How Does Liquidity Constraint Affect Employment and Wages?
Evidence from Danish Mortgage Reform[*]

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October 23, 2018

Abstract

This paper studies the effects of liquidity constraints on employment and earnings 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 liquidity-constrained homeowners extracted housing equity, increased debt levels and experienced higher earnings growth after the reform. In contrast, the reform had little impact on employment and earnings of homeowners with high liquid asset holdings. Consistent with models of job search with risk aversion, the option to borrow against housing equity allows individuals to seek jobs that have higher earnings growth but higher unemployment risks. This effect is larger for low-income and older individuals. The results imply that relaxing liquidity constraints can increase output, and policies restricting mortgage refinancing during economic distress may backfire in recessions.

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

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1 Introduction

A large 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 the types of jobs that individuals are willing to take. Recent works show that liquidity constraints can affect individuals' job search behavior (Herkenhoff, Phillips and Cohen-Cole 2016a; Kaplan 2012; Ji 2018) and mobility across occupations and locations (Hawkins and Mustre-del-Rio 2016; Brown and Matsa 2017). In this paper, we exploit a unique mortgage reform in Denmark to provide causal estimates of the effects of liquidity constraints on employment and earnings.

Estimating the effects of liquidity constraints is challenging since the assets and earnings are both endogenously determined. Even studies using exogenous variations often have modest effects on the amount of credit access, or have confounding effects that makes it hard to isolate the effects of liquidity constraints. For example, credit reports also affect the credit checks and therefore employment opportunities (Dobbie et al. 2016; Herkenhoff, Phillips and Cohen-Cole 2016b). Debt relief programs and changes in housing prices affect both short-run liquidity constraints and long-run debt overhang. Therefore many studies rely on structural models to quantify the effects of liquidity constraints (Kaplan 2012; Herkenhoff, Phillips and Cohen-Cole 2016a; Ji 2018).

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

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[1] An additional 19 percent 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 around whether policies that replenish the liquid balances of households, such as reductions in mortgage payments that are concentrated in the periods of the crisis, would be more effective than debt write-downs that reduce mortgage payments over the entire duration of the mortgage contract (Ganong and Noel 2017; Dobbie and Song 2018). 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 2018).

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 did not exist 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 home owners, even after controlling for detailed life-cycle and demographic characteristics. That is, home owners 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 home owners who bought their homes well before the reform. We then combine the household balance sheets data with detailed matched employer-employee data to study the impact of the expanded credit access on employment and earnings.

We find that the reform led to more housing equity extraction and higher debt levels for individuals with more housing equity, and individuals with more housing equity experienced faster wage and earnings growth after 1992. Individuals with equity to value ratio (ETV) higher than 0.2[3] in 1991 experienced an increase in debt of 6% of annual income and a 0.7% increase in earnings after the reform compared to individuals with ETV lower than 0.2 in 1991.

To isolate the reform's effects of relaxing liquidity constraints, we compare the effects on individuals with liquid assets[4] less than one month's disposable income in 1991, and individuals with more liquid assets in 1991. While liquidity-constrained individuals with ETVs higher than 0.2 experienced an increase in debt levels by 13% of annual income and an increase in earnings by 1.6% following the reform, non-liquidity-constrained individuals with ETVs higher than 0.2 only experienced an increase in debt levels by 4% of annual income and an increase in earnings by 0.1%. Furthermore, among individuals affected by the reform, the employment rate of liquidity-constrained individuals declined after the reform, while the employment rate of non-liquidity-constrained increased slightly after the reform. These results suggest that relaxation of liquidity constraint allowed liquidity-constrained individuals to seek jobs that offer higher wages but also

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[3]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.

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

higher unemployment risks. On the other hand, the reform had little impact on the wages of non-liquidity-constrained households since they mostly substituted other forms of debt with housing debt.

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 conditional on observed characteristics, including demographics, total wealth, industry and location. 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. We also conduct a placebo test using data before the reform, and show that individuals with more housing equity in 1989 and less housing equity in 1989 had similar wage growth rates during the period 1990-1992 when controlling for the observed characteristics in 1989.

Why does more credit access lead to higher earnings? We show that the ability to borrow against housing equity can increases wage through two channels. First, risk-averse workers prefer jobs that pay higher wages but have higher unemployment risks when they are able to borrow to insure against unemployment risks (Acemoglu and Shimer 1999; Kaplan 2012). Second, additional credit access increases the value of unemployment, which allows workers to bargain for higher wages.

We first examine how housing equity extractions interact with unemployment risks. We find that workers who recently become unemployed and experience negative earnings 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. Unemployed workers who have access to housing equity stay in unemployment for longer, and get higher wages when re-entering employment.

We then test the bargaining and sorting channels directly and find evidence supporting both channels. We find that after the reform liquidity-constrained individuals with positive housing equity are more likely to switch jobs and switch cities. The AKM firm fixed effect and average wage of coworkers increase, as well as the probability of being in a top position, suggesting that workers are moving to better firms and better job positions. On the other hand, consistent with the bargaining channel, workers with access to housing equity also experienced higher wage growth

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within job spells after the reform.

Our paper is closest to Herkenhoff et al. (2016a), who shows that more consumer credit access leads to longer unemployment durations and higher reemployment wages. We find similar effects for unemployed workers, but we also find that the option to borrow from housing equity allows employed workers to switch to more highly-paid jobs and bargain for higher wages. We also highlight the heterogeneity by the level of liquid assets – for individuals with little liquid assets, the additional credit leads to higher wages, but for individuals with a lot of liquid assets, the additional credit has little impact on wages and even reduced reemployment wages for unemployed workers. Compared to consumer loans, the mortgage reform also has much larger impact on the amount of credit access – 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 percent of the households in our sample.

Our paper is also related to previous literature on how unemployment benefits and payday loans affect employment and wages. The unexpected credit access provided by the mortgage reform combined with the amount of liquid assets before the reform allow us to isolate the effect of relaxing liquidity constraint on wages and employment. However, changes in unemployment benefits 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 rate on home equity loan in Denmark is 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 loan and high-interest payday loans highlights the importance of the contractual form of credit policies intended to alleviate liquidity constraints (Zingales 2015).

