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

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ABSTRACT

We study the causal effect of liquidity constraints on individual labor market outcomes by exploiting the 1992 mortgage reform in Denmark, which for the first time allowed homeowners to borrow against housing equity for nonhousing purposes. Following the reform, liquidity-constrained homeowners increased debt levels and had higher earnings growth and lower employment rates. The option to borrow against housing equity enabled liquidity-constrained individuals to move to high-wage jobs and invest in valuable human and physical capital. The results imply that relaxing household liquidity constraints during recessions can create better job matches, potentially increasing earnings and output in the longer run.

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, Schneider, and Tufano (2011)).[1] This makes them very fragile to unexpected income shocks. Research on unemployment insurance (UI) has emphasized the role of liquidity constraints in job search, but in the case of UI benefits, the liquidity effect is accompanied by a moral hazard effect, leading to an ambiguous effect on wages and efficiency (Hansen and Imrohoroğlu (1992), Chetty (2008)). At the same time, understanding the effect of liquidity constraints on labor supply and earnings is relevant for stabilization policies

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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)).

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targeting short-term liquidity constraints of households, especially during recessions (Eberly and Krishnamurthy (2014)). 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. In addition, studies using exogenous variation often consider a modest change in 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 credit checks and in turn employment opportunities (Herkenhoff, Phillips, and Cohen-Cole (2021)). Debt relief programs and changes in housing prices tend to affect both short-run liquidity constraints and long-run debt obligations.[2]

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 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. The reform allowed households to borrow up to 80% of the value of the house, so it only affected households with an equity-to-value ratio (ETV) above 0.2. 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. We then combine the household balance sheet data with detailed matched employer-employee data 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. Homeowners with an ETV higher than 0.2 in 1991 experienced an 11% increase in debt as a fraction of annual income and a 0.8% increase in wages after the reform compared to homeowners with an ETV lower than 0.2 in 1991.

To isolate the reform's effects on liquidity constraints, we compare the effects on individuals with liquid assets[3] less than one month's disposable income in 1991 and individuals with higher levels of liquid assets in 1991. While liquidity-constrained individuals with an ETV above 0.2 experienced an increase in debt levels equal to 15% of annual income and an increase in wages of 1.9% following the reform, nonliquidity-constrained individuals with an ETV

[2] Recently, Dobbie and Song (2020) and Ganong and Noel (2020) use randomized experiments and natural experiments created by mortgage modification programs to disentangle the effect of short-term liquidity provision from the effect of long-term debt reduction.

[3] Liquid assets are nonhousing assets, including bank deposits, cash, stocks, and bonds.

above 0.2 experienced an increase in debt levels of 8% of annual income and a decrease in wages of 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 nonliquidity-constrained individuals increased slightly after the reform. The positive effect on earnings is greatest for younger workers and for 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 conditional on observed prereform characteristics, including demographics, total wealth, and municipality of residence. 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 nonliquidity-constrained groups. Second, we show that our results are robust to the inclusion of industry-specific and income-level-specific trends as well as a linear pretrend. Third, we conduct a placebo test using the years prior to the reform and show that individuals with different levels of housing equity right before the placebo reform years had similar subsequent wage and employment growth conditional on observed characteristics. Fourth, we show that renters, who had no housing equity and were not affected by the reform, exhibit similar wage trends as households with an ETV below 0.2.

To further bolster the causal interpretation of our results, we consider two distinct instrumental variables for housing equity in 1991. Our first instrument is the year of home purchase. The instrument has a strong first stage: the year of home purchase is negatively correlated with the ETV in 1991. The identifying assumption is that factors that determine home purchase decisions, such as optimism about housing prices, are uncorrelated with wage growth after 1991 conditional on observable characteristics. We also consider a variant of the instrument using the first year of entering homeownership rather than the year of purchasing the focal home. This variant of the instrument is less likely to be affected by recent life events. Both variations of the instrument yield similar effects on debt and labor market outcomes as the ordinary least squares (OLS) estimates. The second instrument is the cumulative municipality-level housing price appreciation since the year of home purchase following Gerardi et al. (2018). Since the instrument varies both by year of purchase and by municipality, we are able to include both home purchase cohort-by-year fixed effects and municipality-by-year fixed effects, ruling out confounding factors that are only related to the timing of housing purchase or only related to the choice of municipality to reside in. The instrument also has a strong first stage and similar second-stage estimates as the OLS regressions.

Our findings that relaxing liquidity constraints leads to higher wages and lower employment rates are consistent with models of job search with risk-averse workers. For unemployed workers, providing liquidity raises the reservation wages, and therefore, workers stay in unemployment longer and wait for better job offers. We show that for workers who were unemployed in 1991, having access to housing equity increases unemployment duration, reduces reemployment hazard rates, and increases reemployment wages. In particular, individuals with an ETV above 0.2 on average borrowed 5% of annual income and increased their unemployment durations by 3.9 days, which is comparable to the elasticity from the empirical UI literature (Nakajima (2012)). We also find that workers who recently become unemployed or experience negative income shocks are more likely to borrow against housing equity. This suggests that the additional liquidity from housing equity helps individuals weather negative labor market shocks better.

For employed workers, having the extra liquidity buffer through home equity loans enables workers to move to higher wage jobs by allowing them to take on more risks and invest in human and physical capital. We find that after 1992, liquidity-constrained households with more housing equity are more likely to switch to new jobs and new occupations with higher pay and to work for employers paying higher wages, as measured by the average wage of coworkers or firm-wage fixed effects. Workers with more housing equity also had higher probabilities of taking up adult vocational training and purchasing a car, and commuted longer distances to work following the reform, suggesting that liquidity-constrained individuals are forgoing valuable investment opportunities in human capital and physical capital such as cars.

We consider several alternative mechanisms for our findings. First, although access to housing equity leads to higher rates of entrepreneurship, the effect is small compared to the wage gains since only a small share of workers are self-employed. When we exclude self-employed workers from the analysis, we obtain similar results. Second, we show that financial distress and debt overhang are unlikely to explain our results, as we find similar results when we exclude workers who had negative or very low housing equity at any time during the sample period, as well as when we control for ex-ante debt levels. The negative effect on the employment rate of high-ETV individuals is also inconsistent with the negative effect of debt overhang on labor supply. Third, 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. This suggests that productivity changes due to higher consumption (e.g., child support) and lower anxiety is not the primary driver of our baseline results.

Our paper is related to the literature on how unemployment benefits affect employment and wages. Similar to home equity loans, UI benefits insure workers against negative labor income shocks and enable unemployed workers to search more patiently (Mortensen (1977), Diamond (1981)). Markwardt, Martinello, and Sándor (2014) find that homeowners who can borrow against housing equity are less likely to sign up for UI, suggesting that home equity loans and UI benefits are substitutes. However, unlike home equity loans that must be repaid or defaulted upon, unemployment benefits are a transfer to households—and therefore also have a moral hazard effect that distorts incentives to search for jobs. Theoretically, an optimal level of UI benefits that takes into account both the liquidity effect and the moral hazard effect can improve aggregate efficiency and output (Hansen and Imrohoroğlu (1992), Acemoglu and Shimer (2000), Chetty (2008)). In practice, the empirical UI literature finds an ambiguous effect of UI benefits on wages (Lalive (2007), Van Ours and Vodopivec (2008), Schmieder, von Wachter, and Bender (2016), Nekoei and Weber (2017), Johnston and Mas (2018), Price (2019)). Our results on liquidity constraints speak to the effect of expanding UI benefits in recessions, when the liquidity effect is strongest and the moral hazard effect is weakest (Kroft and Notowidigdo (2016), Ganong et al. (2021)).

Our paper contributes to a growing body of work studying the relationship between household balance sheets and labor market outcomes. One strand of literature studies financial distress associated with negative housing equity or high-interest payday loans, which often have adverse labor market consequences (Melzer (2011), Carrell and Zinman (2014), Mian and Sufi (2014), Dobbie and Song (2020), Maturana and Nickerson (2020), Bernstein, Mcquade, and Townsend (2021)). In particular, when the housing equity is negative, households may engage in strategic default (Mayer et al. (2014)), which might hurt job performance. Another strand of literature shows that high levels of mortgage or student debt have a "debt overhang" effect, leading to reduced labor supply and investment (Herkenhoff and Ohanian (2011), Melzer (2017), Fos, Liberman, and Yannelis (2017), Di Maggio, Kalda, and Yao (2019), Donaldson, Piacentino, and Thakor (2019), Ji (2021), Bernstein, Mcquade, and Townsend (2021), Fontaine, Jensen, and Vejlin (2023)). The Danish mortgage reform expanded credit access for homeowners while keeping other parts of the household balance sheet fixed, which allows us to isolate the effect of liquidity constraints from financial distress and debt overhang effects. 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 unemployed workers. We find similar results for unemployed workers, and we additionally find a positive wage effect for employed workers. We provide the first causal estimates of liquidity constraints on earnings of all workers using an unexpected and large liquidity shock, which expands credit access by one year of disposable income for more than 50% of households and is a much bigger shock to liquidity than unsecured credit.

Finally, our paper also relates to the literature on mortgage refinancing and restructuring and household liquidity constraints. During the Great Recession, mortgage refinancing under low interest rates and mortgage restructuring relaxed liquidity constraints of homeowners and led to increases in consumption (Agarwal et al. (2017), Di Maggio et al. (2017)). The mortgage reform studied in this paper allowed households to borrow against housing equity, which effectively delayed mortgage payments during the 1992 recession in Denmark. Our findings echo the conclusions in Eberly and Krishnamurthy (2014) and Ganong and Noel (2020) that transfers and temporary payment reductions (such as interest rate reductions, payment deferrals, or term extensions) to liquidity-constrained households during recessions can bring significant welfare gains. Past research highlights various frictions limiting mortgage refinancing and restructuring, including contract rigidity, equity financing constraints, and intermediary organization constraints (Piskorski, Seru, and Vig (2010), Piskorski and Seru (2018), DeFusco and Mondragon (2020)). Our results imply that reducing these frictions as the reform did can bring efficiency gains by allowing workers to access higher wages and better jobs in addition to realizing housing market and consumption benefits. Our results on spatial heterogeneity are also related to studies on regional variation in mortgage rates and refinancing (Hurst et al. (2016), Beraja et al. (2019)).

The rest of the paper is organized as follows. Section I describes the institutional details of the mortgage reform and discusses the conceptual framework for how liquidity constraints affect earnings. Section II describes our data and empirical strategy. Section III presents the paper's main results, and Section IV explores the mechanisms. Section V concludes.

I. Background

A. The 1992 Mortgage Reform in Denmark

We study the Danish mortgage reform, which took effect on May 21, 1992. The most important aspect 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 home value for loans for nonhousing purposes; 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. Loans were granted 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.[4]

A second aspect of the reform was that the maximum maturity of mortgage loans was extended from 20 to 30 years. This option also provided homeowners with more liquidity by reducing the monthly installments on a mortgage loan while spreading them out over a longer time horizon.

