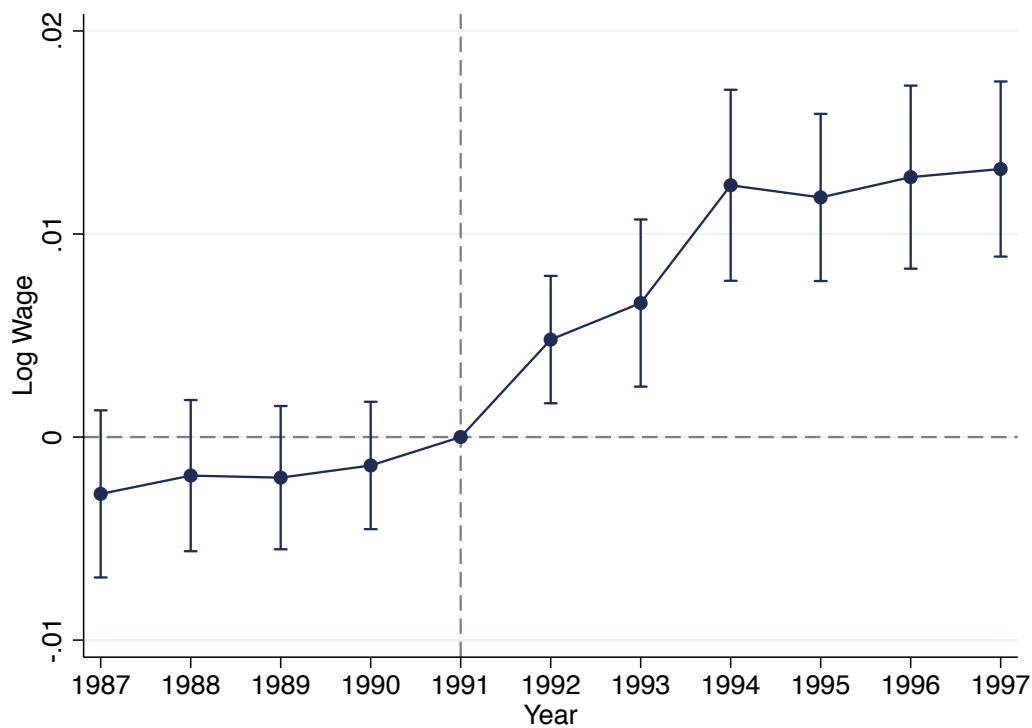| Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
	Switch firm	Move up	Move down	Coworker average wage	AKM establishment FE	Occupation average wage
Post*1(ETV91>0.25)	0.0006	0.0003	0.0003	0.0002	0.0001	-0.0015*
	(0.0008)	(0.0007)	(0.0005)	(0.0007)	(0.0006)	(0.0008)
Post*1(ETV91>0.25)* Low Liquidity	0.0054***	0.0079***	-0.0025***	0.0043***	0.0034***	0.0018
	(0.0012)	(0.0010)	(0.0007)	(0.0008)	(0.0008)	(0.0011)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	6,784,951	6,784,951	6,784,951	6,546,181	6,782,183	6,523,213

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions. In column 1 dependent variable is an indicator variable for changing employer. In column 2 dependent variable is an indicator variable for changing employer and getting higher wages. In column 3 dependent variable is an indicator variable for changing employer and getting lower wages. In column 4 dependent variable is average wage of coworkers. In column 5 dependent variable is the AKM establishment fixed effect of the employer, which is estimated from two-way fixed effect regressions with worker FE and establishment FE as in He and le Maire (2019). The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

Figure A1: Macroeconomic conditions around the reform

Figure A2: Wage changes of homeowners buying before 1981 relative to homeowners buying after 1981

Notes: This figure shows the dynamic treatment effects of whether housing purchase if before 1981 on the earnings of individuals over time. The dependent variable is log wage. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level.