Finally, our paper relates to two previous papers that study the impact of the 1992 Danish mortgage reform on labor market. Jensen, Leth-Petersen and Nanda (2015) finds that access to housing equity increases entrepreneurship. We replicated the positive effect on entrepreneurship rates, but we show that the effect is too small to explain our wage effects. We also find similar results for employment and wages when excluding all self-employed workers. Markwardt et al.

(2014) finds that the home equity loans partially substitute for unemployment benefits. We show that even though the level of unemployment benefit is high in Denmark, the additional credit offered by the mortgage reform still has large positive effect on wages, which implies that the effect may be even larger in countries with less generous unemployment benefits.

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

2   The 1992 Mortgage Reform in Denmark

The Danish mortgage reform took effect on 21 May 1992. The most important element of the reform is that it enabled home owners, for the first time, to borrow against their home for purposes other than financing the underlying property. Until 2007, mortgage debt in Denmark was provided exclusively through mortgage banks, which are financial intermediaries specialized in the provision of mortgage loans. The May 1992 bill introduced a limit of 60% of the house value for loans for non-housing purposes. This limit was extended to 80% in December 1992. The granting of loans is solely on the basis of the value of housing collateral, which is not true for loans from commercial banks.[5] In other words, the reform allowed individuals with housing collateral who could not previously obtain loans through commercial banks to now get access to credit through mortgage banks. Another feature of the reform is that the maximum maturity of mortgage loans was expanded from 20 to 30 years. For people who were already mortgaged to the limit prior

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[5]When granting a mortgage loan for a home in Denmark, the mortgage bank issued bonds that directly matched the repayment profile and maturity of the loan granted. The bonds were sold on the stock exchange to investors and the proceeds from the sale are paid out to the borrower. Once the bank had screened potential borrowers based on the valuation of their property and on their ability to service the loan, 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 at loan origination. In addition, each plot of land in Denmark has a unique identification number, the title number, to which all relevant information about owners and collateralized debt is recorded in a public title number registration system. Mortgage loans have priority over any other loan and the system therefore secures optimum coverage for the mortgage bank in case of default and enforced sale. Creditors can enforce their rights and demand a sale if debtors cannot pay. Furthermore, mortgage banks accumulate a buffer through contributions from all borrowers, and they use this buffer to cover loans 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.

to the reform, and who therefore could not establish additional mortgage loans for non-housing consumption, this option provided the possibility of acquiring more liquidity.

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 identification strategy since individuals have little time to strategically take advantage of the reform. The reform was introduced during the 1992 recession and implemented was right before the Danish economy started to grow rapidly, so the lessons from this reform may shed light on other similar policies during recoveries.

Another element of the reform is the option to refinance. Refinancing makes it possible for borrowers to lower the cost of the loan when the market interest rate falls. This enables the borrower to exploit changes in the market rate of interest in order to reduce the costs of funding. While the other two parts of the reform influence the access to credit, this part of the reform provides 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.

In this paper we focus on the the first two elements of the reform which provided home owners 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 percent 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 percent 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 shortfall 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.

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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.

3 Conceptual Framework

We consider a simple theoretical framework similar to Acemoglu and Shimer (2000). For risk averse agents, a relaxation of credit access allows them to smooth consumption over time and increases the utility when unemployed. For simplicity, we consider a static model and study how increases in the utility of unemployment affect wages and employment.

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 g is an increasing function. However, a high α job is also harder to fill. 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 each 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:

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

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

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

A higher b has two effects on wages. First, it increases wages by raising workers' outside options. Given job type α, $w(\alpha;b)$ is increasing in b. This is because workers have a higher value of unemployment due to better consumption smoothing. Second, it increases wages by increasing the specificity of jobs α that workers search for. Since workers are better insured against unemployment, they are more willing to search for jobs that pay high wages but have lower probability of employment.

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In a dynamic setting, these two forces still exist. When workers are credit constrained, an increase in credit access due to the mortgage reform allows them to smooth consumption across time and therefore increase their value of unemployment. As a result, they are able to bargain for higher wages and switch to jobs that have higher earnings and earnings growth. As the same time, they also face greater unemployment risks. We will test these predictions in the following sections.

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 the reform.

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 1988 to 2000.

The first part of the dataset is regarding wealth and income of the households. The income and wealth information exists because Denmark had a wealth tax during this period. The data on assets and liabilities can be divided into a number of categories.[7] Assets are divided into six different categories: housing assets, shares, deposited mortgage deeds, cash holdings, bonds, and other assets. Housing assets are de ned 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 prices to tax assessed values. 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

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[7]The definitions of these categories are not stable across the observation period, and the level of detail decreases after 1992.

Danish taxpayers and are therefore considered to be very reliable. The remaining components are self-reported, but subject to being audited by the tax authorities.

The second part of the dataset is individuals' labor market history. 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). Among home owners, we focus on those who are between the age of 25 and 55 in 1991, to avoid interference from retirement decisions. Individuals who are living with their parents and those living in a communal or common household are omitted from the sample. To make sure results are not driven by sample attrition during the sample period, we keep individuals who are observed in every year from 1988 to 1996.[8] This leaves a balanced sample of 826,062 individuals.

4.2 Summary Statistics

Table 1 summarizes the statistics of variables on demographics, earnings, and balance sheets for all home owners in 1991. Housing equity constitutes the majority of assets for most of the home owners. The median individual has very little liquid assets: the median level liquid asset is about one tenth of average annual earnings. 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. For many households liquid asset holdings corresponding to one month's disposable household income thus amount to having virtually no liquid assets as a buffer.

On the right panel of Table 1 we compare households with equity to value (ETV) above 0.2

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[8]Since we observe people who are unemployed and out of labor force, the sample attrition is very small. Only less than 3% of the observations are dropped.

and households with ETV below 0.2 in 1991. The reform allowed individuals to borrow up to a maximum of 80% of the home value. Therefore individuals with ETV lower than 0.2 won't be able to extract any 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 79,000 DKK (about 13,000 USD) in housing equity. The average amount of housing equity unlocked for people with ETV below 0.2 is very little, while the average amount of housing equity unlocked for people with ETV above 0.2 is 164,000 DKK, which is close to one year's earning.

4.3 Identify 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 home owners in each year that 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 percent in total mortgage balance has risen sharply to over 5% per year. Between 1993 and 1996, the average fraction of home owners 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 monotonically 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 home owners. Figure 2 (b) (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-section 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 0.2. 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:

$$y_{it} = \beta Post_t \times \mathbf{1}(ETV_{91} > 0.2)_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it} \tag{3}$$

where y_{it} is the outcome for person i at year t, $Post_t \times \mathbf{1}(ETV_{91} > 0.2)$ equals one if person i had ETV greater than 0.2 in year 1991 and year t is 1992 or later. The key coefficient 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.