[4] In Denmark, a mortgage loan for housing is funded solely through the issuance of bonds sold on the stock exchange. Mortgage credit bonds were structured to align with the repayment profile and maturity of the loan granted. Thus, mortgage banks could not use deposits to fund their mortgage lending. Under this system, all borrowers who received a loan at a given point in time were subject to the same interest rate. This uniformity was achievable due to the tightly regulated nature of the mortgage market in Denmark. First, mortgage banks were bound by solvency ratio requirements, which were closely monitored by the Financial Supervision Authority and a legally defined threshold limited lending to 80% of the house value when the loan was issued. Second, each individual land plot in Denmark had a unique identification number to which relevant information about owners and collateralized debt was recorded. Third, mortgage loans had a seniority over other loans and if debtors could not maintain their loans, creditors could demand the 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 these regulatory measures governing mortgage lending and the existence of a buffer to cover loan defaults implied that the loans offered by mortgage banks were deemed highly secure. This approach justified lending decisions based solely on the value of the collateral.

A third feature of the reform was a possibility of refinancing mortgage loans. This made it possible for borrowers to reduce the cost of a loan when the market interest rate falls. While the other two parts of the reform impacted access to credit, this particular element provided homeowners with a possibility of obtaining lower monthly payments on their mortgages and increase their wealth.

The reform was carried out with short notice and passed through the Danish parliament in three months. The short time line from its introduction to implementation is valuable for our empirical strategy since individuals have little time to strategically take advantage of the reform. Internet Appendix Figure IA.1 plots the unemployment rate and real housing price in Denmark around the reform.[5] The reform was introduced during the 1992 recession when unemployment reached over 10% and was implemented before the Danish economy and housing prices started to grow rapidly. Lessons from this reform may therefore shed light on other similar policies during recoveries.

In this paper, we focus on the first two elements of the reform, which increased homeowners access to credit. The increase in credit was comparable to at least one year of disposable income for more than 50% of sample households (Leth-Petersen (2010)). To isolate the credit access effect of the reform, we focus on households with a high level of ETV and credit-constrained households, which are most likely to have been affected by the expanded credit access following the reform. We discuss the empirical design in more detail in Section II.D.

Mortgage loan delinquencies and defaults have traditionally been low in Denmark, averaging around 0.2% compared to 5% in the United States (Stanga, Vlahu, and de Haan (2020)). The loan-to-value ceiling of 80% on new mortgage loans limits lender losses in the event of a default. Furthermore, in Denmark, mortgage loans are full-recourse, and borrowers are personally liable for the outstanding loan amount exceeding the value of a realized sale of a house.[6] Borrowers therefore have strong incentives to keep payments and avoid forced sales.

Two other papers study the same reform. Leth-Petersen (2010) finds that affected households increased their consumption and debt levels following the reform, and Jensen, Leth-Petersen, and Nanda (2022) find that access to housing equity increased entrepreneurship. 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.

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

[6] A mortgage loan is declared in default after 3.5 months of nonpayment. Forced sale procedures are then started unless alternative procedures are agreed with the borrower. Typically, it takes less than 10 months to finalize a forced sale from the time default is declared.

B. Conceptual Framework

Our analysis of the impact of liquidity constraints on labor market outcomes is informed by existing theories of job search. Since most of these models focus on UI and do not directly apply it to the expansion of credit access to the unemployed, we briefly discuss the implications and outline alternative hypotheses.

A first class of models shows that UI extends liquidity to unemployed workers and increases their reservation wages (Mortensen (1977), Diamond (1981), 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 (2019)). The ambiguous effect 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) show theoretically that expanding the credit access of unemployed workers has similar predictions as providing higher unemployment benefits. In this class of models, whether more credit access raises the wages of unemployed workers is therefore an empirical question.

A second class of models shows that higher unemployment benefits encourage risk-averse workers to search for higher wage and riskier jobs. According to Acemoglu and Shimer (2000), an increase in the utility of unemployed workers (due to a relaxation of credit access and better consumption smoothing) increases wages by encouraging workers to search for jobs with higher specificity, that is, jobs with higher wages but a lower match rate.[7] Kaplan (2012) shows that the option to move in with parents insures young people against negative income shocks and enables them to search for jobs with higher earnings growth in the long term. An increase in utility during unemployment could also increase wages if unemployment is considered as the outside option in wage bargaining (Caldwell and Harmon (2019)), although Jäger et al. (2020) show empirically that the value of nonemployment has a much smaller effect on wages than predicted by the Nash bargaining model.

An alternative, yet not mutually exclusive, class of models examines 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 higher unemployment risks, workers who are less credit-constrained are more likely to switch to better occupations and jobs when facing adverse shocks.

These models predict that expanding credit access to workers should increase their unemployment duration and risk and therefore reduce the employment rate. While the effect on wages is ambiguous in the first class of models, other models generally predict a wage increase following a relaxation of credit constraints. In Section III, we examine the effects of the reform on

[7] In a general equilibrium, an increase in workers' access to credit could also affect the equilibrium job composition, for example, by creating more high-wage jobs (Acemoglu and Shimer (1999), Acemoglu (2001)). While we do not explore 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.

homeowners' wage and employment outcomes. We test the mechanism more specifically in Section IV.

II. Data and Research Design

A. Data

We combine several registers from Statistics Denmark to create a matched employer-employee panel data set covering the total population of Denmark from 1981.

The first part of the data set contains households' wealth and income. 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 different categories. Assets are divided into six categories: housing assets, shares, deposited mortgage deeds, cash holdings, bonds, and other assets. Housing assets are defined as the cash value of the property as set by the tax authorities. Tax-assessed house values are a bit different from market values, and we scale them with the aggregate ratio of actual house prices to tax-assessed values at the municipality level. We define liquid assets as the total value of non-housing assets. Liabilities comprise four categories: mortgage debt, bank debt, secured debt, and other debt. Mortgage debt is recorded as the market value of the underlying bonds on 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.[8]

The second part of the data set contains individuals' labor market histories. The data are collected from government registers in the last week of November each year and provide detailed information on individuals' labor market status, including that of the unemployed and of those who do not participate in the labor force. The data contain detailed information on annual wage income, hourly wage, occupation, and unemployment benefits and duration. 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 demographic characteristics such as their age, gender, education level, marital status, and number of children.

Denmark has a high union membership rate: in 2000, more than 80% of workers were covered by a collective agreement. While wage bargaining has been historically centralized, it was decentralized during the period we study. In 1991, less than 20% of workers were covered by the standard rate system (where wages are set by the industry collective agreement), while the wages of the rest of the workers were mostly negotiated at the firm or individual

[8] For couples filing their taxes jointly and co-owning their homes, we record the total assets and liabilities of the household for each individual.

level, with a wage floor set by the industry collective agreement that generally applies to very inexperienced workers (Dahl, le Maire, and Munch (2013)). Denmark also has a "flexicurity" system, which combines low firing and hiring costs with a generous social safety net, and has one of the most generous unemployment benefit systems among all Organisation for Economic Co-operation and Development (OECD) countries (Andersen and Svarer (2007)).

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

B. Summary Statistics

Table I provides summary statistics for variables on demographics, earnings, and balance sheets for all homeowners in 1991. Housing equity constitutes the majority of assets for most homeowners. Most workers 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 liquid asset level is less than the median monthly income.

On the right panel of Table I, we split the sample by ETV in 1991. The reform allowed individuals to borrow up to a maximum of 80% of their home value, and thus, individuals with an ETV lower than 0.2 are able to extract little to no 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 earlier. Nevertheless, the other demographic characteristics (gender, marital status, children, education) are similar across high- and low-ETV groups, and both groups also have similar wages and unemployment rates.

At the bottom of Table I, we calculate the potential amount of housing equity unlocked by the reform as housing equity in 1991 minus 20% of the home value (we assign a value of zero if ETV is less than 0.2). The results show that the amount of equity unlocked was substantial: the reform unlocked on average 78,500 DKK (about 12,000 USD) in housing equity. While the amount of housing equity unlocked is zero for people with an ETV below 0.2 in 1991, the average amount of housing equity unlocked for people with an ETV above 0.2 in 1991 is 145,500 DKK, which is over 70% of the average annual income level. The reform therefore provided a large positive liquidity shock to homeowners with an ETV above 0.2 in 1991, while having little effect on homeowners with a low ETV in 1991.

C. Identifying Housing Equity Extraction

We follow Bhutta and Keys (2016) to identify housing equity extractions in the data. We define equity extractions as instances in which a borrower' s

Table I

Summary Statistics

This table reports summary statistics for our baseline sample of homeowners. Worker-level information comes from the income register and is available for the entire sample period (1987 to 1997). All monetary values are normalized to real 2010 Danish krones (1 DKK $\approx$ 0.15 USD). All ages refer to the age of an individual as of November of 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" corresponds to the two highest categories (5 and 6) in the ISCED, that is, indicator that the individual has a tertiary education. "Vocational education" corresponds to 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 are classified as "basic education." Housing assets refer to the tax-assessed valuation of the individual's property scaled by the ratio of market prices to tax-assessed home values for houses traded in that municipality and year. Nonhousing 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.

	All Homeowners				
	Mean	Median	Std. Dev.	ETV<0.2	ETV>0.2
Age	40.1	41.0	8.04	37.0	42.7
Female	0.34			0.38	0.30
Kids	0.66			0.66	0.66
Married	0.70			0.66	0.73
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	7.79	14.1	17.7
Annual wage (1,000 DKK)	197.7	198.2	128.6	198.2	197.0
Hourly wage	133.8	130.0	87.3	134.2	133.5
Unemployment in 1991	0.10			0.09	0.11
Housing price in 1991 (1,000 DKK)	411.0	355.9	230.6	364.2	451.1
Total asset in 1991 (1,000 DKK)	525.0	410.1	1418	452.7	587.9
Liquid asset in 1991 (1,000 DKK)	92.0	13.9	1320	70.1	110.8
Total liability in 1991 (1,000 DKK)	380.8	312.8	742.3	459.4	313.6
Mortgage debt in 1991 (1,000 DKK)	269.8	234.2	192.4	344.6	205.8
Bank debt in 1991 (1,000 DKK)	81.3	38.4	624.1	81.4	81.3
Potential housing equity unlocked in 1991 (1,000 DKK)	78.5	9.4	127.7	0	145.5
ETV in 1991	0.30	0.25	0.30	0.06	0.51
Number of observations	8,531,288			3,936,866	4,594,422
Number of people	778,260			359,253	419,007

outstanding mortgage debt increases by more than 5% over one year, with a minimum increase of 5,000 DKK. Since we do not observe trade-line information for each mortgage held, we further require that the borrower 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 each year who have positive equity extractions. Before 1992, the fraction is around 1%, and these may be

