

# The price of leverage: Learning from the effect of LTV constraints on job search and wages

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## Abstract

Does household leverage matter for job search, labor market matching, and wages? Exploiting the introduction of a macroprudential borrowing restriction, we study how a forced reduction in leverage affects displaced homeowners and find that it raises wages by 3.9 percentage points following an unemployment spell. Lower leverage enables longer job search, leading workers to switch to other occupations and industries and take jobs in firms offering higher wage premia. Wage improvements are stronger among individuals who benefit more from improved job search, such as the young. Our findings highlight that policies limiting household leverage generate positive unintended labor market outcomes.

**JEL classification:** E21, G21, G51, J21.

**Keywords:** Household leverage, household debt, job displacement, job search, macroprudential policy, wages.

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# 1 Introduction

High levels of household leverage can pose a challenge to the economy through several channels. They can fuel housing booms, weaken financial stability, and predict lower GDP growth and higher unemployment.<sup>1</sup> In the wake of the Global Financial Crisis, many countries have therefore adopted policies to restrict household leverage. Such policies face the challenge of adequately trading off the costs of restricting borrowing in good times against the benefits of a less pronounced economic decline in bad times. These trade-offs have sparked a debate about the effectiveness and undesirable side effects of measures to restrict household borrowing (DeFusco et al., 2020; Acharya et al., 2022; Tzur-Ilan, 2023; Peydró et al., 2024; van Bakkum et al., 2024). While existing research primarily examines the effects of household leverage restrictions on the housing market and financial stability, we contribute to this debate by studying a previously unexplored transmission channel of such borrowing restrictions through household balance sheets to job search and labor market outcomes.

Prior literature has found that negative home equity, mainly caused by a decline in home prices, may impair geographical labor mobility and that increased credit availability during unemployment may help workers to improve their job search, similar to unemployment insurance (e.g., Bernstein (2020); Gopalan et al. (2020); Herkenhoff et al. (2024)). We extend this literature by showing how a regulation-driven decline in household leverage affects job search duration and broadness after displacement, and subsequent wages—comparable to the theoretical mechanism in Ji (2021).

To study this channel, we exploit the introduction of a loan-to-value ratio restriction in Norway and examine the job search behavior of workers who become displaced. Being arguably the most significant negative income shock workers can experience, job displacement is a primary concern for financial stability, as more than two-thirds of US mortgage defaults are driven exclusively by negative income shocks (Ganong and Noel, 2020). Since high

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<sup>1</sup>See Reinhart and Rogoff (2008), Mian and Sufi (2011), Schularick and Taylor (2012), Adelino et al. (2016), Favilukis et al. (2017) and Mian et al. (2017).

household leverage has been a common characteristic in recent recessions and the responses of displaced workers disproportionately shape the path of the broader economy, studying the effects of household leverage and relevant policies on labor market outcomes of displaced workers could be highly relevant for policymakers.

We find that the introduction of an LTV restriction improves the wages of displaced workers in their new jobs. Specifically, we find that the 25 percent decline in a worker's debt-to-income ratio leads to a relative wage improvement of 3.9 pp following displacement. We break down the mechanism behind the rise in wages and document that the reduction in leverage affects job search behavior in three ways. First, workers who are forced to constrain their leverage prolong the duration of their job search by approximately 1.7 months. Second, they become more likely to switch to other occupations and industries. Third, these workers find jobs in firms that pay a higher wage premium. In addition, the improvement in wages does not wane over time and is not associated with a rise in income volatility. Finally, we show that the negative relationship between household leverage and wages also exists in broader samples and the entire population. Overall, our findings indicate that household leverage creates constraints on job search and that a policy restricting leverage relaxes these constraints, enabling job-seeking workers to attain higher wages in their new jobs.

We estimate the effect of the borrowing restriction on labor market outcomes in a difference-in-differences setting that has two parts. The first part relates to the fact that the LTV restriction is applied to all new homebuyers. Due to this policy feature, there is no variable distinguishing the affected workers, who take on smaller mortgages as a result of the restriction, from the unaffected workers, who obtain the same mortgage regardless of the restriction. To make this distinction, we use data on the characteristics and LTV ratio decisions of homebuyers who bought their homes before the restriction was introduced. Since those homebuyers were able to take on mortgages with LTV ratios either above or below the cap, we can correctly determine whether they would be affected by the restriction, i.e., if their unrestricted LTV ratio is above the restriction threshold, they would be affected by

the restriction, and vice versa. We use these correctly classified pre-restriction homebuyers to assign our entire regression sample to a treatment or a control group by employing a random forest (RF) algorithm, a machine learning method (Abadie, 2005). The RF algorithm matches workers in the regression sample to the homebuyers before the restriction using a rich set of individual characteristics. Therefore, a worker in the regression sample is classified as treated if homebuyers with similar characteristics have initial LTV ratios above the cap before the restriction, and other workers are classified as controls. While the RF algorithm we apply has superior classification ability, we show that our main findings are robust to applying other commonly used treatment classification methods, i.e., one based on deposit holdings, a linear probability model, and bunching in the LTV distribution.

The second part considers a potential selection effect that the LTV restriction may induce. After the introduction of the restriction, some workers, for instance, the high-skilled ones, may become more inclined to search for and switch jobs to increase their earnings, potentially creating a selection bias in our estimates. We address this concern by using only displaced workers whose job search is due to job loss triggered by a mass layoff and not by their own characteristics. Furthermore, we restrict our sample to those displaced workers who bought a house within 12 months before their job loss to rule out that unobserved differences in savings rates affect household leverage and housing wealth between the time of home purchase and job loss. To generalize our main findings, we relax our sampling criteria gradually until we reach the whole population in additional analyses and document that our results do not change. With these two parts, our difference-in-differences setting compares those displaced workers who recently bought a house and are likely affected by the LTV restriction (treatment group) to those unaffected by the restriction (control group). We obtain three main results.

First, we find that the policy-induced decline in household leverage affects the labor market outcomes of affected workers. These workers realize higher wage growth between their job from which they are displaced and their next job. In particular, we find that a 25 percent decline in workers' DTI ratio leads to a 3.9 pp smaller decline in wages, compared

to the sample mean wage decline of 9.2 pp. The estimated improvement in wages is robust to controlling for a range of fixed effects and several sample refinements. We also verify that the effect is not driven by workers’ endogenous home purchase decisions, which could have introduced a sample selection effect. Consistent with the absence of such selection effects, we find that the restriction does not change the observable characteristics of the treatment group or the rate of transition into homeownership. In addition, we identify workers who do not have sufficient savings to cover a down payment in the pre-restriction period. Such workers are the underlying source of the selection bias, as they could not purchase a home after the restriction. Reassuringly, estimating our models without these workers yields the same results. Moreover, our results are robust to excluding the workers who make the most of the unemployment benefits and labor market protection.