We include person fixed effects in all regressions. Standard errors are clustered at the individual

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,[9] the municipality of residence, and the industry the person works in. 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 industry and live in the same municipality, but one who bought the home some years before the other.

The 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.

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

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[9]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.

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

in 1991:

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

To isolate the effects of the reform on individuals' liquidity constraints, we compare the effects of the reform on individuals with high level of liquidity assets and low level of liquidity 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 1988 and 1991.[11] By this definition, almost 40% of all the individuals in our sample have low liquidity before the reform.

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

$$y_{it} = \beta Post_t \times \mathbf{1}(ETV_{91} > 0.2)_i + \gamma Post_t \times \mathbf{1}(ETV_{91} > 0.2)_i \times \text{LowLiquidity}_i +$$
$$\delta \text{LowLiquidity}_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it} \qquad (5)$$

where Lowliquidity$_i$ is an indicator for having less liquid assets than one month's disposable income in 1991. β is the effect of the reform on high-ETV group relative to 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 credit-constrained individuals relative to unconstrained individuals to the increased credit access.

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 in Section 5.4 using only years before the mortgage reform. We estimate the following specification:

$$y_{it} = \beta Post89_t \times ETV_{89,i} + \theta X_i^{1989} \times \phi_t + \alpha_i + \varepsilon_{it} \qquad (6)$$

where $Post89_t$ is an indicator for years after 1989, and $ETV_{89,i}$ is the ETV in 1989. If high-ETV

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[11]We also use an alternative measure of the maximum liquid asset to income ratio before 1992, 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.

individuals and low-ETV individuals differ in systematic ways in their unobserved characteristics, we would expect to see different trends in this pre-period even when borrowing against housing equity was not possible.

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 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 1988-1996. High-ETV individuals increased their debt level substantially after the reform: individuals with ETVs higher than 0.2 in 1991 increased their total debt level by 7.6% of their annual earnings than individuals with ETVs lower than 0.2 in 1991.

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 little 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. 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 lot 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 very little. For example, households with high liquidity and ETV higher than 0.2 only increased their total debt by 4% of annual earnings, while households with low liquidity and ETV higher than 0.2 increased their total debt

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by 13% of annual earnings. This suggests that equity extractions crowd out other sources of debt such as bank loans for non-liquidity-constrained households.

In Panel B of Table 2, we use the continuous measure of ETV in 1991 as the treatment variable (Equation 5) and get similar results. A one-standard-deviation increase in ETV of 1991 increases debt level by 8% of a annual salary. The effect on borrowing is twice larger for liquidity-constrained individuals than non-constrained individuals.

These results indicate that the reform indeed relaxed credit constraint for individuals' with high ETVs. For credit-constrained individuals, this increased the borrowing significantly; for non-credit-constrained individuals, the additional borrowing from housing equity crowds out other sources of borrowing and has small impact on the amount of total debt.

5.2   Effects of the Reform on Wages and Employment

How does the relaxation of credit constraint affect wages and employment? Table 3 shows results from our baseline regressions using measures of wages and employment as dependent variables. In Column 1, we use normalized earnings as dependent variable where we divide annual earnings by the average annual earnings from 1988 to 1996.[12] This measure takes into account individuals with zero earnings. Following the reform, individuals with ETVs higher than 0.2 experienced a 0.7% gain in earnings, and individuals with ETVs between 0.4 and 0.6 experienced a 0.6% gain in earnings. In Column 2, we use log annual wage as dependent variable and thus excludes individuals with zero earnings and get similar results: earnings increased by 0.4% for individuals with ETVs higher than 0.2 in 1991. In Column 3, the dependent variable is employment rate, which equals to one if the individual has positive earnings and zero otherwise. The employment rate of high-ETV groups increased by 0.8%, but the difference is not statistically significant.

Column 4 to 6 Table 3 present results for triple-differences specification (equation 5). For credit-constrained individuals, an ETV of greater than 0.2 leads to an 1.6% increase in earnings . On the other hand, for non-constrained individuals, a higher ETV is not associated with significantly higher earnings after the reform. Nevertheless, the employment rates of liquidity-constrained individuals fell after the reform, while employment rates of non-liquidity-constrained individuals

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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 (1988-1991).

increased after the reform. 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 Panel B of Table 3, we use continuous ETV as the treatment variable. A one-standard-deviation increase in ETV in 1991 increases earnings by 0.4 percent on average, and increases earnings by 0.8 percent for liquidity-constrained individuals.

How big is this effect? The estimates in Column 4 indicates that the earnings of liquidity-constrained individuals with ETVs higher than 0.2 increase by 1.6% 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 1.6% earnings increase implies a loss in present discounted value equal to 20% of annual earning, which is larger than the increase in amount of borrowing by these individuals (13% of annual earning from Column 6 of Table 2).

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

$$y_{it} = \alpha_i + \sum_{\tau=1988}^{1996} \beta_\tau \mathbf{1}(ETV_{91} > 0.2)_i \times D_t(\tau) + \theta X_i^{1991} \times \phi_t + \varepsilon_{it} \tag{7}$$

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 3 plots the coefficients β_τ. The effects are insignificant from zero before 1991, and are increasing over time after 1991. We estimate the same regression separately for low-liquidity individuals and high-liquidity individuals and plot the coefficients in the bottom figure of Figure 3. For both groups, individuals with high ETVs have similar wage trends as individuals with low ETVs before 1991, which suggests 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 Heterogeneity

We examine the heterogeneity of treatment effects on wages by demographic characteristics in Table 4. Each column is a separate regression for all individuals in a demographic group, and the dependent variable is normalized earnings.

Column (1) to Column (3) show that workers with basic education benefited the most from the reform. Workers with only basic education who were liquidity-constrained and had high ETVs in 1991 experienced a wage increase of over 3%, while workers with vocational education and higher education have much smaller wage gains. This might be due to the fact that less skilled workers have higher income volatility.

Column (4) and Column (5) show that women have larger wage responses to the reform than men. The last two columns show that older workers experienced larger increases in earnings following the reform than younger workers. 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. However, in our setting older homeowners are more likely to have higher ETVs and more expensive houses, and therefore are more likely to benefit from the reform.