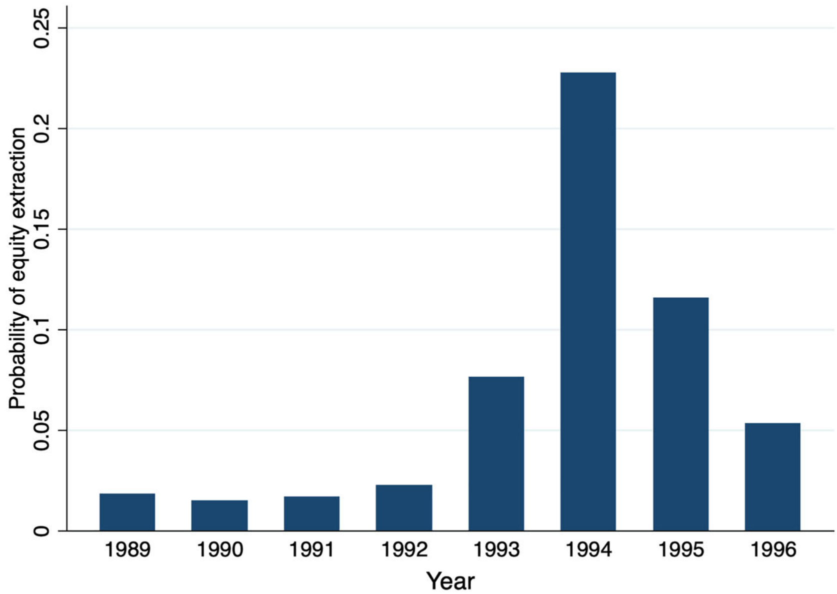

Figure 1. Share of homeowners extracting equity by year. This figure shows the share of homeowners extracting housing equity in Denmark by year. Following Bhutta and Keys (2016), we define extraction of housing equity as instances in which a borrower's outstanding mortgage debt increases by more than 5% 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 does not move over the one-year period to exclude second mortgages and new mortgages. (Color figure can be viewed at wileyonlinelibrary.com)

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 rose sharply to over 5% per year. Between 1993 and 1996, the average 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, Panel A, shows that the probability of extracting housing equity between 1992 and 1996 increases in ETV in 1991. Borrowers with an ETV higher than 0.6 in 1991 are twice as likely to extract housing equity than households with an ETV lower than 0.2 in 1991. Note that the probability of extracting equity is not zero even for households with an ETV lower than 0.2 in 1991, since housing prices grew rapidly from 1991, and higher housing prices led to higher ETVs for homeowners. Figure 2, Panel 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 the period 1992 to 1996. Borrowers with low ETV in 1991 extracted little

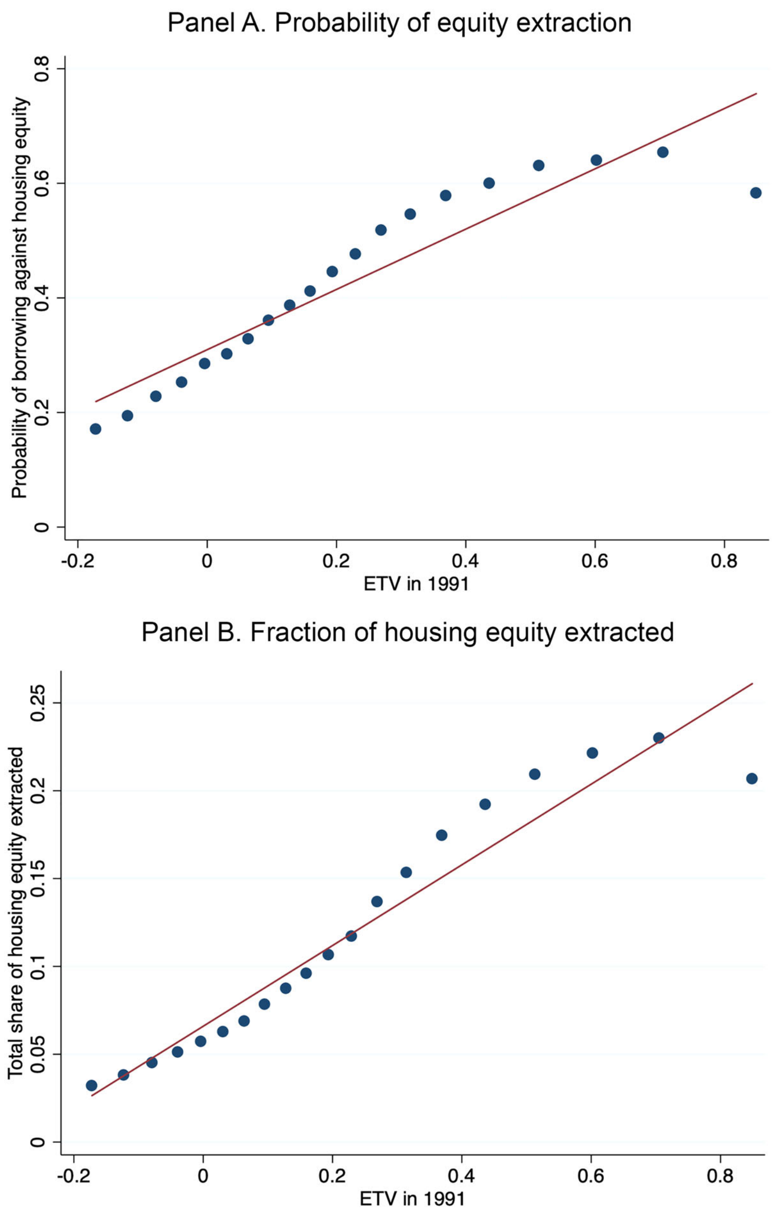

Figure 2. Equity extraction by ETV in 1991. This figure shows a binned scatter of the probability of equity extraction and the share of housing equity extracted over the five-year period from 1992 to 1996 against the equity-to-value ratio (ETV) in 1991. Each dot contains the same number of individuals. 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 of the shares for years between 1992 and 1996. (Color figure can be viewed at wileyonlinelibrary.com)

equity, while borrowers with ETV greater than 0.6 extracted about 20% of their housing equity.

D. Empirical Strategy

The reform allowed individuals, for the first time, to borrow against their housing equity for nonhousing 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 a high ETV in 1991 are more likely to borrow against housing equity and are able to extract more housing equity after the reform. We therefore divide 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. Since the reform was first introduced in May 1992, we include 1992 in our postreform period and measure individual attributes as of 1991.

Our baseline specification is as follows:

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

where y_{it} is the debt or labor outcome of person i in year t, and $Post_t \times \mathbf{1}(ETV_{91,i} > 0.2)$ equals 1 if person i had an 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, which was not affected 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 postreform dummy with the level of ETV in 1991:

$$y_{it} = \beta Post_t \times ETV_{91,i} + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it}. \tag{2}$$

We include person fixed effects in all regressions. Standard errors are clustered at the municipality level because local labor market shocks and housing price growth can be correlated within municipalities.[9] We also account for the differential response of individuals at different points in the life cycle, with different wealth, 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 individual's gender, education level, marital status, whether having children, age, decile of total household wealth,[10] and municipality of residence. We interact each of these characteristics with year dummies, ϕ_t, to control for different

[9] Clustering standard errors at the individual level does not change the significance of our results.

[10] 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.

trends in debt accumulation and earnings across people with different observable characteristics. We compare two "identical" individuals (in terms of age, gender, education level, wealth, marital status, and children) who live in the same municipality but such that one had a higher ETV than the other in 1991. Our identification relies on the assumption that these two "identical" individuals would have followed parallel trends in terms of wages and employment.[11]

To further isolate the effects of the reform on individuals' liquidity constraints, we compare the effects of the reform on individuals with a high level of liquid assets and individuals with a 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.[12] By this definition, almost 50% of all 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:

$$\begin{aligned} y_{it} = {} & \beta Post_t \times \mathbf{1}(ETV_{91,i} > 0.2) + \gamma Post_t \times \mathbf{1}(ETV_{91,i} > 0.2) \times LowLiquidity_i \\ & + \delta Post_t \times LowLiquidity_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \varepsilon_{it}, \end{aligned} \tag{3}$$

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 the 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.[13]

The key concern is that ETV in 1991 is correlated with individuals' future earnings through channels other than liquidity constraints. For example, more impatient individuals might choose lower down payments (i.e., lower ETV at origination) and have different career choices (Gerardi et al., 2018). To address this concern, we use two instrumental variable strategies and conduct placebo analyses using prereform years and renters. Our first instrument is

[11] The parallel trends assumption corresponds to the specific functional forms we consider (i.e., log wage, employment rate), but may not apply to monotonic transformations of those outcomes (Roth and Sant'Anna (2023)).

[12] We use the years prior to the reform so that differences in liquidity are less likely to be driven by reverse causality. We also use alternative measures, including the liquid asset-to-income ratio in 1991 and the *maximum* liquid asset-to-income ratio over 1986 to 1990, and get similar results. Liquid asset holdings are 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 and that at least some households with low liquid assets are restricted.

[13] We obtain similar results when estimating the baseline difference-in-differences specification separately for high-liquidity and low-liquidity individuals, or when using inverse probability weighting based on covariates.

the year of home purchase before 1991. Figure 3, Panel A, shows that the timing of home purchase strongly predicts the ETV in 1991, with individuals purchasing their homes more recently having a lower ETV in 1991. Internet Appendix Figure IA.2 shows that conditional on all of the baseline controls (including birth cohort, gender, education level, marital status, children, wealth decile, municipality), the time of home purchase still strongly predicts the ETV in 1991. This suggests that even after controlling for factors like age that clearly affect the timing of home purchase, the residual variation in the timing of home purchase is still correlated with the ETV in 1991. The identifying assumption is that, conditional on the observed covariates in 1991, the timing of the home purchase is uncorrelated with changes in employment and wages after 1992. In other words, the residual variation in the timing of home purchase after partialling out the baseline controls is uncorrelated with wage growth 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. For example, the residual variation in the timing of home purchase could be driven by optimistic beliefs about future changes in housing prices (Bailey et al. (2018)) or life events (e.g., getting married, birth of a child) many years ago (Bernstein and Koudijs (2021), Bernstein and Struyven (2022)), which are presumably uncorrelated with wage growth after 1991 conditional on controls. As a variant of this instrument, we also consider the first year of entering homeownership, which is less likely to be affected by recent life events close to 1991 that affect home purchase decisions (such as divorce) than the year of purchasing the focal home.

The second instrument for housing equity in 1991 is the local housing price change since the year of home purchase. Similar instruments have been used in Gerardi et al. (2018) and Bernstein, Mcquade, and Townsend (2021). In particular, for an individual that owns a house in 1991, we calculate the cumulative growth in average housing price of the municipality from the year of the home purchase to 1991 and use it to instrument for ETV in 1991. The identifying assumption is that the interaction between the timing of home purchase and the municipality of residence is exogenous to wage growth after 1991. Since the instrument varies by both the year of purchase and municipality, we are able to include both home purchase cohort*year fixed effects and municipality*year fixed effects. This allows us to rule out confounding factors that are only related to the timing of housing purchase or only related to the choice of municipality to reside in. The identifying assumption would be violated only if other factors that vary across both municipalities and cohorts are simultaneously correlated with prereform housing price growth and postreform wage growth (but not prereform wage growth).