Second, we establish that the improvement in starting wages stems from the mitigation of constraints on job search that higher leverage had created. Theoretically, higher leverage should tighten the constraints on job search since it increases the default probabilities due to larger debt-related payments, leading workers to shorten the duration or narrow the scope of their job search to avoid costly defaults (Chetty and Szeidl, 2007; Herkenhoff, 2019; Ji, 2021).<sup>2</sup> In line with this theoretical prediction, the leverage restriction enables workers to extend their job search by 1.7 months, suggesting that lower leverage reduces the pressure on displaced workers to find or accept a new job quickly. Moreover, displaced workers find job matches with firms that pay higher wage premiums (Abowd et al., 1999). This improved matching accounts for approximately 30 percent of the estimated wage gain. Workers also broaden the scope of their job search and are approximately 25 percent more likely to change their occupation or find a new employer in another industry. Changes in geographical labor mobility, spousal income, or working hours do not drive these results. The mechanism we identify, while related, differs from what earlier research has found about the effect of unemployment insurance (UI) and access to credit during unemployment on job matching.

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<sup>2</sup>Donaldson et al. (2019) show that under limited liability, household leverage can create a debt-overhang problem that reduces workers’ willingness to work. However, mortgages in Norway are full recourse.

Debt-reducing measures are not prone to moral hazard, unlike UI (([Hansen and İmrohoroglu, 1992](#))), nor do they raise loan default risk, as increasing access to credit does.<sup>3</sup>

Third, we provide further support for the presence of the above mechanism by exploiting the heterogeneity across the sample. Consistent with a mechanism that works through relaxing constraints on job search, we find that workers below the median age, having a shorter job tenure with the previous employer, or with higher education, drive the improvement in wages. This is in line with the notion that it is easier for workers who are younger or have higher education to invest in the human capital required for a different occupation or industry. Longer job tenure with the same firm also tends to make human capital more firm-specific and limit the value of a longer and broader job search. These findings indicate that workers who have more potential to benefit from a less constrained job search indeed experience a larger effect. Further heterogeneity tests indicate that the improvement in wages is particularly large for low-income workers.

Last, we find that the positive effect on wages is persistent over the four-year post-displacement period we observe. Treated workers also enjoy lower wage volatility during these four years, indicating that the rise in wages is not attributable to taking jobs with higher wages but to larger discontinuity risk. Moreover, we document that the negative relationship between household leverage and wages also holds in broader samples, where we cannot exploit the same exogenous variation in leverage for identification purposes.

The findings in our paper speak to three strands of the literature. The first one studies the effects of household balance sheets on job search.<sup>4</sup> This literature has found that a decline in home prices induces negative home equity, mainly for households with high leverage. Negative home equity, in turn, limits such households' labor mobility as selling their homes becomes costly for them ([Bernstein and Struyven, 2022](#); [Brown and Matsa, 2019](#); [Gopalan](#)

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<sup>3</sup>We detail the differences between the mechanism in this paper and that in UI and access to credit during unemployment at the end of Section 5.3.

<sup>4</sup>A related strand of literature investigates how household balance sheet influences labor supply decisions (e.g., [Chetty and Szeidl \(2007\)](#); [Rothstein and Rouse \(2011\)](#); [Bos et al. \(2018\)](#); [Li et al. \(2020\)](#); [Di Maggio et al. \(2024\)](#); [Zator \(2019\)](#); [Cespedes et al. \(2020\)](#)).

et al., 2020; Bernstein, 2020).<sup>5</sup> In addition, this literature has documented that access to credit via credit cards or home equity loans during a job search can act like unemployment insurance and enable workers to improve search (Chetty, 2008; He and le Maire, 2023; Herkenhoff et al., 2024; Kumar and Liang, 2024). We contribute to this literature by, to the best of our knowledge, providing the first causal evidence for the effects of macroprudential borrowing constraints on job search behavior and wages.<sup>6</sup> We identify a negative effect of household leverage on job search that is consistent with the mechanism in Ji (2021) rather than the one with the presence of a debt overhang channel as in Donaldson et al. (2019). Different from prior research, in this mechanism, risk-averse workers prefer to avoid costly defaults and compensate for the influence of higher debt on their default risk by accepting earlier job offers, shortening search time, and consequently lowering their wages.

Second, we add to the debate on the costs and benefits of macroprudential policies as well as the underlying externalities of credit (Badarinza, 2019). Being widely used since the Global Financial Crisis, these policies can potentially curb credit booms and improve financial stability (Cerutti et al., 2017; Dávila and Korinek, 2018; van Bakkum et al., 2024; de Araujo et al., 2019; Peydró et al., 2024) but can also generate adverse side effects, such as reducing access to housing (DeFusco et al., 2020; Acharya et al., 2022; Aastveit et al., 2020; Tzur-Ilan, 2023).<sup>7</sup> We contribute to this discussion by documenting a previously overlooked and quantitatively important channel through which these policies affect labor markets and the real economy.<sup>8</sup> Discussions on the welfare implications of macroprudential policies may therefore benefit from incorporating the unintended effects we document.

Third, our paper contributes to the literature on how household debt and access to credit affect labor markets through a demand channel. This channel starts with the detrimental

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<sup>5</sup>Fonseca and Liu (2023) study the relationship between mortgage rates and labor mobility.

<sup>6</sup>Bednarzik et al. (2017); Meekes and Hassink (2019), and Fontaine et al. (2020) document a correlation between household balance sheets and labor market outcomes.

<sup>7</sup>See Farhi and Werning (2016), Dávila and Korinek (2018) and Badarinza (2019) for theoretical justifications for macroprudential policies.

<sup>8</sup>In related work, Pizzinelli (2018) develops a life-cycle model with LTV and LTI restrictions to study second earners' labor supply and finds no effect of an LTV restriction on female employment.

effect of household leverage on credit availability via a deterioration in financial stability or in collateral values that subsequently triggers deleveraging by households (Reinhart and Rogoff, 2008; Schularick and Taylor, 2012; Corbae and Quintin, 2015; Adelino et al., 2016). Such deleveraging is accompanied by a cut in household spending (Eggertsson and Krugman, 2012; Mian et al., 2013; Guerrieri and Lorenzoni, 2017), which weakens aggregate demand and increases unemployment (Mian and Sufi, 2014; Mian et al., 2017). We complement these studies by documenting a *direct* effect of household leverage on labor markets through job search and matching quality. The channel we document contributes to understanding why sluggish recoveries occur after recessions with high household leverage (Mian et al., 2017).<sup>9</sup>

The rest of the paper is organized as follows: Section 2 provides information about economic conditions in Norway, Section 3 describes the data and variables constructed, Section 4 explains the empirical strategy, Section 5 presents the impact of the LTV constraint on household finances and labor market outcomes, and Section 6 concludes.

## 2 Institutional background

This section discusses the institutional details of labor and housing markets and the macroeconomic environment in Norway that are relevant to our paper.