5.4 Robustness

5.4.1 Are the results driven by entrepreneurs?

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 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 found that homeowners with high ETVs in 1991 are more likely to become entrepreneurs.

Consistent with Jensen, Leth-Petersen and Nanda (2015), we find that individuals with high ETVs in 1991 have a 0.1% higher probability of becoming self-employed, and the effect is more pronounced for liquidity-constrained individuals. However, the effect on entrepreneurship rate is much smaller than the effect on earnings – for entrepreneurship to explain all of the increase in earnings, the earnings of the entrepreneurs would have to be 7 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 1996. Table 5 shows that after excluding entrepreneurs, we still find a similar earnings increase among individuals who had high ETVs in 1991 and were liquidity-constrained. Therefore increase in entrepreneurship cannot explain the positive effect of credit access on earnings.

5.4.2 Placebo Test Using Pre-Reform Years

The key identifying assumption of our empirical strategy is that individuals with high ETVs follow the same wage trends as individuals with low ETVs conditional on observable characteristics. We have shown that individuals with high ETVs in 1991 and individuals with low ETVs in 1991 have parallel wage trends before 1991. Nevertheless, it is still possible that individuals with higher ETVs in 1991 have different wage trends after 1991 for reasons other than the mortgage reform. For instance, individuals with higher ETVs have less debt and lower leverage, and previous studies have shown that debt overhang may affect labor supply and job search behavior (Bernstein 2018; Ji 2018).

To test this we perform a placebo test using data before 1992. We divide the period into a pre-period (1988-1989), and a post-period (1990-1991), and test whether individuals with higher ETVs in 1989 had higher wage growth in 1990 and 1991. Since the placebo sample is before the mortgage reform took place, we would not expect to see differential wage trends for individuals with high ETVs in 1989 since they wouldn't be able to extract their housing equity to finance their other needs. We apply the same difference-in-differences specification as Equation 3, and measure all observable characteristics at 1989.

Table 6 presents the results of the placebo test. Individuals with ETV higher than 0.2 in 1989 have similar trends in normalized earnings and log wages from 1990 to 1991 as individuals with ETV lower than 0.2 in 1989.[13] In Column (4) to Column (6), we compare the wage responses for liquidity-constrained and non-liquidity-constrained individuals based on their level of liquid

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[13]The positive effect of high ETV on subsequent earnings is consistent with the debt overhang effects (Ji 2018; Bernstein 2018). For example, individuals with more debt may have less incentive to work due to implicit taxes. However, such effects are small in our setting.

assets in 1988 and 1989. The coefficient of the interaction term between low liquid assets and high ETV is statistically insignificant from zero in all of these regressions, indicating that liquidity-constrained and non-liquidity-constrained individuals have nearly identical wage and employment responses to different levels of ETV in 1989. This suggests that liquidity-constrained individuals with high ETVs had faster wage growth after 1992 precisely because the reform relaxed their credit constraints.

6 Mechanisms

In this section we investigate the mechanisms of how expanding credit access leads to higher earnings. As shown in our conceptual framework, a relaxation of credit constraint increases the value of unemployment and allows individuals to choose jobs that are risker and have higher earnings, as well as bargain for higher wages at current 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 look at the job search behavior 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 behaviors and within-job-spell wage changes of all employed workers to examine the job search channel and the bargaining channel separately.

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) find 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[14]:

$$\text{Extract}_{ict} = \beta_1(\text{IncomeGrowth}_{it}) + \beta_2(\text{IncomeGrowth}_{it} \times \text{LowLiquidity}_i) + \gamma\mathbf{X}_i + \alpha_{ct} + \epsilon_{ict} \quad (8)$$

where Extract_{it} is an indicator variable for housing equity extraction, IncomeGrowth_{it} represents the average income growth rate over the past three years. We interact the income growth with an indicator variable for having low level of liquid assets in 1991 to study the different responses of high-liquidity and low-liquidity individuals. The vector $\mathbf{X}_i$ 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 1991.

Column (1) of Table 7 shows that individuals that experienced a negative earnings shock are more likely to borrow against housing equity. A standard deviation decrease in income growth leads to a 1.1 percentage point rise in equity extraction: a 10 percent increase relative to the 11 percent average extraction rate across all years after 1991. Households with little liquid assets have on average 3.4 percentage points higher extraction rate. Column (2) shows that liquidity-constrained individuals are also more likely to extract equity in response to negative earnings shocks: a standard deviation decrease in income growth leads to a 0.7 percentage points increase in equity extraction for individuals with sufficient liquid assets, and a 1.4 percentage points increase in equity extraction for individuals with little liquid assets.

In Columns (3) and (4) we examine how labor market shocks affect equity extraction. Workers who lost their jobs are more likely to extract equity. Workers are also more likely to extract equity when their employers have negative employment growth. To account for unobserved heterogeneity across homeowners in their propensity to extract equity that may be correlated with labor market outcomes, we include person fixed effects in Columns (5) and (6). For the same person, the timing of equity extraction is positively correlated with unemployment and negatively correlated with

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[14]The large dataset and large number of FEs raise challenges for a probit specification related to computation and interpretation. Furthermore, comparing the main results from our estimated linear probability model with the appropriate marginal effects (including accounting for the interaction term) from a probit model yielded virtually identical estimates.

shocks to earnings. These evidence indicate that homeowners borrow against their housing equity to insure against negative labor market shocks.

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 et al. (2016a) finds 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 at year 1991 and have different levels of housing equity. In particular, we estimate the following equation:

$$D_i = \gamma \mathbf{1}(ETV_{91} > 0.2)_i + \pi \mathbf{1}(ETV_{91} > 0.2)_i \times \text{LowLiquidity}_i + \beta \mathbf{X_i} + \varepsilon_i \qquad (9)$$

where D_i is the unemployment duration of individual i, control $\mathbf{X_i}$ include age dummies, 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 of liquidity-constrained individuals relative to non-liquidity-constrained individuals.

Table 8 shows that having positive housing equity on average increases unemployment durations by 0.07 years, or 3.7 weeks. Liquidity-constrained households increased unemployment durations by 0.18 years, or 9.1 weeks, while non-liquidity-constrained households increased their unemployment durations by 0.04 years, or 2.1 weeks.