For both instruments, we estimate the first stage,

$$Post_t \times ETV_{91,i} = \beta_1 Post_t \times Instrument_i + \theta X_i^{1991} \times \phi_t + \alpha_i + \epsilon_{it}, \qquad (4)$$

where $Instrument_i$ is either the year of home purchase or the cumulative growth in local housing prices since the year of home purchase. The control

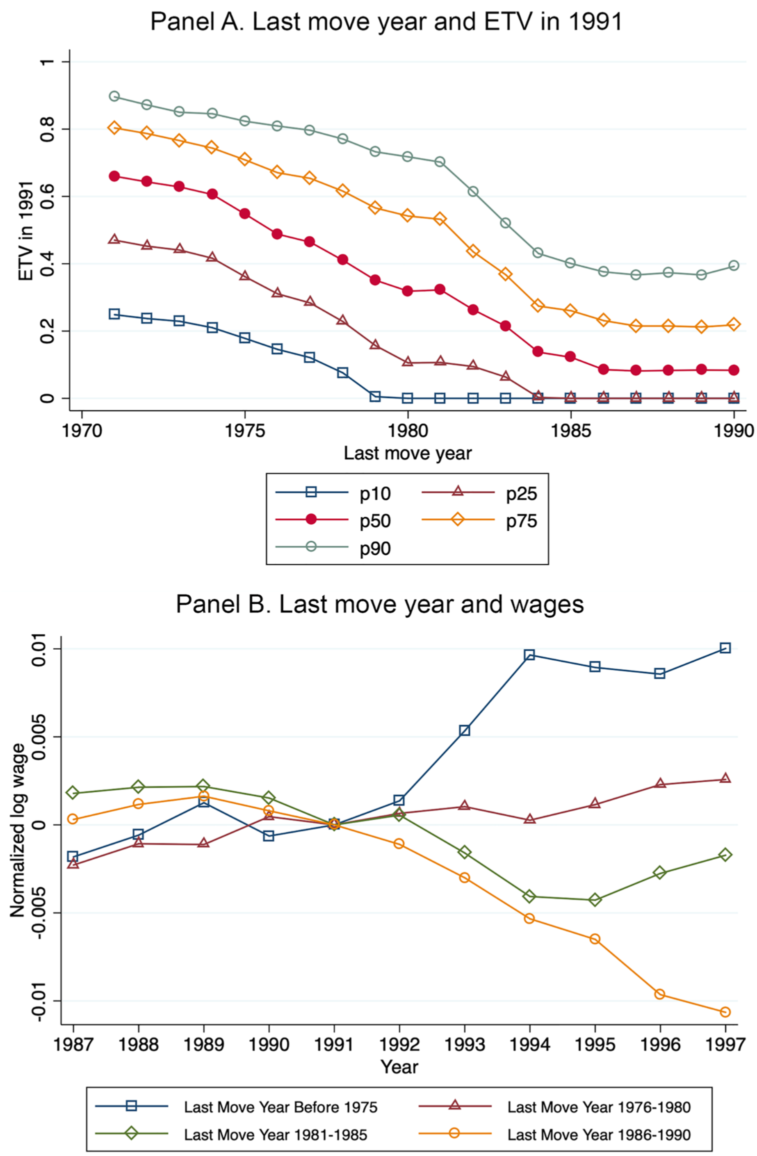

Figure 3. Effect of last move year on 1991 ETV and wages. Panel A plots the distribution of 1991 ETV (median and 10$^{\text{th}}$, 25$^{\text{th}}$, 75$^{\text{th}}$, and 90$^{\text{th}}$ percentile) against the last moving year before 1991 for the sample of homeowners in 1991. Panel B plots the normalized wage growth for four groups of homeowners in 1991 based on last moving year: before 1975, between 1976 and 1980, between 1981 and 1985, and between 1986 and 1990. The normalized wage is the residual after regressing log annual wage on year fixed effects interacted with fixed effects for birth cohort, municipality, wealth decile, education level, partner indicator, gender, and children indicator, each measured in 1991, with the 1991 wage level normalized to zero for each group. (Color figure can be viewed at wileyonlinelibrary.com)

variables are the same as in equation (1), and for the second instrument, we additionally control for home purchase cohort*year fixed effects. The second stage takes the predicted 1991 ETV from equation (4) and looks at the effect on debt and labor outcomes (we run this using two-stage least squares (2SLS) to obtain the correct standard errors):

$$y_{it} = \beta_2 \widehat{Post_t \times ETV}_{91,i} + \theta X_i^{1991} \times \phi_t + \alpha_i + u_{it}. \quad (5)$$

III. Results

A. Effects of the Reform on Borrowing

To verify that the mortgage reform impacted homeowners, we first look at the effects of the reform on equity extraction and overall liabilities. Columns (1) to (3) of Table II, Panel A report results from difference-in-differences regressions of borrowing measures on indicators for high- and low-ETV groups after 1992 (equation (1)). The unit of observation is the person-year. Following the mortgage reform, individuals with a high ETV are more likely to extract housing equity and extract a larger share of their housing equity, consistent with 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 over the period from 1987 to 1997. High-ETV individuals increased their debt level substantially after the reform: individuals with an ETV higher than 0.2 in 1991 increased their total debt level as a fraction of their annual earnings by 11.2% more than individuals with an ETV lower than 0.2 in 1991. This finding 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. If the reform increased the level of debt because it relaxed credit constraints, it should have less impact on the borrowing of housing equity for individuals who have high liquid assets and are not credit constrained. Columns (4) to (6) of Table II report the triple-differences estimates (equation (3)). We find that the triple-interaction terms for low liquidity, high ETV, and post-1991 have positive and significant effects for all three measures, indicating that individuals with low liquid assets borrow more against housing equity and increase their debt more after the reform. Among individuals with a high level of liquid assets and thus are not liquidity constrained, those with a high ETV also borrow more against housing equity, but the change in total debt level is smaller. For example, households with high liquidity and ETV greater than 0.2 increased their total debt by 8.1% of annual earnings, while households with low liquidity and ETV greater than 0.2 increased their total debt by 14.5% of annual earnings.

Table II

Effects of Mortgage Reform on Borrowing

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 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 the equity-to-value ratio (ETV) in 1991 interacted with an indicator for the postmortgage reform period, and interactions of ETV in 1991, postreform dummy, and an indicator equal to 1 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, wealth decile, education level, partner indicator, gender, and children indicator, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Columns (4) to (6) also control for postreform dummy interacted with the dummy variable for low liquid assets. Standard errors are clustered at the municipality level (there are 275 municipalities) and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	Equity Extraction (1)	Fraction of Equity Extracted (2)	Liability/Income (3)	Equity Extraction (4)	Fraction of Equity Extracted (5)	Liability/Income (6)
Panel A. Treatment: Dummy for (ETV91>0.2)						
Post*1(ETV91>0.2)	0.0661***	0.0212***	0.1117***	0.0496***	0.0173***	0.0806***
	(0.0013)	(0.0003)	(0.0078)	(0.0012)	(0.0003)	(0.0107)
Post*1(ETV91>0.2)				0.0352***	0.0083***	0.0646***
* Low Liquidity				(0.0011)	(0.0003)	(0.0097)
Panel 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				0.0806***	0.0207***	0.1059***
* Low Liquidity				(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

While all homeowners with an ETV above 0.2 in 1991 are affected by the reform, homeowners with a higher ETV are affected more because they are able to get more liquidity by extracting the housing equity. In Panel B of Table II, we use the continuous measure of ETV in 1991 as the treatment variable (equation (2)). The results are similar.[14] A one-standard-deviation increase in ETV in 1991 increases an individual's debt level by 11.1% of their annual salary. The effect on borrowing is larger for liquidity-constrained individuals than nonconstrained individuals. In Internet Appendix Table IA.I, we nonparametrically estimate the effect for four ETV buckets: 0.2 to 0.4, 0.4 to 0.6, 0.6 to 0.8, and greater than 0.8. The results show that the effect is indeed increasing in ETV, with individuals in higher ETV buckets seeing a larger increase in equity extraction and debt levels.

These results indicate that the reform did indeed relax credit constraints for individuals with a high ETV. Homeowners with a higher ETV borrowed against their housing equity and increased their overall debt level, with the effect larger for credit-constrained individuals.

B. Effects of the Reform on Wages and Employment

How does the relaxation of liquidity constraints affect wages and employment? Table III reports results from our baseline regressions using measures of wages and employment as dependent variables. In column (1), we use the log annual wage as the dependent variable. Following the reform, individuals with an ETV higher than 0.2 in 1991 experienced a wage gain of 0.78% relative to individuals with an ETV lower than 0.2 in 1991. In column (2), we use normalized earnings as the dependent variable, where we divide annual earnings by the average annual earnings over the period 1987 to 1997. This measure takes into account individuals with zero earnings.[15] We find that high-ETV individuals experienced a 0.63% increase in earnings. In column (3), the dependent variable is an employment indicator equal to one if the individual has positive earnings and zero otherwise. The employment rate of the high-ETV group increased by 0.05%, but the difference is not statistically significant.

Columns (4) to (6) of Table III present results for triple-differences specifications (equation (3)). Among liquidity-constrained individuals, an ETV greater than 0.2 leads to a 1.9% increase in wages. In contrast, for nonconstrained individuals, the effect of a high ETV on wages is negative and not statistically significant from zero. This suggests that the higher earnings experienced by individuals with a high ETV are due to the relaxation of borrowing constraints for liquidity-constrained individuals. In line with the conceptual framework in Section I.B, we find a negative effect of the reform on the employment rate for

[14] As shown in Callaway, Goodman-Bacon, and Sant' Anna (2021), this specification with continuous treatment requires the "strong" parallel trends assumption: higher-dose individuals not only need to have the same evolution of untreated potential outcomes as lower dose individuals, but also need to have the same treatment effects if they had a lower dose treatment.

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

Table III

Effects of Mortgage Reform on Wages and Employment

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 equal to 1 if the wage income is positive. The main right-hand-side variables are the equity-to-value ratio (ETV) in 1991 interacted with an indicator for the postmortgage reform period, and interactions of ETV in 1991, postreform dummy, and an indicator equal to 1 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, wealth decile, education level, partner indicator, gender, and children indicator, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Columns (4) to (6) also control for postreform dummy interacted with the dummy variable for low liquid assets. Standard errors are clustered at the municipality level (there are 275 municipalities) and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable	Log Wage (1)	Normalized Earnings (2)	Employment Rate (3)	Log Wage (4)	Normalized Earnings (5)	Employment Rate (6)
Panel A. Treatment: Dummy for (ETV91>0.2)						
Post*1(ETV91>0.2)	0.0078***	0.0063**	0.0005	−0.0024	−0.0012	0.0020**
	(0.0014)	(0.0025)	(0.0006)	(0.0021)	(0.0043)	(0.0009)
Post*1(ETV91>0.2)* Low Liquidity				0.0210***	0.0157***	−0.0032***
				(0.0027)	(0.0048)	(0.0010)
Panel 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

liquidity-constrained workers, while we find a positive effect on the employment rate for nonliquidity-constrained workers.[16]

In Panel B of Table III, we use the continuous ETV as the treatment variable. A one-standard-deviation (0.3) increase in ETV in 1991 increases wages by 0.7% on average and by 1.4% for liquidity-constrained individuals. In Internet Appendix Table IA.II, we separately estimate the effects for four ETV buckets and find larger effects for higher ETV buckets. For example, relative to individuals with an ETV below 0.2, individuals with an ETV between 0.2 and 0.4 experienced a 0.2% increase in wages, while individuals with an ETV above 0.8 experienced a 1.5% increase in wages.

How big is this effect? The estimates in column (5) of Table III indicate that the earnings of liquidity-constrained individuals with an ETV higher than 0.2 increase by 1.4% after the reform. Assuming that the earnings growth remains the same thereafter, careers last 20 years, and the discount rate is 5%, an 1.4% earnings increase implies an increase in the present discounted value equal to 18% of annual earnings, which is larger than the increase in the amount of borrowing by these individuals (15% of annual earnings from column (6) of Table II).