Figure A3: Share of workers experiencing a 20 percent income shock
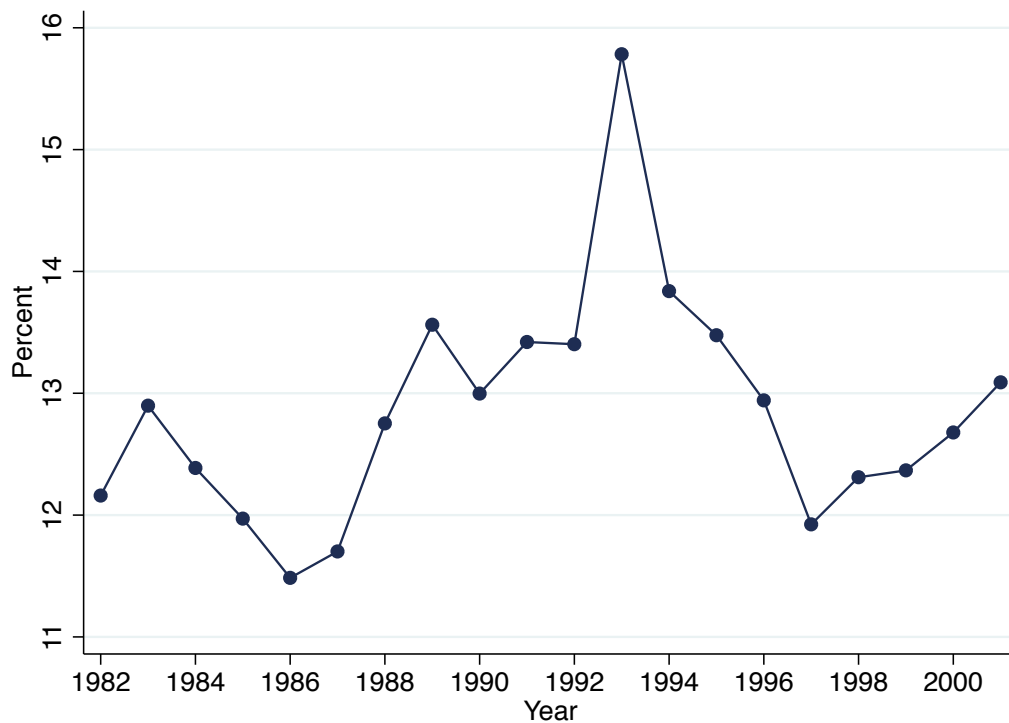
Notes: This figure plots the share of workers in our sample of homeowners who experience an at least 20% reduction in wage income compared to last year. The series is from 1981 to 2001, with a peak in year 1993. Labor income is measured in the last week of November each year.

Figure A4: Regions of Denmark

Notes: The five Regions of Denmark were created as part of the 2007 Danish Municipal Reform, when the counties were abolished. Each region is close to a commuting zone in the United States: it mostly takes less than two hours to travel between places within a region.

Table A1 Robustness of Results to Industry- and Occupation-specific Trends

Dependent variable	(1) Equity Extraction	(2) Fraction of equity extracted	(3) Liability/ Income	(4) Log wage	(5) Normalized earnings	(6) Employment rate
Post*1(ETV91>0.25)	0.0475***	0.0174***	0.1348***	-0.0036*	0.0006	0.0025***
	(0.0007)	(0.0002)	(0.0050)	(0.0019)	(0.0027)	(0.0006)
Post*1(ETV91>0.25)* Low Liquidity	0.0326***	0.0084***	0.0506***	0.0205***	0.0126***	-0.0041***
	(0.0010)	(0.0003)	(0.0061)	(0.0024)	(0.0032)	(0.0007)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Income decile*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,081,925	7,081,925	6,915,916	6,798,979	6,949,456	7,081,925

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions. Dependent variables are as defined in Table 2 and Table 3. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include all the baseline controls plus industry*year fixed effects, occupation*year fixed effects and 1991 income decile*year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

Table A2 Robustness to Linear Pre-trend

Dependent variable	(1) Equity Extraction	(2) Fraction of equity extracted	(3) Liability/ Income	(4) Log wage	(5) Normalized earnings	(6) Employment rate
Post*1(ETV91>0.25)	0.0625***	0.0248***	0.0985***	0.0009	-0.0007	-0.0004
	(0.0015)	(0.0004)	(0.0066)	(0.0023)	(0.0030)	(0.0008)
Post*1(ETV91>0.25)* Low Liquidity	0.0326***	0.0127***	0.0535***	0.0214***	0.0161***	-0.0029***
	(0.0014)	(0.0004)	(0.0075)	(0.0028)	(0.0033)	(0.0011)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Linear pre-trend	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	8,531,288	8,531,288	8,133,236	7,595,214	8,200,343	8,531,288

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions. Dependent variables are as defined in Table 2 and Table 3. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include all the baseline controls plus a linear term in calendar year. Standard errors are clustered at the municipality level and are reported in parentheses.

Table A3 Placebo Test Using Years 1982-1990

Dependent variable	(1) Log wage	(2) Normalized earnings	(3) Employment rate	(4) Log wage	(5) Normalized earnings	(6) Employment rate
Post*1(ETV85>0.25)	0.0013	-0.0015	-0.0010	-0.0003	-0.0022	-0.0011
	(0.0016)	(0.0042)	(0.0006)	(0.0019)	(0.0047)	(0.0007)
Post*1(ETV85>0.25) * Low Liquidity				0.0032	0.0004	-0.0004
				(0.0024)	(0.0065)	(0.0009)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	2,951,875	3,003,968	3,050,184	2,951,875	3,003,968	3,050,184

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from placebo OLS regressions for the pre-reform period (1982-1990). Normalized earnings are annual earnings divided by the average annual earnings from 1982 to 1985, which takes into account individuals with zero earnings. Employment rate is an indicator variable which equals one if the wage income is positive. The main right-hand-side variables are equity to value ratio in 1985, ETV interacted with an indicator for the post-1986 period, and interactions of ETV, post-1986 dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1985. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1985, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