Column 3 and Column 4 looks at how access to housing equity affects reemployment wages. The dependent variable is replacement rate, defined as reemployment wage divided by average wage in three years before the unemployment spell. 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 posi-

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tive housing equity experienced a 5% higher replacement rate, whereas non-constrained households with positive housing equity experienced a 2% lower replacement rate.

Our results are similar to Herkenhoff et al. (2016a), who finds 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 replacement rate by 6%. In addition, we show that the effect is heterogeneous by individuals' liquidity constraint: while more credit access allows liquidity-constrained individuals to search for jobs with higher wages, more credit access make non-liquidity-constrained individuals stay in unemployment for too long and hurt their reemployment wages.

6.3 Sources of Wage Growth Following the Reform

We investigate the mechanisms through which access to housing equity affects wages. As shown in the conceptual framework there are two channels. The first channel is sorting. Workers with access to housing assets are more selective and therefore are more likely to move to firms and jobs with higher wages. The second channel is bargaining. The ability to borrow in unemployment improves workers' outside options and allows them to bargain for higher wages given the job type. Both mechanisms are widely discussed in the theoretical literature, but direct empirical evidence to distinguish between the bargaining and sorting mechanisms is rare.

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 5. 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 0.2 are 1.5% more likely to switch jobs after the reform, while non-constrained individuals with ETV higher than 0.2 are only 0.2% more likely to switch jobs. Individuals are also more likely to move to new cities when they can borrow against their housing equity. The additional credit helps individuals cover their moving costs or finance down payments for their new homes. This is consistent with the view that liquidity constraints lock in people at their current locations (Brown and Matsa 2016).

The access to housing credit also allows people to move to better firms. We estimate two-way fixed effects model as in Abowd, Kramarz and Margolis (1999) for the period 1988-1996, and use

the estimated firm fixed effects as dependent variables to measure the firm-specific wage premium. Column (3) shows that workers with high ETVs move to firms that pay higher wages after the mortgage reform. Individuals with ETVs higher than 0.2 in 1991 are employed in firms that pay 0.2% higher wages. In Column (4), we use the average wage of coworkers as an alternative measure of firms' wage premium, and find similar results.

Having credit access also affects workers' job positions within firms. We define a job position as a top position in a firm if it is a managerial position or in the highest hierarchy. About 10% of the workers are in top positions. Column (5) shows that individuals with high ETVs are more likely to work in a top position following the reform.

Finally, we test the bargaining channel by looking at wage changes within job spells. In Column 6, we include establishment-year fixed effects i.e. job fixed effects. Workers with ETVs higher than 0.2 experience an increase in wages of 0.4% in their existing jobs, which accounts for approximately 20% of the positive wage effect.

Taken together the results suggest that both sorting and bargaining channels are important. Bargaining explains about 20% of the positive wage effect, and the rest is explained by moving to firms paying higher wages and better job positions.[15]

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 (Gorea and Midrigan 2018). 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.

Our results suggest that access to housing collateral plays an important role in insuring against negative income shocks even in a country like Denmark, which has one of the most generous UI benefit systems among OECD countries. Markwardt et al. (2014) find that people with more

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[15]Another explanation is that relaxation of credit constraint increases workers' productivity by encouraging more human capital accumulation. Similar to firms cutting investment when financially constrained (Bolton, Chen and Wang 2011), individuals may also invest less in human capital when credit constrained (Sun and Yannelis 2016; Fos, Liberman and Yannelis 2017). In Appendix Table A3 we show that the probability of training increases for individuals with high ETVs after the reform, but the effect on duration of training is insignificant.

housing equity are less likely to take up UI, suggesting that borrowing in credit markets is substitute for public insurance like UI benefits. However, contrary to UI benefits, we do not find negative employment effects of more credit access, perhaps because borrowers have more incentives to work to pay up the debt. How to optimally combine credit markets with public insurance to relax liquidity constraints is an interesting direction for future research.