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

$$y_{it} = \alpha_i + \sum_{\tau=1987}^{1997} \beta_\tau \mathbf{1}(ETV_{91,i} > 0.2) \times D_t(\tau) + \theta X_i^{1991} \times \phi_t + \varepsilon_{it}, \tag{6}$$

where $D_t(\tau)$ is equal to 1 if $t = \tau$, β_τ is the effect of a high ETV on wages in year τ, and year 1991 is the base year. Figure 4, Panel (A), plots the β_τ coefficients. The effects are insignificantly different from zero before 1991 and become 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)).

High-ETV and low-ETV individuals also exhibit similar wage growth before 1992, implying that individuals with more housing equity do not systematically have higher wage growth absent the reform.[17] Internet Appendix

[16] The third part of the reform, which introduces the option to refinance, is a positive shock to homeowners' wealth. This shock 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 for high-ETV individuals relative to low-ETV individuals among high-liquidity individuals. In principle, the positive employment effect could also be a general equilibrium effect: the lower employment rate of liquidity-constrained individuals makes it easier for nonliquidity-constrained individuals to find jobs due to less competition for (lower-wage) jobs.

[17] We also do not observe any significant pretrends for normalized earnings or the employment rate.

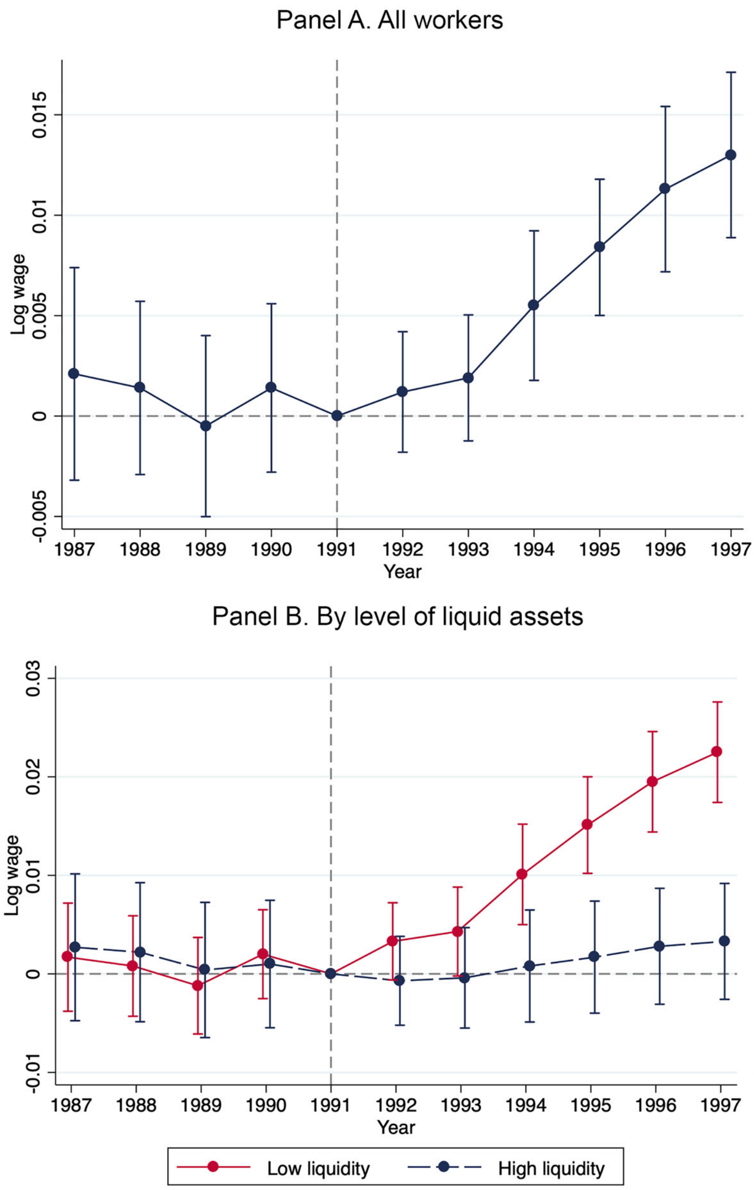

Figure 4. Effects of reform on wages over time. This figure shows dynamic treatment effects of the mortgage reform on earnings of individuals with an ETV higher than 0.2 in 1991 over time, that is, the coefficients β_τ in equation (8). The dependent variable is log wage. Control variables include year fixed effects interacted with fixed effects for birth cohort, wealth decile, education level, partner indicator, gender, and children indicator, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Standard errors are clustered at the municipality level. Panel B 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 separately. (Color figure can be viewed at wileyonlinelibrary.com)

Figure IA.3 shows that the results are robust to using simultaneous (rather than pointwise) confidence intervals (Freyaldenhoven et al. (2021)) and clustering standard errors at the individual level. Following Rambachan and Roth (2023), we conduct sensitivity analysis of the treatment effect allowing for potential deviations from parallel trends. Internet Appendix Figure IA.4 shows confidence sets of the treatment effect in 1997 relative to prereform years for various bounds. We can reject a null effect on log wage in 1997 even if the violation of parallel trends in each year between 1992 and 1997 is equal to the maximum pretreatment violation of parallel trends.

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

To shed light on the treatment effect heterogeneity, we calculate the average treatment effect on the treated (ATT) and the average treatment effect on the untreated (ATU) following Słoczyński (2020). Since the baseline treatment variable—whether individuals had an ETV above 0.2 in 1991—is a binary variable, the OLS effect on wages (0.78%) is a weighted average of the ATT, which equals 1.17%, and the ATU, which equals 0.46%. Because the treated and untreated groups are of similar size, OLS provides a good approximation to the policy-relevant average treatment effect (ATE), which equals 0.83%. We discuss heterogeneous treatment effects across worker demographics and regions in detail in the Internet Appendix.

C. Robustness

C.1. Unobserved Heterogeneity

As discussed in Section II, the main identification challenge is unobserved heterogeneity between people with a high ETV in 1991 and people with a low ETV in 1991. This leads us to include a rich set of controls in the baseline specification, so that in effect, we compare individuals with the same age, gender, education level, wealth level, and family status. In this section, we explore the possibility that other unobserved shocks exist that affect both housing equity and labor market performance. We use instrumental variables to further address these issues in the next subsection.

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 home purchase decisions and different labor market performance during and after recessions. Our data set includes 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.[18] In addition, we control for deciles of income level in 1991 interacted with year fixed effects to absorb differences in income shocks across the income distribution.

We report the results in Internet Appendix Table IA.III. The estimated coefficients are similar to those that we obtain when we include the additional labor market controls, suggesting that industry-specific income shocks as well as shocks by income level do not drive our results.

Linear pretrend: A related concern is that the expectation of income growth is correlated with the timing of house purchase. For example, if people are expecting faster income growth in the future purchase homes sooner, then this could explain the positive correlation between higher ETV and higher income growth. Nevertheless, the lack of a pretrend 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 that includes a linear trend to absorb differences in income trends between high-ETV and low-ETV individuals,

$$y_{it} = \beta Post_t \times \mathbf{1}(ETV_{91,i} > 0.2) + \theta X_i^{1991} \times \phi_t + \delta t \times \mathbf{1}(ETV_{91,i} > 0.2) + \alpha_i + \varepsilon_{it}. \tag{7}$$

The regression allows for a linear pretrend, such that our estimated effects are relative to a linear timetrend. Consistent with the insignificant pretrend in the event study, Internet Appendix Table IA.IV shows that with the inclusion of the linear pretrend, we find a similar positive effect on wages and a negative effect on the employment rate for liquidity-constrained individuals. The effect on the employment rate of nonliquidity-constrained households becomes insignificant.

C.2. Alternative Measures of Liquidity

In Internet Appendix Table IA.V, we examine the robustness of our results to alternative measures of household liquidity. Our baseline measure uses the ratio of liquid assets to monthly income between 1986 and 1990, where liquid assets include cash, bank deposits, stocks, and bonds. We find similar results when using the ratio of liquid assets to monthly income in 1991. In addition, we consider two alternative measures. First, we consider a narrower measure that 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 proxy for liquidity using an indicator for

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

whether an individual is hand-to-mouth in 1991. A person is taken to be hand-to-mouth if liquid assets minus liquid liability is lower than half of the per-pay-period income minus the credit limit, where the pay frequency is two weeks and the credit limit is one month of income. Under this definition, around 30% of homeowners in Denmark are hand-to-mouth in 1991.

The two alternative measures of liquidity constraints yield similar 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 the effects on wages for nonconstrained individuals are insignificant, and there is a positive effect on the employment rate.

D. IV Results

Table IV presents the IV results using the last move year (i.e., year of home purchase) as the instrument. Panel A reports the first stage of the IV regression. The year of home purchase strongly predicts the ETV in 1991, and the first-stage F-statistics are above 100 in all regressions.

Panel B of Table IV reports the 2SLS estimates. Consistent with the OLS estimates, people with an ETV higher than 0.2 in 1991 are more likely to extract housing equity, increase overall debt levels more, and experience higher earnings. The effect on earnings is positive and significant and slightly larger in magnitude than in the OLS specification: a one-standard-deviation increase in ETV in 1991 leads to 0.77% higher wages and 0.83% higher earnings. In Figure 3, Panel (B), we plot the nonparametric reduced-form effect of the year of home purchase on residual wage income (conditional on the observables) over time, and find that individuals who purchased homes earlier experienced higher wage growth after 1992 but had similar wage trends prior to the reform compared with individuals who purchased their home later.

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

A potential issue with using the year of home purchase as an instrument is that recent life events such as divorce may cause individuals to buy new homes and have a lower ETV in 1991 but also affect wage growth. While the lack of pretrends helps mitigate this concern, we further address this issue by using the first year of homeownership as an instrument. This instrument is less likely to be affected by recent life events. Since we do not observe the exact first year of homeownership if the first home was purchased before 1980

Table IV

Instrumental Variable Estimates Using Last Move Year as IV

This table reports first-stage and 2SLS estimates from IV regressions, where ETV in 1991 is instrumented by the last year of a move before 1991. Dependent variables are as defined in Tables II and III. In Panel B, ETV in 1991 interacted by postreform dummy is instrumented by last move year interacted with postreform dummy. In Panel C, ETV in 1991 interacted with postreform dummy and interactions of ETV in 1991, postreform dummy, and the indicator for low liquid assets in 1991 are instrumented by last move year interacted with postreform dummy and interactions of last move year, postreform dummy, and the indicator for low liquid assets in 1991. First-stage F-statistics are larger than 100 in all regressions. Control variables include year fixed effects interacted with fixed effects for birth cohort, wealth decile, education level, partner indicator, gender, and children indicator, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Regressions in Panel C also control for postreform dummy interacted with the dummy variable for low liquid assets. Standard errors are clustered at the municipality level (there are 275 municipalities) and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A. First Stage						
Dependent variable: Post*ETV91						
Post*Last Move Year	−0.0109***					
	(0.0001)					
Dependent variable	Equity Extraction (1)	Fraction of Equity Extracted (2)	Liability/Income (3)	Log Wage Income (4)	Normalized Earnings (5)	Employment Rate (6)
Panel B. Diff in Diff						
Post*ETV91	0.1067***	0.0376***	0.1129**	0.0256***	0.0276**	0.0029
	(0.0035)	(0.0008)	(0.0556)	(0.0076)	(0.0118)	(0.0032)
Panel C. Triple Diff						
Post*ETV91	0.0621***	0.0268***	0.0454	0.0009	0.0118	0.0067*
	(0.0028)	(0.0007)	(0.0610)	(0.0127)	(0.0171)	(0.0040)
Post*ETV91*	0.1145***	0.0275***	0.1561***	0.0550***	0.0353*	−0.0089**
Low Liquidity	(0.0038)	(0.0010)	(0.0326)	(0.0167)	(0.0181)	(0.0041)
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

and the individual moved from that home before 1980, we construct a binary variable indicating whether the first year of homeownership is before 1981. Internet Appendix Table IA.VI presents the estimates using this instrument. The estimates are similar to the OLS estimates for all debt and labor market outcomes.