**Housing market** Norway’s housing market can be characterized by its high home ownership ratio. Since the Second World War, the Norwegian government has supported home ownership through several policies, such as tax breaks for homeowners. Due to these policies, the homeownership rate in Norway has been stable at slightly above 80 percent since the beginning of the 1990s, one of the highest among advanced countries. This high homeownership rate is coupled with full-recourse mortgages, most of which have floating rates. The average mortgage amortization amount is approximately 30 percent of the households’ net

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<sup>9</sup>Also see Jacobson et al. (1993); Couch and Placzek (2010); Davis and Von Wachter (2011); Lachowska et al. (2020) for the decline in earnings after a job loss.



income. The default rate on these mortgages is low, which can be explained by the high costs of default. In addition to non-pecuniary costs, such as involuntary relocation, a default creates an additional financial burden on defaulters in two ways. First, banks apply fees for delayed payments that are added to the total mortgage payments. Second, seized real estate usually sells at a discount that can be up to 20 percent, to be covered by the defaulters.

**Labor market regulation** The labor market in Norway is governed by the Working Environment Act and the Labor Market Act. The Working Environment Act sets standards for working conditions and procedural rules that need to be followed when an employer wishes to terminate an employment relationship. Norwegian law recognizes a special status for collective redundancies—situations where notice of dismissal is given to at least 10 employees within a 30-day period. In such situations, the employer does not have to provide personal-specific reasons to the workers. The notification period for job termination depends on the worker’s job tenure and age. The Act states that a minimum period of one month’s notice shall be applicable to both workers and employers, gradually increasing to six months with worker age. Most workers in our sample have one month’s notice.

Displaced workers in Norway can receive 62 percent of their previous income from unemployment insurance (UI) benefits, which is approximately equal to the OECD average of 60 percent. This UI replacement rate suggests that, on average, job displacement reduces households’ available income net of mortgage payments by slightly more than 50 percent, as the mortgage amortization amount is 30 percent of the income before job loss (i.e., after paying their mortgage, households have 70 percent of their net income before job loss and 32 percent of their net income after the job loss). Neither the coverage ratio nor the duration changed during our sample period.<sup>10</sup>

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<sup>10</sup>Displaced workers can receive 62.4 percent of their previous income up to six times the National Insurance Scheme’s basic amount, which was NOK 75,641 (USD 12,712) annually in 2010. Unemployment insurance can be obtained for up to two years, depending on workers’ previous earnings. Workers need to earn at least 1.5 times the basic amount over the previous 12 months or, on average, more than three times the basic amount over the past 36 months to be eligible for unemployment benefits. To be entitled to the maximum of 104 weeks of unemployment benefits, a person had to earn an income of at least twice the basic amount during the previous 12 months or twice the basic amount on average during the previous 36 months.

**Norwegian economy and macroprudential policy framework** Norway’s economy has shown stable economic growth, with inflation and average unemployment below 4 percent during the past 30 years. For instance, during the Global Financial Crisis (GFC), its GDP fell by only 1.7 percent. Reflecting this stability, house prices have nearly tripled since 2000. Norwegian households’ debt to GDP ratio has simultaneously grown from 50 to 105 percent (Figure A1).<sup>11</sup> Due to the steep rise in house prices and household indebtedness and the resulting financial stability risks, Norwegian policymakers implemented a series of macroprudential policies that we describe in greater detail in Section 4. Under the Financial Institutions Act (*Lov om finansforetak og finanskonsern*, henceforth FIA) Finanstilsynet (the Financial Supervisory Authority—FSA) advises the Ministry of Finance (MoF) on desirable regulations, while decisions on regulations are made by the MoF.

### 3 Data and sample construction

We combine several official Norwegian population registers. Each data set covers the entire adult population of Norway, and we link the data sets with unique, anonymized, personal identifiers. We introduce the data sets below and describe how we construct our sample and variables.

**Data sets** We obtain the labor market data for our study from the official employer-employee register administered by the Norwegian Labour and Welfare Administration (NAV). All employers and contractors are obliged by law to report their employees and details on the employment relationship to this register that tracks for which employer a worker works, what occupation she held, what wages were paid, the job start and termination dates, as well as the geographic location of the workplace. We complement this labor market information

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Only a small number of workers in our sample have longer unemployment duration than two years and, as explained in Section 5.2, removing such workers does not change our results, alleviating concerns regarding the influence of unemployment insurance on our results.

<sup>11</sup>Figure A1 also illustrates other macroeconomics conditions in Norway.

with data from the population register and official tax records. The population register includes background variables such as sex, age, parent identifiers, marriage status, residential municipality, immigration status, and education. The tax records enable us to isolate labor income and business income, capital gains, interest expenses, government transfers, debt, bank deposits, and total wealth. The last data set is collected by the Norwegian Mapping Authority and contains information on all real estate and housing transactions, including the buyers’ identifiers, the transaction value, and a location identifier.

**Variable construction** In our regressions, we employ variables that are calculated at two levels. Since individuals in the same household are likely to buy and finance a home together and they share the legal responsibility for debt, we calculate household leverage at the household level. We also measure deposits, income, and interest payments at the same level. When considering labor market outcomes and job search behavior, we use individual (worker) level data instead. In what follows, we describe the main variables.

We compute the LTV ratios using information obtained from the official tax and housing transaction registers. Norwegian banks report individual data on debt, deposits, and interest received and paid to the Norwegian Tax Administration to produce pre-filled personal tax filings. We define mortgage credit as the increase in households’ total debt in the year of the home purchase, and divide it by the house transaction value observed in the Mapping Authority’s housing transaction register.<sup>12</sup> On average, households have an LTV ratio of 93 percent in our sample (Table 1). We can calculate the DTI ratio exactly from the tax filings as the ratio between a household’s total debt and its total income prior to the layoff. The average DTI ratio is 4.22 in our sample, with a standard deviation of 2.08.

To analyze wages, we use the wage growth between the job that a worker is displaced from and the next job that she finds. We follow the literature and use the symmetric growth rate

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<sup>12</sup>This calculation could potentially overestimate the regulatory LTV ratio if households who buy a house systematically use unsecured loans or increase their utilization of existing lines of credit in the year of the home purchase, but would still accurately identify all housing related debt.

to allow for labor market exit and limit the role of outliers.<sup>13</sup> To avoid noise stemming from additional, one-time payments that workers receive in the beginning or at the end of a job, we follow (Graham et al., 2023; He and le Maire, 2023) and use wages in the year before the job loss and the year after the job initiation. Consistent with the job displacement literature, the average wage growth for displaced workers in our sample is negative. We measure the unemployment spell as the exact number of days between two jobs. On average, displaced workers in our data experience an unemployment spell of 146 days.

Table 1: **Summary statistics**

This table provides summary statistics of the main variables, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50.