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Figure 1: Share of homeowners extracting equity by year
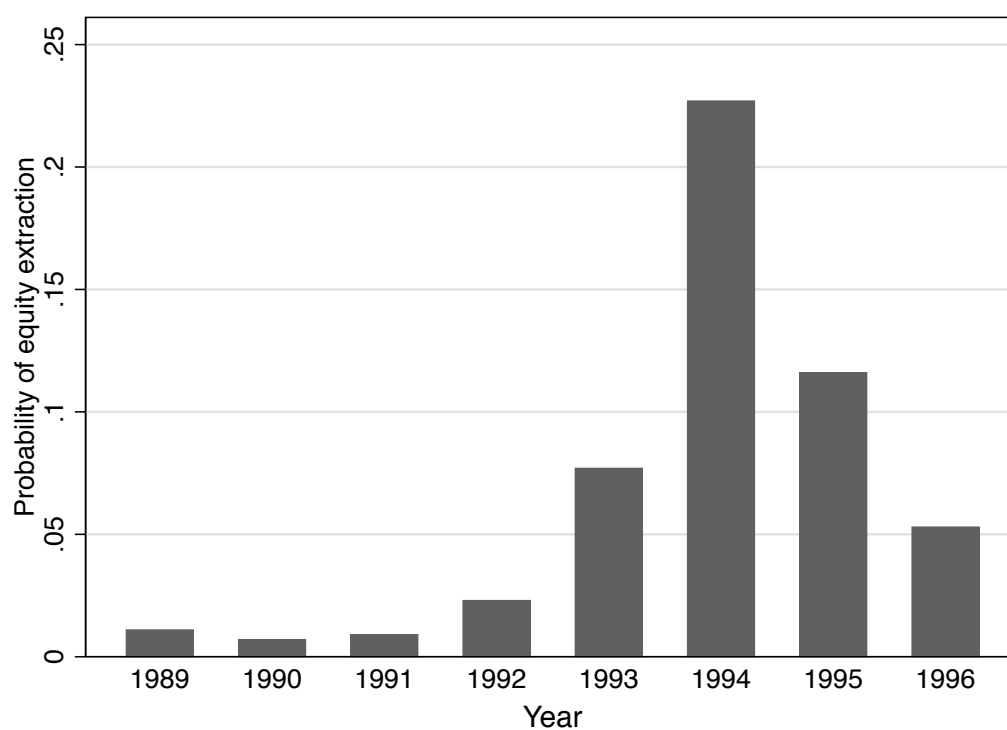
Notes: This figure shows the share of home owners extracting housing equity in Denmark by year. Following Bhutta and Keys (2016), we define extraction of housing equity as instances when a borrower's outstanding mortgage debt increases by more than 5 percent over a one year period, with a minimum increase 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
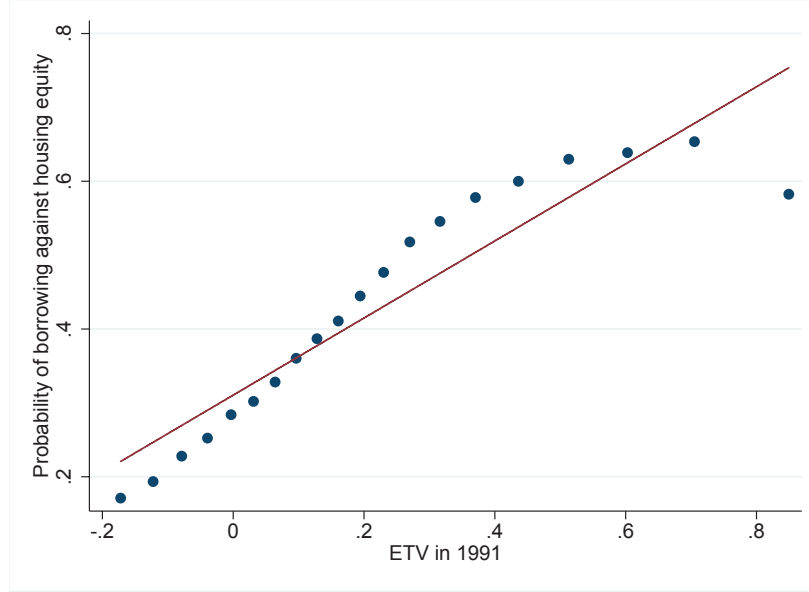
(a) Probability of equity extraction
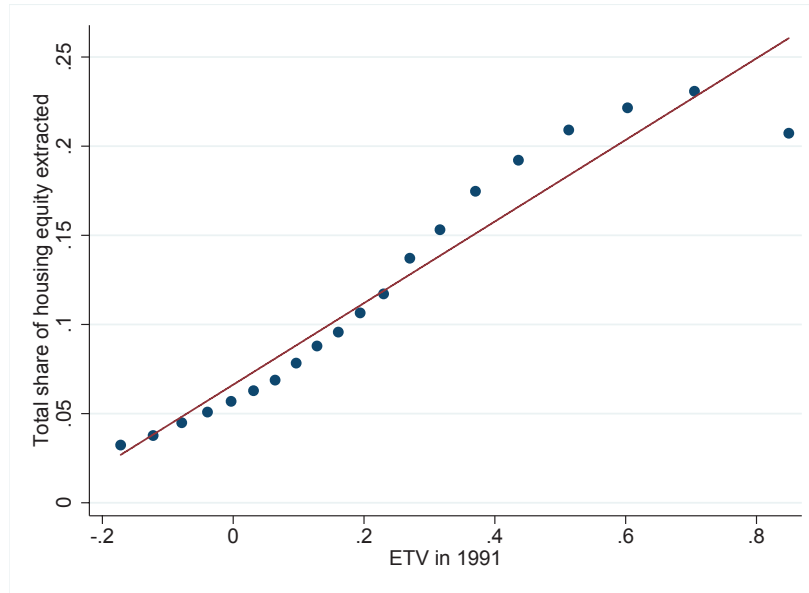
(b) Fraction of housing equity extracted

Notes: This figure shows the binscatter 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: Effects of reform on wages over time
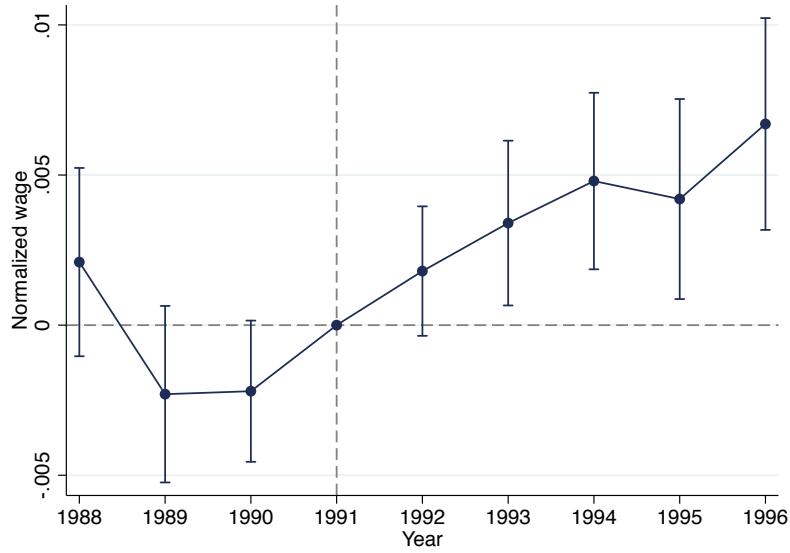
(a) All workers
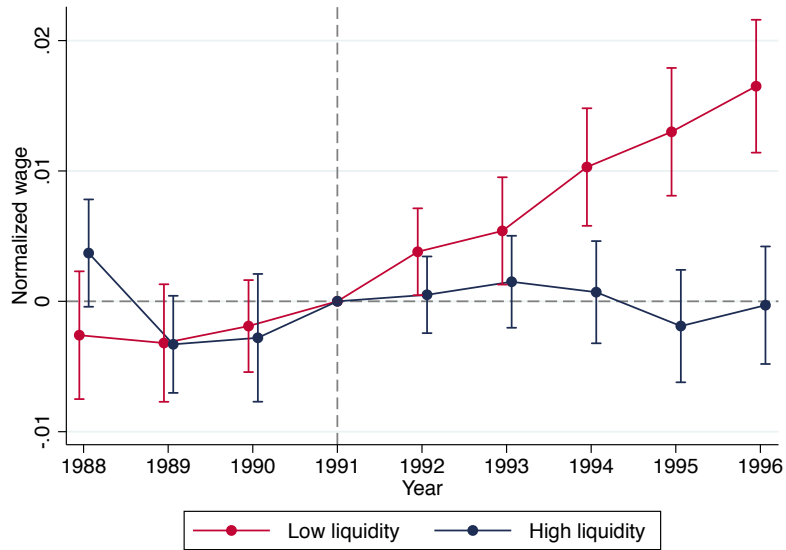
(b) By level of liquid assets