Table V presents the instrumental variable using the cumulative appreciation in the municipality-level average housing price since the year of home purchase as the instrument. The instrument varies by year of home purchase and municipality, and we control for home purchase cohort*year fixed effects and municipality*year fixed effects in all regressions to rule out confounding factors that are related only to the timing of home purchase. Higher cumulative growth in local housing prices strongly predicts the ETV in 1991, and the first-stage F-statistics are above 100 in all specifications. We find a positive effect of 1991 ETV on debt levels and wage growth, and a negative effect on the employment rate, with a stronger effect among liquidity-constrained individuals. For example, for liquidity-constrained individuals, a one-standard-deviation increase in ETV in 1991 leads to 1.7% higher wages and 1.2% higher earnings.

In general, the IV estimates of the effects on wages are slightly larger in magnitude 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 payments and monthly payments at the beginning and have lower ETVs, which leads to a negative bias in OLS estimates. However, the OLS and IV coefficients are not statistically different, which suggests that 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.

E. Placebo Tests

We conduct two placebo tests for the causal effects of the reform. The results are reported in the Internet Appendix (Tables IA.VII, IA.VIII, and IA.IX). First, we consider three placebo reform years: 1985, 1986, and 1987. If high-ETV individuals and low-ETV individuals observe different wage growth for reasons other than liquidity constraints, we should see a difference in wage growth between high-ETV and low-ETV groups before 1992. For each placebo year t, we consider the cohort from year $t-5$ to year $t+4$, and estimate an identical difference-in-differences regression as equation (1) treating the placebo year as the year of the reform. For example, for placebo year 1985, we estimate the regression

$$y_{it} = \beta Post85_t \times \mathbf{1}(ETV_{84,i} > 0.2) + \theta X_i^{1984} \times \phi_t + \alpha_i + \varepsilon_{it}, \qquad (8)$$

where $Post85_t$ is an indicator for years after 1985, $ETV_{84,i}$ is the ETV in 1984, and all observable characteristics are measured in 1984. We then run a pooled

Table V

Instrumental Variable Estimates Using Local Housing Prices as IV

This table reports first-stage and 2SLS estimates from IV regressions, where ETV in 1991 is instrumented by the change in municipality-level housing price since the year of purchase. Dependent variables are as defined in Tables II and III. In Panel B, ETV in 1991 interacted by postreform dummy is instrumented by local housing price change since purchase interacted with postreform dummy. In Panel C, ETV in 1991 is interacted with postreform dummy and interactions of ETV in 1991, postreform dummy, and an indicator for low liquid assets in 1991 are instrumented by local housing price change since purchase interacted with postreform dummy and interactions of local housing price change since purchase, postreform dummy, and an indicator for low liquid assets in 1991. First-stage F-statistics are larger than 100 in all regressions. Control variables include year fixed effects interacted with fixed effects for birth cohort, wealth decile, education level, partner indicator, gender, and children indicator, each measured in 1991, as well as person fixed effects and municipality-year fixed effects. Regressions in Panel C also control for postreform dummy interacted with the dummy variable for low liquid assets. Standard errors are clustered at the municipality level (there are 275 municipalities) and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A. First Stage						
Dependent Variable: ETV91						
Post*Local Housing Price Change	0.1544***					
	(0.0034)					
Dependent Variable	Equity Extraction (1)	Fraction of Equity Extracted (2)	Liability/Income (3)	Log Wage (4)	Normalized Earnings (5)	Employment Rate (6)
Panel B. Diff in Diff						
Post*ETV91	0.1523***	0.0399***	0.1441*	0.0138	0.0130	0.0062
	(0.0062)	(0.0011)	(0.0871)	(0.0154)	(0.0189)	(0.0051)
Panel C. Triple Diff						
Post*ETV91	0.0971***	0.0270***	0.0698	−0.0185	−0.0074	0.0131**
	(0.0051)	(0.0009)	(0.0860)	(0.0183)	(0.0216)	(0.0066)
Post*ETV91* Low	0.1105***	0.0255***	0.1728***	0.0748***	0.0465*	−0.0155***
Liquidity	(0.0025)	(0.0008)	(0.0495)	(0.0259)	(0.0252)	(0.0057)
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Moving cohort*Year 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

regression with the three placebo cohorts (1980 to 1989, 1981 to 1990, 1982 to 1991) stacked together. We also divide individuals into high-liquidity and low-liquidity groups based on their liquid assets before each placebo year and apply the same triple-differences specification as in equation (3). If high-ETV individuals and low-ETV individuals would have different counterfactual wage trends absent the reform, we should see a difference in wage growth between high-ETV and low-ETV groups for the placebo years as well.

The results show that high-ETV and low-ETV individuals in placebo years have similar trends in wages and the employment rate before the reform actually took place. 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. For example, among liquidity-constrained individuals, those with an ETV above 0.2 in the placebo year experience only a 0.12% increase in annual wage, and the effect is not significant. This suggests that liquidity-constrained individuals with a high ETV had faster wage growth after 1992 precisely because the reform allowed home equity loans and relaxed their credit constraints.

As a second placebo test, we examine whether the reform had any effect on renters. In theory, renters do not have any housing equity to extract, and are not affected by the reform. We first include renters in the baseline regression and compare their wage and employment growth around the reform to high-ETV and low-ETV homeowners. We find that renters have similar wage growth as low-ETV individuals, although they have relatively higher employment rates after the reform, potentially due to the positive wealth shock from refinancing for the low-ETV individuals. There is also no significant difference between the effects on liquidity-constrained renters and nonliquidity-constrained renters. When comparing high-ETV individuals with renters, high-ETV individuals have higher wages and lower employment rates, and all of the effects are concentrated on liquidity-constrained high-ETV individuals.

We also estimate the reduced-form effect of the first instrument—last move year before 1991—on wages and the employment rate for the sample of renters in 1991 to examine whether the last move year is systematically related to wage growth beyond the liquidity constraint channel. If the last move year is correlated with factors other than housing equity that also affect labor market outcomes (such as recent life events), we should see an effect of the last move year among renters. However, we do not find any effect of the last move year on wages or the employment rate for renters. This supports our assumption that the last move year only affects labor market outcomes through housing equity conditional on the observables.

IV. Mechanisms

In this section, we explore how expanding credit access leads to higher earnings. We first start by describing which individuals borrow from housing equity, and show that access to housing equity is indeed used to insure against negative labor income shocks. Next, we consider unemployed individuals and show that when they have more housing equity, they remain unemployed longer and receive higher reemployment wages. We then investigate the effect of liquidity constraints on the job mobility and investments of employed workers. Finally, we consider alternative mechanisms that may explain our findings.

A. 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,

$$Extract_{ict} = \beta_1 IncomeShock_{it} + \gamma X_i^{1991} + \alpha_{ct} + \epsilon_{ict}, \tag{9}$$

where $Extract_{ict}$ is an indicator variable for housing equity extraction, and $IncomeShock_{it}$ is a measure of income shocks to person i in year t. The vector X_i^{1991} contains individual-level covariates including ETV in 1991, the level of liquid assets in 1991, and the 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 the person-year, and we only include observations for homeowners after 1992.

Table VI presents the results. In column (1), we measure income shocks in year t using the percentage growth in labor income from year $t-1$ to year t. Individuals who 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 increase in equity extraction. This corresponds to an increase of 5% relative to the average extraction rate of 11% 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 after 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. Shocks to employers cannot be diversified away or avoided and this represent uninsurable risk to workers' income (Fagereng, Guiso, and Pistaferri (2018)). In columns (5) and (6), the independent variable is an indicator for mass layoffs at the worker's establishment. A mass layoff is defined as a reduction in

Table VI

Determinates of Equity Extraction

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 change in wages. Unemployment is an indicator variable equal to 1 if the individual has zero labor earnings. Mass layoff is an indicator variable equal to 1 if the employer reduces the 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 columns (5) and (6) also control for individual fixed effects. Standard errors are clustered at the municipality level (there are 275 municipalities) and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Outcome Variable Is Extract={0,1}							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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

employment by over 30% in an establishment with 50 or more employees. In columns (7) and (8), the independent variable is the average wage change 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. Importantly, the potential to borrow can affect job search decisions no matter whether the home equity is actually extracted. As Herkenhoff, Phillips, and Cohen-Cole (2019) point out, "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 Internet Appendix Figure IA.5, the percentage of workers experiencing negative income shocks peaked around 1993, and the liquidity buffer provided by the home equity loans was particularly important the first few years after the reform came into effect.

B. 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 (2019) and Braxton, Herkenhoff, and Phillips (2020) show that the unemployed have significant access to credit, and 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 borrowing against housing equity affects the job search behavior of unemployed workers, we compare unemployment durations and reemployment wages of workers who are unemployed in 1991 just before the mortgage reform and have different levels of housing equity. We focus on people who became unemployed before the reform so that our estimates only reflect the effect of the reform conditional on being unemployed, that is, do not contain the effect of the reform on selection into unemployment. In particular, we estimate the following equation for workers unemployed in 1991:

$$D_i = \gamma \mathbf{1}(ETV_{91,i} > 0.2) + \pi \mathbf{1}(ETV_{91,i} > 0.2) \times LowLiquidity_i + \theta LowLiquidity_i + \beta X_i + \varepsilon_i, \tag{10}$$

where D_i is the unemployment duration of individual i, and vector X_i includes the individual's preunemployment wage, age fixed effects, municipality fixed effects, and dummies for the year the individual enters 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 nonliquidity-constrained individuals.

We first estimate equation (10) using OLS focusing only on people who reentered the labor market by 2005. Column (1) of Table VII, Panel A, shows that having positive housing equity increases liquidity-constrained households' unemployment duration by 0.18 months, or 5.4 days, while increasing nonliquidity-constrained households' unemployment duration by only 0.05 months, or 1.6 days. For liquidity-constrained individuals, the increase in unemployment duration explains nearly all of the negative effect on the employment rate.[19]

In column (2), 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 (10) 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 column (3), we consider how access to housing equity affects reemployment wages. The dependent variable is the change in log wage between pre- and postunemployment jobs. Theoretically, the increase in unemployment duration has two opposing effects on reemployment wages (Chetty (2008), Nekoei and Weber (2017), Price (2019)). 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 distort search incentives and reduce search effort, and longer UI duration could depreciate human capital, resulting in lower wages following unemployment. We find that the sign of the effect is different for liquidity-constrained and nonliquidity-constrained individuals: liquidity-constrained households with a high ETV experienced a 3.6% higher reemployment wages, whereas nonconstrained households with a high ETV experienced 1.8% lower reemployment wages.