	Mean	Std. Dev.	25 <sup>th</sup> pctl	50 <sup>th</sup> pctl	75 <sup>th</sup> pctl
Loan-to-value	0.93	0.22	0.78	0.96	1.07
Debt-to-income	4.22	2.08	2.72	3.84	5.58
House price (NOK 1000)	1948.43	1246.52	1205.73	1712.03	2457.94
Interest expense (NOK 1000)	92.44	71.39	45.36	75.17	121.31
Deposits (NOK 1000)	218.03	358.87	43.78	118.46	256.72
Income (NOK 1000)	709.65	713.82	394.46	589.76	877.12
Wage growth rate	-0.09	0.12	-0.15	-0.08	-0.02
Unemployment spell (days)	146.12	222.45	46.00	65.00	152.00
$\Delta \ln(\text{Ex-post debt})$	0.09	0.97	-0.04	0.00	0.07
$\Delta \ln(\text{Firm wage premium})$	-0.29	0.03	-0.30	-0.29	-0.27
Different occupation	0.77	0.43	1.00	1.00	1.00
Different industry	0.66	0.47	0.00	1.00	1.00
Different job location	0.44	0.49	0.00	0.00	1.00
$\Delta \text{ Education}$	0.07	0.23	0.00	0.00	0.00
Observations	2,088				

Our main sample includes workers who lose their jobs between 2006 and 2014 and covers

<sup>13</sup>We follow Davis et al. (1998) and compute the symmetric growth rates as

$$\hat{w}_{it} = \frac{(w_{it} - w_{it-1})}{0.5 \times (w_{it} + w_{it-1})} \quad (1)$$

their entire job search until 2019.<sup>14</sup> To further tighten our identification, we limit our benchmark analysis to workers who bought a home between 2006 and 2013 and subsequently lost their jobs due to a mass layoff, as we detail in Section 4. In an extension, we again relax this restriction to verify that our findings generalize to a broader population. Applying this filter yields 2088 workers who are displaced from 578 different firms. Of the workers in our regression sample, 15 percent reside in Oslo, almost the same as the city’s population share in Norway. Roughly half of the workers in our sample were displaced from businesses in the services industry, while the remainder is evenly distributed among the other industries.

## 4 Empirical strategy

Our objective is to estimate the effect of the macroprudential borrowing restriction on job search and subsequent wages. Reaching our objective entails an empirical strategy with two specific features. First, since the restriction is applied to all new homebuyers, the empirical strategy should enable us to distinguish the workers affected by the restriction from the unaffected ones. Second, the empirical strategy should enable us to observe job search behavior clean from individual characteristics since such characteristics would likely affect job search initiation and introduce bias into our estimations. To have both of these features and estimate the causal effect of the restriction on labor market outcomes, we combine the LTV restriction with displaced workers who lost their jobs due to mass layoffs.

**Mass layoffs** We start with addressing the possible influence of LTV restriction on job search initiation and its interaction with individual characteristics. This influence could induce a selection bias since the LTV restriction can alter the characteristics of job-seeking workers. A reason could be that a higher down payment requirement due to the LTV restric-

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<sup>14</sup>Our sample period for layoffs ends in 2014 because of a change in the enforcement by NAV of reporting standards for employment data by multi-plant firms in 2015. This temporarily reduces the accuracy of the identification of displaced workers after 2014. We are, however, able to observe workers’ labor market outcomes, such as wages and unemployment spells, until 2019.

tion may incentivize workers to look for better-paying jobs, creating a correlation between unobserved individual characteristics and job search. For instance, high-skilled workers may be more likely to switch to another job after the restriction. Alternatively, risk-averse workers may be less likely to start a job search since a new job can be riskier, and losing a job may lower the ability to accumulate enough savings for the down payment. These arguments indicate that the job-switchers after the restriction could have different unobservables compared to the ones before the restriction, and changes in wages and job search behavior could be driven by this difference in unobservables—an indication of a selection bias. Therefore, a job search not generated by individual-level decisions is essential to cleanly identify the policy’s effect. To obtain the needed clean job search, we use only displaced workers who lost their jobs in a mass layoff in our regression sample. Mass layoffs provide an appropriate setting for our purpose because they trigger job displacement exogenously, i.e., job displacement that is not caused by worker-specific characteristics (Flaaen et al., 2019). We define a mass layoff as a situation where a firm loses at least 30 percent of its workers in a year or ceases operations entirely. We follow the literature (e.g., Lachowska et al. (2020)) and use only firms with at least 50 employees to limit the risk of treating the layoff of small numbers of workers for idiosyncratic reasons as a mass layoff.<sup>15</sup>

**Macroprudential policy as a quasi-experimental setting** Due to the steep and enduring rise in house prices and household debt levels, Finanstilsynet (FSA) initially issued "Guidelines for prudent lending standards for new residential mortgage loans" to be effective by fall 2010. The guidelines established a maximum permissible LTV ratio of 90 percent. Because of the low compliance with the initial guideline, the FSA issued an update to reduce ambiguities and set a precise implementation period in December 2011.<sup>16</sup> The updated

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<sup>15</sup>Our results are robust to using firms with fewer employees. Note that as our analysis is at the worker level, including smaller firms does not significantly change the number of observations, as the vast majority of workers are employed by large firms.

<sup>16</sup>The FSA explained the motivation for the update as "the proportion of residential mortgages with a high loan-to-value ratio is on the increase, and a round of inspections of mortgage lending practice at a selection of banks shows that credit assessments need to improve (link)." The FSA also mentioned the vague starting date in the original announcement as another reason for the update.

guidelines that came into effect in January 2012 reduced the LTV threshold to 85 percent and specified that mortgages on the same property granted by other lenders should also be included in the LTV ratio.<sup>17</sup> In our main regressions, we therefore remove all observations between the two guidelines and start the post-treatment period in 2012 and the pre-treatment period in 2006 until 2010. In the absence of other regulations or policy changes that could potentially have affected labor markets, our setting provides a clean quasi-experimental setting in which we can study the impact of the LTV restriction on labor market outcomes.

**Sample** Since we investigate the effect of a borrowing restriction implemented for housing markets on labor markets, we need to relate these two markets to each other. Not doing so may mask the true magnitude of the effect. For instance, the restriction is unlikely to affect employed homeowners directly since it does not change these homeowners’ leverage. Thus, we relate the labor markets to housing markets by restricting our sample to displaced workers who recently bought their homes before losing their jobs due to the mass layoffs explained above. Job displacement is one of the most important negative income shocks a worker can experience, and it is an essential determinant of mortgage defaults—one of the main concerns for financial stability. Therefore, displaced workers constitute arguably the most policy-relevant sample. For instance, [Ganong and Noel \(2023\)](#) find that 70 percent of defaults are driven by negative income shocks, and 24 percent of defaults are driven by negative income shocks and negative equity. Among displaced workers, we define a recent homebuyer as those who bought their homes up to 12 months before losing their jobs. This short duration ensures that the LTV restriction is relevant and that workers’ propensity to save does not influence the effect of the restriction on leverage. Workers’ propensity to save may introduce a bias if there is a long enough time between the home purchase and job loss since workers with a high propensity to save would have lower leverage at the time of the job loss. Moreover, this short duration effectively shuts down any wealth effect that may work

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<sup>17</sup>In addition, interest-only mortgages and collateralized lines of credit were restricted by an LTV ratio of 70 percent.

through real estate as it is highly unlikely to experience significant changes in home values during this duration. In Section 5.2, we relax these restrictions stepwise until we study the whole population, as a way to check if our findings generalize to the entire population, for which we cannot use our tightest identification.