Notes: This figure shows the dynamic treatment effects of the mortgage reform on earnings of individuals with ETVs higher than 0.2 in 1991 over time, i.e. coefficients β_τ in equation (7). The dependent variable is annual wage earnings normalized by the average annual wage earnings during the sample period. 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 individual 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<0.2	ETV>0.2
	Mean	Median	Std. Dev.		
Age	40.6	41	8.52	38.2	44
Female	0.35	0	0.48	0.37	0.34
Kids	0.27	0	0.45	0.29	0.25
Partner	0.84	1	0.37	0.84	0.84
Basic education	0.3	0	0.46	0.29	0.31
Vocational training	0.44	0	0.5	0.43	0.44
College education	0.26	0	0.44	0.26	0.24
Experience	15.6	15.1	7.69	14.4	17.3
Annual earning (1000 DKK)	212.5	199.6	181.9	217.7	207.7
Annual wage (1000 DKK)	192.3	197.1	131.8	198.8	186.4
Hourly wage	152.2	137	79.2	150.6	151.9
Unemployment in 1991	0.06	0	0.24	0.06	0.06
AKM Firm FE	0.33	0.35	0.2	0.33	0.32
Job tenure	4.7	3	4.3	4.2	5.3
Housing price in 1991 (1000 DKK)	411.1	356	230.6	367.9	434.9
Total asset in 1991 (1000 DKK)	525.4	410.4	1425	455.9	590.6
Liquid asset in 1991 (1000 DKK)	114.3	22.8	1374	88	140.7
Total liability in 1991 (1000 DKK)	380.7	312.7	743.7	452.6	302.4
Mortgage debt in 1991 (1000 DKK)	263.7	228.9	188	331.6	189
Bank debt in 1991 (1000 DKK)	79.4	37.4	611.7	78.9	79.8
Maximum housing equity unlocked in 1991 (1000 DKK)	78.7	9.5	127.8	1.2	163.5
ETV IN 1991	0.34	0.27	0.31	0.05	0.62
Number of observations	7,434,558			3,564,324	3,870,234
Number of person	826,062			396,036	430,026

Notes: This table reports the summary statistics for 826,062 in our balanced-panel sample of home owners. Worker level information are from income register and is available for the entire sample period (1988-1996). 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>0.2)												
Post*1(ETV91>0.2)	0.0620	***	0.0211	***	0.0757	***	0.0491	***	0.0178	***	0.0427	***
	(0.0004)		(0.0001)		(0.0026)		(0.0004)		(0.0001)		(0.0032)	
Post*1(ETV91>0.2)* Low Liquidity							0.0374	***	0.0097	***	0.0906	***
							(0.0007)		(0.0002)		(0.0046)	
B. Treatment: ETV91												
Post*ETV91	0.1045	***	0.0385	***	0.2384	***	0.0788	***	0.0313	***	0.1799	***
	(0.0006)		(0.0002)		(0.0044)		(0.0007)		(0.0002)		(0.0053)	
Post*ETV91* Low Liquidity							0.0863	***	0.0241	***	0.1788	***
							(0.0012)		(0.0004)		(0.0076)	
Person FE	✔		✔		✔		✔		✔		✔	
Municipality*year FE	✔		✔		✔		✔		✔		✔	
Observables*year FE	✔		✔		✔		✔		✔		✔	
Number of observations	6,819,246		6,819,246		6,819,246		6,819,246		6,819,246		6,819,246	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (4) and (5)). 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 individual level and are reported in parentheses.

Table 3 Effects of Mortgage Reform on Wages and Employment

Dependent variable	(1) Normalized earnings		(2) Log wage		(3) Employment rate		(4) Normalized earnings		(5) Log wage		(6) Employment rate	
A. Treatment: Dummy for (ETV91>0.2)												
Post*1(ETV91>0.2)	0.0072	***	0.0044	***	0.0008		0.0009		-0.0046	***	0.0018	***
	(0.0023)		(0.0017)		(0.0007)		(0.0022)		(0.0016)		(0.0007)	
Post*1(ETV91>0.2)* Low Liquidity							0.0155	***	0.0209	***	-0.0030	***
							(0.0052)		(0.0022)		(0.0009)	
B. Treatment: ETV91												
Post*ETV91	0.0130	***	0.0107	***	0.0019	**	0.0030		-0.0074	***	0.0033	***
	(0.0031)		(0.0021)		(0.0009)		(0.0037)		(0.0027)		(0.0011)	
Post*ETV91* Low Liquidity							0.0243	***	0.0445	***	-0.0041	***
							(0.0065)		(0.0036)		(0.0016)	
Person FE	✔		✔		✔		✔		✔		✔	
Municipality*year FE	✔		✔		✔		✔		✔		✔	
Observables*year FE	✔		✔		✔		✔		✔		✔	
Number of observations	6,819,246		6,178,846		6,819,246		6,819,246		6,178,846		6,819,246	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (4) and (5)). 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 individual level and are reported in parentheses.

Table 4 Heterogeneity of Wage Effects by Individual Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: earnings normalized by average annual earnings 1988-1996						
	Basic Education	Vocational Education	Higher Education	Male	Female	Age<40	Age>=40
Post*1(ETV91>0.2)	0.0077	0.0020	-0.0045	-0.0033	0.0040	-0.0046 *	0.0033
	(0.0055)	(0.0029)	(0.0033)	(0.0024)	(0.0043)	(0.0028)	(0.0039)
Post*1(ETV91>0.2)* Low Liquidity	0.0300 **	0.0069	0.0164 **	0.0100 *	0.0515 ***	0.0144 *	0.0353 ***
	(0.0118)	(0.0070)	(0.0075)	(0.0056)	(0.0123)	(0.0080)	(0.0065)
Person FE	✔	✔	✔	✔	✔	✔	✔
Municipality*year FE	✔	✔	✔	✔	✔	✔	✔
Observables*year FE	✔	✔	✔	✔	✔	✔	✔
Number of observations	2,008,718	3,021,472	1,704,596	4,276,165	2,281,393	3,118,932	3,438,639

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (5)) 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 individual level and are reported in parentheses.