Our estimates of the effect of liquidity on unemployment duration and reemployment wages are comparable to the effect of UI on unemployment duration and wages.[20] In the empirical UI literature, a 10% increase in the UI replacement rate for six months typically increases unemployment duration by 3.5 to 7 days (e.g., see Nakajima (2012) for a review of recent estimates from the literature). Our estimates imply that high-ETV individuals on average borrow 2.1% of the housing equity, or 5% (=2.1%*451.1/197) of the annual wage, and

[19] In Table III, column (6), the effect on the employment rate for liquidity-constrained individuals is −0.0012, which is equivalent to an increase in unemployment duration of 0.44 (0.0012*365) days. Since the unemployment rate was around 10% around 1992, an increase in unemployment duration of 5.4 days among the unemployed translates into an average increase in unemployment duration of 0.54 days.

[20] In Denmark, the unemployment system is two-tiered. One may voluntarily pay a fee to be a member of a UI fund and be insured against unemployment. If not eligible for UI benefits, the worker may 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.

Table VII

Effects of Mortgage Reform on Unemployed and Employed Workers

Panel A reports estimates from cross-sectional regressions on unemployed workers in 1991, and Panel B reports estimates from OLS regressions on the sample of homeowners who were employed at the time of the reform in 1992. In Panel A, unemployment duration is measured in months. The change in wages between jobs is the change in log wage between pre- and postunemployment jobs. Column (2) estimates a Cox proportional hazard model. The main right-hand-side variables are the equity-to-value ratio (ETV) in 1991 and ETV interacted with an indicator for 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, log wage before unemployment, and postreform dummy interacted with the dummy variable for low liquid assets. In Panel B, variables are as defined in Table III, and regressions control for year fixed effects interacted with fixed effects for birth cohort, wealth decile, education level, partner indicator, gender, and children indicator, each measured in 1991, as well as person fixed effects, municipality-year fixed effects, and postreform dummy interacted with the dummy variable for low liquid assets. Standard errors are clustered at the municipality level (there are 275 municipalities) and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
	Panel A. Unemployed Workers		
Dependent Variable	Unemployment Duration	Hazard Rate	Wage Change between Jobs
1(ETV91>0.2)	0.0546	−0.0726***	−0.0177**
	(0.0495)	(0.0187)	(0.0086)
1(ETV91>0.2)*Low Liquidity	0.1238**	−0.0589***	0.0538***
	(0.0538)	(0.0225)	(0.0088)
Preunemployment wage	Yes	Yes	Yes
Age dummies	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Number of observations	42,780	42,780	42,780
	Panel B. Employed Workers		
	Log Wage	Normalized Earnings	Employment Rate
Post*1(ETV91>0.2)	−0.0035	−0.0021	0.0015**
	(0.0022)	(0.0022)	(0.0007)
Post*1(ETV91>0.2)*Low Liquidity	0.0207***	0.0175***	−0.0014*
	(0.0032)	(0.0027)	(0.0008)
Person FE	Yes	Yes	Yes
Municipality*Year FE	Yes	Yes	Yes
Observables*Year FE	Yes	Yes	Yes
Number of observations	7,024,978	7,629,624	7,629,624

increase unemployment duration by 3.9 days on average. The elasticity of unemployment duration to borrowed housing equity is on the lower end of the elasticity of unemployment duration to UI benefits, since the former is a debt that needs to be repaid and does not have the disincentive effect of UI benefits. On the other hand, the effect on reemployment wages is on the higher end of the elasticity of reemployment wages to UI benefits. The UI literature typically finds insignificant effects (e.g., Lalive (2007), Van Ours and Vodopivec (2008), Johnston and Mas (2018)) or smaller positive effects (e.g., 0.5% increase in wages in Nekoei and Weber (2017)). The combination of a smaller increase in unemployment duration and a larger increase in reemployment wages in our setting compared to the UI literature is consistent with the negative association between UI duration and UI wage effects in Nekoei and Weber (2017).

Our results for unemployed individuals are also similar to Herkenhoff, Phillips, and Cohen-Cole (2019). They find that an increase in unused revolving debt of 10% of one year's income leads to an increase in unemployment duration of 2.3 to 3.7 days and an increase in reemployment wages of 0.61% to 1.34%. Our effects on unemployment duration and wages are slightly larger than an increase in revolving debt of 10% of prior annual income because (i) the maximum housing equity that homeowners can extract is usually much larger than 10% of annual earnings, although most homeowners do not borrow the maximum amount, and (ii) the interest rate on a home equity loan is lower than the interest rate on credit cards, which allows individuals to borrow more and stay unemployed longer without being worried about delinquency.

Since unemployed workers account only for about 10% of the sample, the higher reemployment wages of unemployed workers can explain less than half of the positive wage effects of the reform.[21] We explore the effects of the reform on the wages of employed workers next.

C. Effects on Employed Workers

In this section, we show that the reform increased wages of employed workers and explore the potential channels. As we have discussed in the conceptual framework in Section I.B, better insurance against negative income risks could increase the wages of employed workers by encouraging workers to switch to better-paid jobs and better-paid firms. A relaxation of liquidity constraints could also allow workers to invest more in human and physical capital and find better jobs.

In Panel B of Table VII, we rerun the baseline specification restricting attention to workers who were employed with positive wages at the time of the reform in 1992. We find a slightly smaller but positive and significant effect on wages for employed workers. For example, for workers with low liquidity,

[21] For example, if we consider the triple-differences estimates in column (3) of Table VII, Panel A, the effect on the reemployment wages of low-liquidity high-ETV individuals is 3.6%. When multiplying by the unemployment rate in 1992 (12%), this explains roughly 20% to 30% of the positive wage effects in column (4) of Table III.

having access to housing equity increases wage levels by 1.7% and total earnings by 1.5%. We also find a nearly zero effect of access to housing equity on the employment rate for low-liquidity individuals, consistent with the result in Section IV.B that the negative effect on the employment rate is driven mostly by unemployed workers.

We next explore the channels through which access to housing equity affects the wages of employed workers. We first examine whether workers are more likely to switch jobs after the reform allowed them to borrow against housing equity. Panel A of Table VIII shows difference-in-differences estimates for workers who were employed in 1992. The dependent variable in column (1) is an indicator variable that equals 1 if the worker switches employer. Liquidity-constrained individuals with an ETV higher than 0.2 are 0.6% more likely to switch jobs after the reform, while nonconstrained individuals do not significantly change their job-switching behavior.

In columns (2) and (3), we split job mobility into 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. Liquidity-constrained individuals with a high ETV are 0.8% more likely to move up and 0.2% less likely to move down. The differences are not statistically significant for nonconstrained 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 capture a firm's wage level using two measures. The first is coworkers' average wage within the establishment. The second measure is establishment wage fixed effects (Abowd, Kramarz, and Margolis (1999)), specifically we estimate a two-way fixed-effects model as in He and le Maire (2023) for all workers (including nonhomeowners) and all establishments for the period 1980 to 2000, and use the estimated establishment fixed effects as a measure for the establishment-specific wage premium. Columns (4) and (5) show that workers with a high ETV move to firms that pay higher wages after the mortgage reform. Liquidity-constrained individuals with a high ETV in 1991 are employed in establishments that pay 0.4% higher average wages to coworkers and 0.3% higher wage premiums. In column (6), we consider whether workers move to better occupations following the reform. The dependent variable is the average real wage of a worker's occupation. We find that liquidity-constrained workers with a high ETV move to occupations with relatively higher wages.

In Panel B of Table VIII, we investigate several mechanisms driving the upward job mobility of employed workers. In the first two columns, we consider whether the relaxation of credit constraints leads to better jobs by encouraging workers to invest more in human capital accumulation. Similar to firms cutting investment when financially constrained, individuals may invest less in human capital when credit constrained (Sun and Yannelis (2016), Fos, Liberman, and Yannelis (2017)). We measure investment in human capital using the incidence and duration of job training. Job training can help work-

Table VIII

Mechanisms of Wage Growth for Employed Workers

This table reports estimates from OLS regressions on the sample of homeowners who were employed at the time of the reform in 1992. In Panel A, the dependent variable is an indicator for changing employer in column (1), an indicator for changing employer and getting higher wages in column (2), an indicator for changing employer and getting lower wages in column (3), the average wage of coworkers in column (4), the AKM establishment fixed effect of the employer (estimated from two-way fixed effect regressions with worker fixed effect and establishment fixed effect as in He and le Maire (2023)) in column (5), and the average log wage at the occupation level in column (6). In Panel B, the dependent variable is an indicator for vocational training in column (1), duration of vocational training in months in column (2), an indicator for purchasing a new passenger car in column (3), an indicator for owning at least one car in column (4), log travel distance from home zipcode to work municipality in column (5), and an indicator for working in a different zipcode than the home address in column (6). The main right-hand-side variables are the equity-to-value ratio (ETV) in 1991 interacted with an indicator for the postmortgage reform period, and interactions of ETV in 1991, postreform dummy, and an indicator variable equal to 1 if the individual has liquid assets less than one month's disposable income in 1991. Control variables are the same as the baseline regression. Standard errors are clustered at the municipality level (there are 275 municipalities) and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Job Mobility			
	Switch Firm	Move Up	Move Down	Coworker Average Wage	AKM Establishment FE	Occupation Average Wage
Post*1(ETV91>0.2)	0.0010	0.0008	0.0002	−0.0002	−0.0005	−0.0013
	(0.0008)	(0.0007)	(0.0005)	(0.0007)	(0.0006)	(0.0008)
Post*1(ETV91>0.2)*	0.0051***	0.0075***	−0.0024***	0.0045***	0.0039***	0.0019*
Low Liquidity	(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	7,024,978	7,024,978	7,024,978	6,954,180	6,992,466	7,024,978

			Panel B. Investment in Human Capital and Physical Capital			
Dependent Variable	Training	Duration of Training	New Car Purchase	Whether Have a Car	Log Distance from Home to Work	Work in a Different Zipcode
Post*1(ETV91>0.2)	0.0026***	0.0024	−0.0002	−0.0016	−0.0086	−0.0003
	(0.0009)	(0.0018)	(0.0004)	(0.0024)	(0.0122)	(0.0018)
Post*1(ETV91>0.2)*	0.0058***	0.0042*	0.0028***	0.0110***	0.0435***	0.0074***
Low Liquidity	(0.0015)	(0.0025)	(0.0005)	(0.0029)	(0.0143)	(0.0027)
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	6,257,571	6,257,571	7,024,978	7,024,978	6,678,560	3,138,078

ers update their skills or learn skills of another occupation when they switch careers. Although the government subsidizes training in Denmark and the direct cost to workers is low, there is an indirect cost in terms of forgone earnings while participating in training during work hours. We find that for liquidity-constrained individuals, being able to extract their housing equity increased their probability of training by 0.8 percentage points (a 5% increase relative to the average probability of 15%) and increased the average duration of training by 0.007 months, or 0.2 days, consistent with the relaxation of liquidity constraints helping workers overcome the costs of training.