**Treatment status** We use the timing of home purchase, not job loss, to determine the treatment status, i.e., the workers in the post-treatment period are the ones who buy their homes starting from 2012. A feature of the LTV restriction we exploit is that it covers the whole population of new homebuyers, meaning that all LTV ratios are below 85 percent after the restriction. However, this feature does not mean that the restriction affects every homebuyer.<sup>18</sup> Before the restriction, 35 percent of homebuyers obtain mortgages with initial LTV ratios lower than 85 percent. This implies that approximately one-third of the workers in the post-treatment period would have had LTV ratios below 85 percent if the LTV restriction were not implemented. Such workers who endogenously prefer to have low LTV ratios are natural candidates for the control group. Nevertheless, we do not have a variable that enables us to separate these workers from the treated ones.

A common solution the literature has applied to cases where the treatment status is missing is to proxy the treatment status with a variable that is positively correlated with the actual treatment status. Some recent papers studying the effects of LTV restrictions have followed Abadie (2005) and used linear probability prediction models to construct the treatment and control groups (van Bekkum et al., 2024; Aastveit et al., 2020). Considering its suitability for our purpose, we use a random forest (RF), a machine learning (ML) method, to classify workers into treated and control groups.

Using an RF to proxy the treatment status comes with three advantages. The first

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<sup>18</sup>There is a small number of households with LTV ratios higher than the threshold after the policy. Lenders could grant loans with LTVs in excess of 85 percent if additional collateral was pledged or a special prudential assessment was performed. Anecdotal evidence indicates that collateral pledged by parents is the most common justification for a higher LTV. Since these households do not experience a change in their leverage and are thus untreated, we remove these observations from our estimation sample. Robustness checks show that this removal does not affect our results.



is that by using many variables instead of a single variable, RF improves the accuracy of the treatment classification. This gain is expected since a rich set of variables has more information than a single variable (Athey and Imbens, 2019; Calvi et al., 2021). The second advantage is that, unlike linear probability models, RF does not impose any functional form on the classification model. Therefore, RF is capable of capturing the true data-generating process more flexibly. Third, similar to other ML methods, RF is designed to maximize out-of-sample forecasting power. This is important for our purpose, as using many variables in the classification model can generate an overfitting problem. By focusing on out-of-sample instead of in-sample properties, RF alleviates concerns about overfitting and provides a more robust classification performance for the post-treatment period. We also consider three other classification methods in Section 5. We first use only one variable, bank deposit holdings, as a proxy for the treatment status. Then, we use a linear probability model as the classification method. Last, we exploit the bunching in the LTV ratio distribution with the idea that the households constrained by the restriction are likely to have LTV ratios right below the threshold. We obtain similar magnitudes when we use these alternative methods (Table 8).

We apply the RF in three steps. First, we construct the training and validation samples with homebuyers from the period between 2002 and 2010 but exclude the workers in our final regression sample to mitigate the risk of overfitting. We use several population registers to collect a rich set of household-level data that includes income, wages, deposits, DTI, business income, education, age, location, immigration status, and information about parental status and background like deposits, debt, wealth, education, and immigration status. All balance sheet items are lagged one period. To incorporate the influence of the macroeconomic conditions and house prices on the LTV ratio decisions, we include GDP growth, inflation, unemployment, the monetary policy rate, and regional and national house prices. We label homebuyers as treated if their LTV ratios are above 85 percent and the others as controls. In the second step, we use these correctly classified homebuyers with the variables above to train and validate the RF model. In the last step, we classify all workers in the

regression sample into treated and control groups using the trained RF model. We provide more details about the application, including the pruning and how we choose the parameters in Section A3.

Thanks to the ample availability of household-level and worker-level data, the classification power of the RF is high. A common way to assess the performance of a binary classifier is by plotting its receiver operating characteristics (ROC) curve and calculating the area under the curve (AUC).<sup>19</sup> In the literature, AUC values of 0.9 and higher are considered excellent (Hosmer et al., 2013). The AUC of our RF is 0.88. Another way to evaluate the performance is by looking at the success rate of RF for the pre-treatment workers. We can compare these workers' true treatment status with the classification by RF. We see that RF correctly classifies 82 percent of these workers.

Figure 1 summarizes the contribution of each variable to the performance of the RF classification model. Household balance sheet items, location, age, and parents' financials are important features related to the likelihood of being affected by the LTV ratio restriction. Table 2 lays out these differences between the treated and control groups by using the variables' one-year lagged values. Workers in the treatment group have, for instance, lower income and deposits. Moreover, their parents have lower deposits and wealth. Notably, none of the variables dominates the improvement in the model. Using a single variable to proxy the treatment status would thus miss a substantial fraction of the information available to the researcher, which reconfirms the advantage of a prediction model over a single variable strategy.

A possible concern about the RF classification model could be that the LTV restriction might shift house prices outside the span of the training and testing samples and thereby

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<sup>19</sup>A ROC curve shows the true positive rate and false positive rate for different probability thresholds to classify an observation to be treated. The AUC measures the area under the ROC curve (Bradley, 1997). The range of AUC values is between 0.5 and 1, and higher values indicate a more successful classifier. A perfect predictor that classifies each observation correctly would have an AUC of 1. Specifically, AUC shows the probability that a randomly chosen treated observation will have a higher estimated treatment probability than a randomly chosen control observation.

Figure 1: **Variable importance**

This figure shows the variable importance for the variables used in RF classification model. Variable importance is calculated by feature permutation importance, which evaluates the variable importance by calculating the difference in the prediction accuracy with and without the variable. The reported scores are the percentage contribution of each variable to the classification model's accuracy with respect to the accuracy of a model with all variables. Macro variables enter the model with levels and changes.



reduce the model's predictive accuracy after the implementation of the borrowing restriction.

Figure A3 shows that house price growth rates after the policy are, in fact, within the span of the pre-policy growth rates. This suggests that the price effects of the borrowing restriction, if any, should not be a concern for the classification ability of the RF.

**Empirical specification** After classifying workers into treated and control groups, we estimate the following difference-in-differences model:

$$y_{ht} = \beta d(\widehat{LTV} > 0.85)_h \times Post_t + \alpha_1 d(\widehat{LTV} > 0.85)_h + \alpha_2 Post_t + \alpha_n controls_{ht} + \epsilon_{ht} \quad (2)$$

Table 2: **Comparison of treated and control groups**

This table compares the variables used in the prediction model for the treated and control groups.  $d(\widehat{LTV} < 0.85)$  indicates that the household is predicted to be control and  $d(\widehat{LTV} \geq 0.85)$  indicates that the household is predicted to be treated. All variables are lagged by one year. Balance sheet items (income, wage, deposits, business income) are in thousands.