Table 5 Robustness to Self Employment

Dependent variable	(1) Self employment		(2) Self employment	(3) Normalized earnings		(4) Log wage		(5) Employment rate	
				(Excluding self-employed workers)					
Post*1(ETV91>0.2)	0.0009	**	0.0008	-0.0026		-0.0060	***	0.0014	**
	(0.0004)		(0.0005)	(0.0020)		(0.0014)		(0.0006)	
Post*1(ETV91>0.2)* Low Liquidity			0.0009	0.0288	***	0.0440	***	-0.0008	
			(0.0010)	(0.0049)		(0.0032)		(0.0014)	
Person FE	✔		✔	✔		✔		✔	
Municipality*year FE	✔		✔	✔		✔		✔	
Observables*year FE	✔		✔	✔		✔		✔	
Number of observations	6,819,246		6,819,246	6,143,229		5,786,635		6,143,229	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (4) and (5)). In Column 1 and Column 2, the dependent variable is an indicator variable which takes the value of 1 if the individual is an entrepreneur in a given year. In Column 3 to 5 we exclude all individuals who were entrepreneurs at any time during 1988-1996. 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 individual level and are reported in parentheses.

Table 6 Placebo Test: Year 1988 to 1991

Dependent variable	(1) Normalized earnings	(2) Log wage	(3) Employment rate		(4) Normalized earnings		(5) Log wage	(6) Employment rate	
Post*1(ETV89>0.2)	0.0039 (0.0024)	-0.0017 (0.0013)	0.0010 (0.0005)	**	0.0058 (0.0030)	*	-0.0012 (0.0021)	0.0013 (0.0007)	*
Post*1(ETV89>0.2)* Low Liquidity					-0.0029 (0.0050)		0.0005 (0.0024)	-0.0015 (0.0010)	
Person FE	✔	✔	✔		✔		✔	✔	
Municipality*year FE	✔	✔	✔		✔		✔	✔	
Observables*year FE	✔	✔	✔		✔		✔	✔	
Number of observations	2,452,892	2,219,735	2,452,892		2,452,892		2,219,735	2,452,892	

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 (1988-1991). Normalized earnings are annual earnings divided by the average annual earnings from 1988 to 1991, 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 1989, ETV interacted with an indicator for the post-1990 period, and interactions of ETV, post-1990 dummy, and an indicator variable which equals one if the individual has liquid assets less than one month's disposable income in 1989. 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 1989, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 7 Determinates of Equity Extraction

	Outcome variable is Extract={0,1}					
	(1)	(2)	(3)	(4)	(5)	(6)
Income growth	-0.0347 ***	-0.0227 ***				
	(0.0017)	(0.0018)				
Income growth × Low liquidity in 1991		-0.0227 ***				
		(0.0045)				
Unemployment			0.0028 ***		0.0023 *	
			(0.0007)		(0.0014)	
Firm employment growth				-0.0032 ***		
				(0.0008)		
Log earnings						-0.0037 ***
						(0.0012)
Low liquidity in 1991	0.0341 **	0.0346 **	0.0333 **	0.0335 **		
	(0.0005)	(0.0005)	(0.0005)	(0.0005)		
Municipality*Year FE	✔	✔	✔	✔	✔	✔
Person FE					✔	✔
No. of observations	2,196,158	2,196,158	2,196,158	2,196,158	2,196,158	2,196,158

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 (equation (8)). The dependent variable is an indicator variable for extracting housing equity. Individuals with low liquidity in 1991 are individuals who had liquid assets less than one month's disposable income in 1991. The regressions control for municipality-year fixed effects, and in Column 5 and 6 also control for individual fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Table 8 Effects of Mortgage Reform on Unemployed Workers

Dependent variable	(1) Unemployment duration		(2) Unemployment duration		(3) Replacement rate		(4) Replacement rate	
1(ETV91>0.2)	0.0703	**	0.0409		0.0080		-0.0177	
	(0.0300)		(0.0348)		(0.0122)		(0.0146)	
1(ETV91>0.2)* Low Liquidity			0.1347	**			0.0674	***
			(0.0650)				(0.0249)	
Log wage before unemployment	-0.0523	***	-0.0623	***	-0.3243	***	-0.3326	***
	(0.0086)		(0.0087)		(0.0034)		(0.0035)	
Age dummies	✔		✔		✔		✔	
Municipality FE	✔		✔		✔		✔	
Cohort FE	✔		✔		✔		✔	
Number of observations	42,952		42,952		42,952		42,952	

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 (equation (9)). Unemployment duration is measured in years. The replacement rate is calculated as the reemployment wage divided by the average annual wage during three years before unemployment. 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 9 Mechanisms of Wage Growth

Dependent variable	(1) Switch firm		(2) Switch city		(3) AKM Firm FE		(4) Coworker wage		(5) Top position		(6) Log wage	
Post*1(ETV91>0.2)	0.0024	*	0.0223	***	0.0008	**	0.0010	*	-0.0033		-0.0010	
	(0.0013)		(0.0006)		(0.0004)		(0.0006)		(0.0024)		(0.0007)	
Post*1(ETV91>0.2)* Low Liquidity	0.0121	***	0.0083	***	0.0012	**	0.0017	**	0.0100	*	0.0049	***
	(0.0020)		(0.0009)		(0.0006)		(0.0009)		(0.0056)		(0.0009)	
Person FE	✔		✔		✔		✔		✔		✔	
Municipality*year FE	✔		✔		✔		✔		✔		✔	
Observables*year FE	✔		✔		✔		✔		✔		✔	
Person*firm (job) FE											✔	
Mean of Dep. Var.	0.164		0.055		0.496		11.71		0.093		11.71	
Number of observations	5,445,480		5,445,480		5,445,480		5,445,480		5,445,480		5,445,480	

Notes: (* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$) This table reports estimates from OLS regressions (equation (5)). In Column 1 dependent variable is an indicator variable for changing employer. In Column 2 dependent variable is an indicator variable for changing municipality. In Column 3 dependent variable is the AKM firm fixed effect of the employer, which is estimated from two-way fixed effect regressions with worker FE and firm FE. In Column 4 dependent variable is average wage of coworkers. In Column 5 dependent variable is an indicator variable for working in a top position, which is identified by the job hierarchy code. 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. In Column 6 the regression also includes person-establishment fixed effects. Standard errors are clustered at the individual level and are reported in parentheses.

Figure A1: 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.