In addition to human capital, individuals may invest more in physical capital such as cars when they have more liquidity. For example, during the Great Recession, liquidity constraints led to a sharp decline in car purchases in the United States (Benmelech, Meisenzahl, and Ramcharan (2017), Di Maggio et al. (2017)). We collect information on cars from the Central Register of Motor Vehicles (CRM), which contains information about the entire population of cars registered with Danish license plates and exact registration and deregistration dates. In column (3) of Panel B, the dependent variable is a dummy variable that equals 1 if an individual purchased a new car in that year. The stock of cars is registered only as of 1992. To avoid bias due to missing cars bought between 1987 and 1991 but deregistered before 1992, we set the variable to zero before 1992.[22] For liquidity-constrained individuals with a high ETV, the reform led to an increase in the probability of purchasing a new car in a given year by 0.26 percentage points, which is a 7% increase relative to the average probability of 4%. However, there is no effect on car purchases for high-liquidity individuals. In column (4), we use a dummy variable indicating car ownership as the dependent variable, and again find a positive and significant effect for low-liquidity individuals and no effect for high-liquidity individuals.

In the last three columns of Panel B, we examine whether individuals with a high ETV choose jobs further away from home following the reform. Buying a car enables individuals to commute longer distances. In addition, searching more extensively for high-paid and low-match-rate jobs can be associated with finding jobs further away from home. Being able to commute further can sometimes bring sizable wage gains. Studies on microcredit in developing countries identify investment in mobility as one of the most important investments people make when credit constraints are relaxed (Karlan and Zinman (2010), Kaboski and Townsend (2012)). Doornik et al. (2021) find that access to credit for buying motorcycles increased distance between home and work by 14% to 20% and increased wages by 18% over a 10-year period.

In our data, we observe zip codes of home addresses and municipalities of work addresses for nearly all workers. We use the Open Source Routing Machine and OpenStreetMap to calculate actual travel distance (instead of airline distance) between home and work addresses. Column (5) uses the log

[22] For example, if a car is registered before 1992 but still owned in 1992, we will be able to see the registration date in our data. However, if a car is deregistered before 1992, we will not observe it in the data. We get similar results without setting the variable to zero.

distance between home zip code and work municipality as the dependent variable (we find similar results when using the distance between home zip code and work zip code, although work zip code is available only for 45% of workers). The average travel distance between home and work is 12 km. After 1992, liquidity-constrained individuals with high ETV increased their commuting distance by 4%. In the last column, we find that liquidity-constrained individuals are also more likely to work in a different zip code than the zip code in which they.

Overall, our results indicate that being able to borrow from housing equity allowed liquidity-constrained individuals to move to jobs with higher salaries, in higher-paying firms, further away from home. These findings are consistent with the insurance provided by home equity loans enabling individuals to search for higher-wage and riskier jobs. We also identify investments in training and cars as two important mechanisms for the upward job mobility.[23]

D. Alternative Mechanisms

D.1. Financial Distress and Debt Overhang

One potential alternative explanation is that a high ETV is associated with more mortgage debt and higher risk of financial distress. In particular, there are three nonmutually exclusive channels. First, individuals with low housing equity have higher risk of default, which could depress their earnings. When housing equity is negative, households may engage in "strategic" default (Mayer et al. (2014)), which 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 (2021)).[24] However, Ganong and Noel (2021) show that pure strategic default is rare, and most mortgage defaults are driven by negative cash flow shocks, suggesting that financial distress could also impact homeowners with positive housing equity. Although mortgage default is very rare in Denmark, the stress associated with risks of financial distress can reduce worker productivity or job search among the unemployed, especially for liquidity-constrained individuals. In Panel A of Internet Appendix Table IA.X, we exclude individuals who had negative ETV at some point between 1991 and 1997, which represents 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 for individuals with ETV above 0.2 in 1991 compared to individuals with ETV below 0.2. In Panel B, we further restrict attention to homeowners who always had ETV above 15% between 1991 and 1997 and for whom the risks of financial distress are very low, and we are left with about

[23] We confirm that the results in Tables VII and VIII are robust to instrumented ETV using the IV strategies in Section III.D.

[24] Direct effects on credit scores from distress could hurt labor market outcomes because of employer screening (Bos, Breza, and Liberman (2018)), although Dobbie et al. (2020) show that the removal of bankruptcy flags does not significantly affect labor income.

55% of the sample. In this subsample, we still find a positive and significant relationship between ETV in 1991 and wage growth for liquidity-constrained individuals. In Panel C, we consider high-liquidity individuals who had negative net worth (i.e., total assets minus total liability) in 1991, which is a key indicator for financial distress (Brown and Taylor (2008), Kuhnen and Melzer (2018)). For this subsample of individuals who have high risks of financial distress but are not liquidity-constrained, we find no effect of ETV on wages or employment rate.

Second, negative housing equity may also prevent households from moving geographically and searching for jobs widely, known as "housing lock" (Brown and Matsa (2020)). Housing lock is unlikely when a household has positive home equity since the home can be sold without needing substantial additional resources, and we show that our results are identical when excluding households with negative housing equity. In addition, we find no effect of the reform on geographical mobility across municipalities.

A third channel is that high debt levels could change incentives to work caused by household protection under limited liability, referred to as "household debt overhang" by Bernstein (2021). Theoretically, Donaldson, Piacentino, and Thakor (2019) argue that highly levered households would reduce their labor supply when a portion of their marginal income is transferred to a lender via increased expected liability repayment. Empirically, Bernstein (2021) and Di Maggio, Kalda, and Yao (2019) show that high levels of mortgage debt and student loans reduce labor supply and wages. To account for the nonlinear relationship between debt levels and labor supply, we control for deciles of the liability-to-income ratio in 1991 interacted with year fixed effects in Internet Appendix Table IA.XI. Since debt levels are correlated with ETV, controlling for debt levels absorbs part of the variation in ETV. Nevertheless, we find a positive and significant effect on wages. We also find a negative and significant effect on the employment rate, which is inconsistent with the debt overhang channel.[25]

Taken together, our results suggest that financial distress and debt overhang are unlikely to drive our results. It is also important to note that the financial distress channel also applies to prereform years when homeowners were unable to extract their housing equity, and our placebo test using prereform years finds no significant difference in wages between high-ETV and low-ETV individuals, implying a limited role for financial distress conditional on our controls.[26]

[25] We also find a zero effect on the number of hours worked for high-ETV individuals. Information on number of hours worked comes from the mandatory pension fund, ATP, which collects a small mandatory pension fund payment from all workers. The payment is a step function in the number of hours worked: (i) no payment when working 0 to 9 hours per week, (ii) 1/3 of full-time payment when working 9 to 18 hours per week, (iii) 2/3 of full-time payment when working 18 to 27 hours per week, and (iv) full-time payment when working at least 27 hours per week.

[26] Debt overhang or financial distress would also imply that households with a low ETV would fare worse than renters who do not have any mortgage debt, but we show that low-ETV individuals and renters had similar wage growth, and both had lower wage growth than high-ETV individuals.

D.2. Entrepreneurship

Another alternative explanation for our findings is that the option to borrow against housing equity encourages workers to start up their own business and earn more. Schmalz, Sraer, and Thesmar (2017) show that an increase in the value of housing collateral leads to a higher probability of becoming an entrepreneur. Jensen, Leth-Petersen, and Nanda (2022) studied the same mortgage reform as our paper and find that homeowners with a high ETV in 1991 are more likely to become entrepreneurs. We find that individuals with an ETV higher than 0.2 in 1991 have a 0.1% higher probability of becoming self-employed, which is consistent with Jensen, Leth-Petersen, and Nanda (2022). The effect 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 entrepreneurs would have to be more than five times higher than the earnings of other jobs.

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

D.3. Productivity

Liquidity could affect labor market performance by increasing worker productivity. Bernstein, McQuade, and Townsend (2021) show that a decline in housing wealth is associated with lower productivity for innovative workers. One explanation put forth by the authors is that a decline in housing wealth could lead to a reduction in consumption (Mian, Rao, and Sufi (2013)), specifically, a decreasing in 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 loans are more likely to pay for home services that may free up additional time that they can allocate to working and innovating, which may increase their productivity. Having a liquidity buffer through housing equity may also reduce workers' level of anxiety and stress and boost their productivity at work (Engelberg and Parsons (2016), Kaur et al. (2021)).

In Internet Appendix Table IA.XIII, 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 a high ETV have slightly lower within-job wage changes after 1992. In column (2), we interact ETV with liquidity and find a positive yet small and statistically insignificant effect on wages within jobs for liquidity-constrained individuals. Splitting the sample by education level shows that this positive effect on wages is driven entirely by college-educated workers. The results suggest that the productivity channel can explain only a small part of the wage gains of college-educated workers and cannot explain the wage gains of noncollege-educated workers.

A related channel is that households use the additional liquidity to pay for childcare, which frees up time to be more productive in an existing job or search for a new job. This channel is less likely in Denmark, where parental leave is long (about a year) and childcare is subsidized and provided by local governments. In Internet Appendix Table IA.XIV, we split the sample by the age of the children in 1991 to examine whether childcare explains the positive wage effects. In column (1), we consider households with children under the age of 18 in 1991. We find a significant increase in wages by about 1.6% for liquidity-constrained households. Among these households, we find similar wage effects for households with young children under the age of 6 (column (2)) and households with older children (column (3)). We further find that the wage effects are larger for women (column (4)) than men (column (5)), consistent with the reform allowing women to benefit disproportionately from buying child support. However, the difference is not statistically significant. In columns (6) to (8), we show that households with no children under 18 experienced a significant wage gain of over 2% if they have an ETV above 0.2 and are liquidity-constrained, with the effect almost the same for men and women. This suggests that childcare is unlikely to be the primary driver of our baseline results.

V. Conclusion

Housing assets constitute the majority of wealth for most households, but they are highly illiquid, and many individuals are liquidity-constrained despite having a large amount of housing wealth. For example, Kaplan, Violante, and Weidner (2014) document that over 20% of U.S. households are wealthy hand-to-mouth. Boar, Gorea, and Midrigan (2022) estimate that four-fifths of homeowners in the United States are liquidity-constrained. In this paper, we exploit a natural experiment in Denmark that allowed homeowners to borrow against housing equity. We find that the expanded credit access increased earnings and job quality for liquidity-constrained individuals.

While it has been well established that relaxing household liquidity constraints can help stabilize consumption and employment in recessions through the aggregate demand channel (Agarwal et al. (2017), Auclert, Dobbie, and Goldsmith-Pinkham (2019), Ganong and Noel (2020)), our results show that providing liquidity to households can have an additional positive effect on earnings and output through a labor market search channel. Since our setting focuses on the period around the 1992 recession in Denmark, our findings are particularly relevant for policies that aim to provide short-term liquidity to constrained households during economic downturns, including mortgage refinancing and restructuring, maturity extensions, and expansion of unemployment benefits. Our results suggest that these policies can allow workers to find better job matches and invest in valuable capital, potentially improving welfare. Moreover, since Denmark has one of the most generous UI benefit systems among OECD countries, the effects are likely to be even more pronounced in countries with less generous UI systems. Further understanding how other policies that relax the liquidity constraints of households affect labor market outcomes in other settings, and how this interacts with the aggregate demand channel, represents a fruitful area for future research.

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Supporting Information

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

Appendix S1: Internet Appendix.
Replication Code.