	$d(\widehat{LTV} < 0.85)$	$d(\widehat{LTV} \geq 0.85)$	Difference	t-stat
Income <sub>t-1</sub>	1136.54	721.14	415.4	8.52
Wage <sub>t-1</sub>	1072.83	679.53	393.3	8.21
Debt-to-Income <sub>t-1</sub>	2.57	1.56	1.01	3.84
Deposits <sub>t-1</sub>	872.17	168.35	705.00	27.65
Business Inc. <sub>t-1</sub>	61.54	23.41	38.13	2.17
Parents' Debt <sub>t-1</sub>	1952.74	1957.37	15.37	-0.43
Parents' Dep. <sub>t-1</sub>	1374.92	750.32	624.6	9.58
Parents' Wealth <sub>t-1</sub>	1541.74	534.38	1007.36	5.24
Age	36.12	32.45	3.67	5.14
Immigrant	0.19	0.20	-0.01	-0.94
Immigrant <sup>Mot</sup>	0.21	0.23	-0.02	-0.84
Immigrant <sup>Fat</sup>	0.29	0.31	-0.02	-0.42
College	0.75	0.41	0.34	9.52
College <sup>Mot</sup>	0.26	0.18	0.08	3.29
College <sup>Fat</sup>	0.33	0.19	0.14	5.84
Observations	2,088			

where  $y_{ht}$  is either a household balance sheet variable such as the DTI ratio or a labor market variable such as wage growth,  $Post_t$  is a dummy variable that equals one after implementation of the policy, and  $d(\widehat{LTV} > 0.85)_h$  is a dummy variable that takes the value of one if a worker is predicted to have an LTV ratio above the 85 percent threshold. We saturate the difference-in-differences model with year, education, location, and industry fixed effects. Given that our sample consists of workers who are displaced in mass layoffs that may be driven by developments at the industry and/or location level, we double cluster the standard errors at the industry and location level (Abadie et al., 2022).

The main identifying assumption underlying the model in Equation 2 is that the outcome variables of treated and control groups would have parallel trends if the policy hadn't been implemented. The standard way to test this identifying assumption is to look at the trends of

the treated and control groups before treatment. A confirmation that the trends are parallel would provide strong support for the assumption that, absent treatment, the treated and control groups would have experienced similar paths in their outcomes. We investigate the trend differences in the pre-treatment period by estimating the following model where we have replaced the  $Post_t$  indicator in Equation 2 with period dummies  $D_k$ :

$$y_{ht} = \sum_{k=-4}^2 \beta_k D_k \times d(\widehat{LTV} > 0.85)_h + \alpha_1 (\widehat{LTV} > 0.85)_h + \alpha_n controls_{ht} + \epsilon_{ht} \quad (3)$$

We omit  $period = -1$  in Equation 3; the estimated  $\beta_k$  coefficients therefore document the difference between treated and control groups at  $period = k$  relative to that at  $period = -1$ . Note that our sample is a repeated cross-section, meaning that we do not track workers over time, and each worker appears once in the sample (Heckman and Robb Jr, 1985).<sup>20</sup> The benefit of not tracking workers over time is that it enables us to rule out any wealth effects. The households in our sample may have different wealth accumulation paths after purchasing their homes due to possibly heterogeneous savings rates. In particular, if we track these workers over time and keep the time between home purchase and job search long enough, households with higher savings rates would accumulate more savings, either as home equity or financial assets, which could influence their job search behavior. Therefore, the job search would be affected by both the exogenous reduction in leverage and endogenous savings rate, making a causal estimate more difficult to obtain. In addition, limiting the time between home purchase and job search helps us to minimize the influence of different location-level house price growth rates on housing wealth, which could only be crudely proxied by an econometrician. In addition to these advantages, the repeated cross-section structure necessitates the assumption that there should not be compositional change before and after the restriction (Heckman et al., 1997; Sant’Anna and Xu, 2023). We investigate compositional

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<sup>20</sup>Heckman and Robb Jr (1985) document that the difference-in-differences design can be applied to repeated cross-section data and justify its use in such settings. Also, see Heckman et al. (1999).

changes in Section 5.2 and fail to find any.

Our construction of the treated and control groups has two implications for our empirical analysis. First, there is the possibility that we incorrectly classify the treatment status of certain workers, similar to situations where a treatment indicator is available in the data but measured with error. The high out-of-sample predictive power of our RF model suggests that misclassification should be minimal. In addition, [Lewbel \(2007\)](#) shows that the misclassification of a binary treatment regressor creates an attenuation bias akin to standard measurement error bias under similar assumptions. We also use three other classification methods that yield the same findings. As these methods rely on different statistical tools or information sources, such as bunching in the LTV ratio distribution instead of household characteristics, the measurement errors in these methods are likely to be different in nature from the one in the RF. Therefore, finding the same results with these classification methods suggests that measurement error in treatment status does not play a significant role in our findings.

The second implication stems from the differences between the treatment and control groups. As we use observable differences among workers to assign them to treatment and control groups, it is natural to see that these groups have different characteristics. As a consequence, the treatment and control groups may have different labor market prospects. In our difference-in-differences estimation, we control for the influence of these different characteristics by taking differences among treated workers and control workers. Different characteristics could pose a threat to a causal interpretation only if their influence on labor market outcomes changes at the same time as the LTV restriction. Therefore, our causal interpretation rests on the assumption that the effect of these different characteristics on labor market outcomes does not change at the same time as the LTV restriction. The graphs in Section 5 display robust parallel trends between the treatment and control groups in the pre-treatment period, indicative of a stable influence of the different characteristics on the outcome variables.

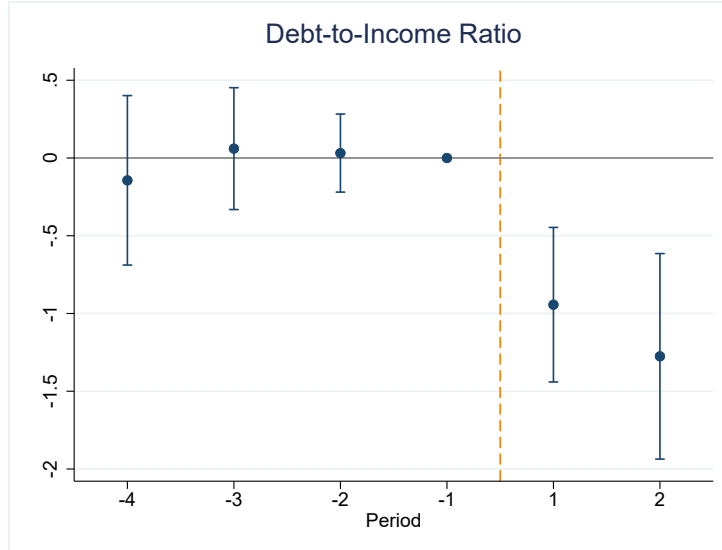
## 5 Impact of the borrowing restriction

In this section, we analyze how a macroprudential policy that reduces households' ability to borrow against collateral affects job search and subsequent wages of job-seeking workers. Section 5.1 presents the direct effect of the policy on the DTI ratio, our measure of household leverage. Section 5.2 details the impact of the policy on wages and related robustness checks. Section 5.3 lays out the mechanism through which lower leverage affects wages and other labor market outcomes. Finally, Section 5.5 contains estimates of the longer-term effects of the policy.