Table A4 Alternative Measures of Liquidity Constraint

Dependent variable	(1) Equity Extraction	(2) Fraction of equity extracted	(3) Liability/ Income	(4) Log wage	(5) Normalized earnings	(6) Employment rate
			A. Excluding Stocks and Bonds in Liquid Assets			
Post*1(ETV91>0.25)	0.0481***	0.0169***	0.0884***	-0.0021	0.0015	0.0029***
	(0.0012)	(0.0003)	(0.0116)	(0.0024)	(0.0040)	(0.0008)
Post*1(ETV91>0.25)* Low Liquidity	0.0428***	0.0105***	0.0885***	0.0208***	0.0152***	-0.0043***
	(0.0011)	(0.0002)	(0.0104)	(0.0031)	(0.0043)	(0.0009)
			B. Hand to Mouth			
Post*1(ETV91>0.25)	0.0437***	0.0156***	0.0552***	-0.0009	0.0017	0.0015**
	(0.0010)	(0.0003)	(0.0094)	(0.0017)	(0.0031)	(0.0007)
Post*1(ETV91>0.25)* Low Liquidity	0.0688***	0.0179***	0.1631***	0.0172***	0.0159***	-0.0022**
	(0.0010)	(0.0003)	(0.0081)	(0.0030)	(0.0038)	(0.0009)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	8,531,288	8,531,288	8,133,236	7,595,214	8,200,343	8,531,288

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions. Dependent variables are as defined in Table 2 and Table 3. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable for low liquidity. In Panel A, the indicator for low liquidity equals one if liquid assets (excluding stocks and bonds) is less than one month's income. In Panel B, the indicator for low liquidity equals one if the individual is hand-to-mouth in 1991. Hand-to-mouth is defined in Kaplan, Violante and Weidner (2014) as well as Section 5.5.2. Control variables are the baseline controls in Table 2 and 3. Standard errors are clustered at the municipality level and are reported in parentheses.

Table A5 Robustness to Self Employment

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
	Log wage	Normalized earnings	Employment rate	Log wage	Normalized earnings	Employment rate
Post*1(ETV91>0.25)	0.0074***	0.0045**	-0.0013**	-0.0040*	-0.0061*	-0.004
	(0.0013)	(0.0022)	(0.0005)	(0.0022)	(0.0032)	(0.0007)
Post*1(ETV91>0.25)* Low Liquidity				0.0246***	0.0236***	-0.0019**
				(0.0030)	(0.0035)	(0.0008)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,271,464	7,721,892	7,886,215	7,271,464	7,721,892	7,886,215

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions. In all columns we exclude individuals who were entrepreneurs at any time during 1987-1997. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include year fixed effects interacted with fixed effects for birth-cohort, decile of wealth, educational level, partner, gender and having children, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.

Table A6 Effects on Home Owners with Non-negative ETVs

Dependent variable	(1) Equity Extraction	(2) Fraction of equity extracted	(3) Liability/ Income	(4) Log wage	(5) Normalized earnings	(6) Employment rate
Post*1(ETV91>0.25)	0.0472***	0.0167***	0.0786***	-0.0026	0.0002	0.0028***
	(0.0006)	(0.0001)	(0.0062)	(0.0024)	(0.0041)	(0.0010)
Post*1(ETV91>0.25)* Low Liquidity	0.0331***	0.0082***	0.0573***	0.0208***	0.0166***	-0.0036***
	(0.0008)	(0.0002)	(0.0071)	(0.0031)	(0.0050)	(0.0012)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	6,880,654	6,880,654	6,534,280	6,096,236	6,590,897	6,880,654

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions for the sample of homeowners who have positive ETVs in every year between 1987 and 1997. Dependent variables are as defined in Table 2 and Table 3. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables are the baseline controls in Table 2 and 3. Standard errors are clustered at the municipality level and are reported in parentheses.

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Table A7 Effects of Mortgage Reform on Wages Within Job Spells

	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	College educated	College educated	Non college educated	Non college educated
Post*1(ETV86>0.25)	-0.0015	-0.0024	-0.0040**	-0.0055*	0.0001	0.0006
	(0.0010)	(0.0016)	(0.0018)	(0.0030)	(0.0011)	(0.0016)
Post*1(ETV86>0.25)* Low Liquidity		0.0023		0.0069**		-0.0014
		(0.0017)		(0.0035)		(0.0018)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Person*establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observables*year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	6,770,847	6,770,847	1,878,509	1,878,509	4,892,338	4,892,338

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions. Dependent variable is log wage. Columns 1 and 2 include all individuals in the baseline sample, columns 3 and 4 include only individuals with a college degree, and columns 5 and 6 include only individuals without a college degree. The main right-hand-side variables are equity to value ratio in 1991, ETV interacted with an indicator for the post mortgage reform period, and interactions of ETV, post-reform-period dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1991. Control variables include all the baseline controls plus establishment*person fixed effects. Standard errors are clustered at the municipality level and are reported in parentheses.