### 5.1 Impact of borrowing restriction on household leverage

Figure 2: Dynamic impact of macroprudential policy on DTI ratio

This figure shows the dynamic effect of the LTV policy on DTI ratio. The sample is individual-level data, where leverage is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is DTI ratio calculated from tax filings and is the ratio of total debt to total income.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. The figure shows the  $\beta$ s on the y-axis of the regression model,  $DTI_{ht} = \sum_{k=-4}^2 \beta_k D_k \times d(\widehat{LTV} > 0.85)_h + \alpha_1 (\widehat{LTV} > 0.85)_h + \epsilon_{ht}$ . Baseline event period is  $k = -1$ . Regression model includes year fixed effects. Orange bar specifies the implementation of LTV restriction. Standard errors are two-way clustered at location and industry level and bars indicate 95% confidence intervals.



To provide visual evidence of how the LTV restriction reduces households' DTI ratios, we estimate Equation 3 with the DTI ratio as the dependent variable. Figure 2 shows the estimated coefficients. The difference between the DTI ratios of treated and control groups is essentially constant during the pre-treatment period, lending support to the parallel trends assumption. After the introduction of the restriction, the treated group has substantially lower leverage. Table 3 displays the parameter estimates from the corresponding difference-in-differences model (Equation 2) of Section 4 and confirms the implications of Figure 2. In the baseline regression without any fixed effects, the LTV restriction reduces treated households' DTI by 25 percent at the mean value. In column (2), we include year-fixed effects to control for time effects, and we further saturate the model with education fixed effects in column (3).

Table 3: **Impact of macroprudential policy on DTI ratio**

This table documents the effectiveness of the LTV ratio policy on the households' debt-to-income (DTI) ratio. Each column uses individual level data, where DTI is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is DTI ratio calculated from tax filings and is the ratio of total debt to lagged total income before the displacement.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value.  $Post$  is equal to 1 for the years after 2012 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	$\frac{Debt}{Income}$					
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times Post$	-1.082*** (0.366)	-1.035*** (0.337)	-1.127*** (0.389)	-1.097*** (0.352)	-1.023*** (0.348)	-1.009*** (0.382)
$d(\widehat{LTV} > 0.85)$	0.862*** (0.279)	0.853*** (0.247)	1.089*** (0.292)	1.154*** (0.258)	1.138*** (0.224)	1.139*** (0.238)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location $\times$ Industry FE						✓
Obs.	2,088	2,088	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.024	0.027	0.173	0.188	0.212	0.271



A potential concern about our model specification could be that mass layoffs may not occur randomly but rather due to location- or industry-specific shocks, which may create a selection bias. To address this concern, we add location, industry, and location $\times$ industry fixed effects in columns (4)-(6) to control for location and industry-specific characteristics. In all specifications, we estimate negative and significant coefficients that are quantitatively close to those in the model without fixed effects. Overall, [Table 3](#) confirms that the policy works as expected: it reduces household leverage.

In [Section A2](#), we further document *how* the restriction reduces household leverage. After the introduction of the policy, treated households take on mortgages that are, on average, NOK 636,000 smaller and pay for homes that are NOK 516,000 cheaper. The restriction reduces households' liquidity, but not in a statistically significant way. We also show that the smaller mortgages reduce interest expenses. Together with a reduced need for principal repayments, this cut cash outflows by approximately 10 percent of the household's wages before displacement occurs.

## 5.2 Impact of borrowing restriction on wages

After establishing its effects on leverage, we now investigate how the macroprudential policy affects the wages of displaced workers in their new jobs. In principle, the leverage restriction can increase the wages of displaced workers since it can reduce the pressure that leverage creates during job search. This pressure is created by default costs that workers have to bear if they cannot service their debt, such as limited access to credit markets, worsened labor market prospects, forced moves, and even elevated divorce rates, particularly among marginal homeowners ([Ji, 2021](#)).<sup>21</sup> Alternatively, leverage can influence job search broadness. By increasing consumption commitments, leverage can induce a higher risk aversion, which may

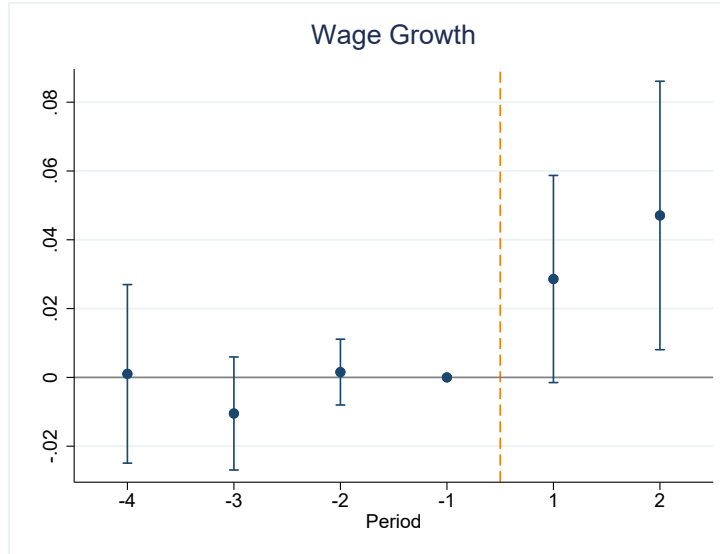
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<sup>21</sup>[Dobbie et al. \(2020\)](#); [Gross et al. \(2020\)](#) show worsened credit scores after a default make it harder to regain access to credit, while [Dobbie and Song \(2015\)](#); [Bos et al. \(2018\)](#); [Di Maggio et al. \(2024\)](#) document the impact on labor market prospects and [Diamond et al. \(2020\)](#) find non-pecuniary costs of foreclosures on forced moves and divorce rates.

narrow the scope of job search (Chetty and Szeidl, 2007). For these reasons, workers with high leverage may either accept earlier job offers and forego later, potentially better-paid offers, or narrow their job search and neglect some feasible options. These mechanisms suggest that the leverage restriction may improve workers' labor market outcomes by enabling them to have a better job search and a better subsequent match.

Figure 3: **Dynamic impact of policy on wage growth**

This figure shows the dynamic effect of the LTV policy on wage growth for displaced workers. The sample is individual level data, where leverage is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is wage growth between the wage in the previous job and the wage in the new job.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. The figure shows the  $\beta_s$  on the y-axis of the regression model  $wage\ growth_{ht} = \sum_{k=-4}^2 \beta_k D_k \times d(\widehat{LTV} > 0.85)_h + \alpha_1 (\widehat{LTV} > 0.85)_h + \epsilon_{ht}$ . Baseline event period is  $k = -1$ . Regression model includes year fixed effects. Orange bar specifies the implementation of the LTV restriction. Standard errors are two-way clustered at location and industry level and bars indicate 95% confidence intervals.



We first estimate the dynamic effect of the policy on wages in Figure 3, depicting  $\beta_k$  from Equation 3, where the dependent variable is a worker's wage growth between the job she is displaced from and her next job. During the years before the policy, wage growth for the treated and control groups follows parallel trends. However, after the introduction of the borrowing constraint, treated workers experience higher wage growth when they are displaced, indicating that reducing leverage improves the wages of displaced workers.

Table 4 complements Figure 3 with regression evidence from the difference-in-differences model in Equation 2, where wage growth between job switches is the dependent variable. In column (1), where we leave out any controls,  $d(\widehat{LTV} > 0.85)_h \times Post_t$  has a positive and statistically significant coefficient. In column (2), we include year-fixed effects to control for time effects. To mitigate any concern that treated displaced workers may have different education levels and that this can influence household leverage or labor market outcomes and create a bias in our coefficient of interest, we include education fixed effects in column (3). Another concern may be related to the labor demand that workers face. Depending on their location and industry, workers may be exposed to different labor demand, and, to the extent that the labor demand is correlated with the propensity to be affected by the restriction, it can introduce a bias into our estimations. To control for labor demand and non-randomness of mass layoffs, we further saturate the model with location and industry fixed effects in columns (4) and (5). We report our most saturated model in column (6), which includes  $Location \times Industry$  fixed effects. In this tight specification,  $d(\widehat{LTV} > 0.85)_h \times Post_t$  still has a positive and statistically significant coefficient.

Our preferred specification in column (5) indicates that treated workers enjoy 3.9 pp higher wages in their new jobs. To assess its economic significance, we compare this effect to the average change in wages in our sample. Consistent with earlier findings, displaced workers in our sample experience a reduction in wages in their new jobs, 9.2 pp on average. Treated workers thus also experience a decline in their wages when they are displaced, but the effect is 43 percent smaller in magnitude.

**Selection effects** A potential threat to the causal interpretation of our results is that the policy may influence workers’ home purchase decisions. The reason is that workers who were able to buy a house before the policy may not be able to do so after the policy due to the required down payment. Therefore, the policy can change the characteristics of the treated workers. Then, the improvement in wages could be partly driven by changes in the characteristics of the treated group induced by the policy itself. Several analyses suggest

Table 4: **Impact of the policy on wage growth**

This table documents the effect of the LTV ratio policy on wage growth for displaced workers. Each column uses individual-level data, where observations between the first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is wage growth between the wage in the previous job and the wage in the new job.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. *Post* equals one for the years after 2012 and equals zero for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.048** (0.026)	0.046** (0.025)	0.041*** (0.023)	0.041*** (0.019)	0.039** (0.018)	0.037* (0.018)
$d(\widehat{LTV} > 0.85)$	-0.020*** (0.006)	-0.017*** (0.005)	-0.017*** (0.006)	-0.016*** (0.006)	-0.018*** (0.007)	-0.019*** (0.007)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location $\times$ Industry FE						✓
Obs.	2,088	2,088	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.007	0.013	0.089	0.102	0.124	0.182

that this possible selection concern does not influence our findings.

First, as explained in Section 2, Norway has one of the highest homeownership rates among advanced economies. Norwegian households have strong fiscal incentives for home ownership because the de facto tax rate on primary houses is substantially lower than on other types of wealth.<sup>22</sup> A plot of the homeownership transition rate in Figure A8 indicates that there is barely any effect of the borrowing restriction on the transition into homeownership. This first evidence suggests that selection is likely not a challenge to our findings.

Second, we check whether the borrowing restriction alters the observable characteristics of the treated workers in our sample using a similar strategy as Bernstein and Koudijs (2024). To this end, we rerun the difference-in-differences model in Equation 2 but use log changes

<sup>22</sup>Official taxation values for real estate are below market value. In addition, for primary houses, the tax value is only 25 percent of the housing value, and the tax rate is 0.7 percent.

Table 5: **Checking observable characteristics and removing households that cannot afford the down payment**

This table documents that the LTV ratio policy does not change the characteristics of the treated group and removing households that cannot afford the down payment does not affect the impact of the LTV ratio policy on wage growth. Each column uses individual level data, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variables are indicated at the column headings. In columns (1)-(6), dependent variables are lagged by one period (i.e., one period before the layoff). Columns (7)-(8) remove households that do not have enough deposits for a hypothetical down payment from the sample from the pre-policy period.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value.  $Post$  equals one for the years after 2012 and equals zero for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

<u>Previous</u>	<u>Inc.</u>	<u>Wage</u>	<u>Buss. Inc.</u>	<u>Trans.</u>	<u>Unemp. Ben.</u>	<u>Educ.</u>	<u>Wage Growth</u>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$d(\widehat{LTV} > 0.85) \times Post$	0.013 (0.176)	0.011 (0.195)	0.042 (0.144)	-0.124 (0.286)	-0.037 (0.283)	0.031 (0.075)	0.051** (0.025)	0.041** (0.019)
$d(\widehat{LTV} > 0.85)$	0.011 (0.061)	0.012 (0.059)	-0.050 (0.055)	0.018 (0.091)	0.084 (0.097)	0.003 (0.021)	-0.011 (0.047)	-0.009 (0.052)
<i>Fixed Effects:</i>								
Year FE	✓	✓	✓	✓	✓	✓		✓
Education FE	✓	✓	✓	✓	✓			✓
Location FE	✓	✓	✓	✓	✓	✓		✓
Industry FE	✓	✓	✓	✓	✓	✓		✓
Obs.	2,088	2,088	2,088	2,088	2,088	1,876	1,021	1,021
R <sup>2</sup>	0.105	0.108	0.079	0.112	0.096	0.087	0.013	0.182

in income, wage, business income, transfers, unemployment benefits, and education level *one period before the layoff* as dependent variables, instead of wage growth after job loss. The first six columns of Table 5 show that the borrowing restriction does not have a statistically or economically significant effect on these variables, which indicates that the restriction does not change the observable characteristics of the treated workers. Columns (1) and (2) also mitigate another potential concern, i.e., that workers might have tried to increase their earnings before home purchase and job loss once the down payment requirement was in place. If present, such an increase in income could, for example, have generated a momentum that benefited workers in their job search. In such a case, our result that low-leverage job seekers find better-paid jobs could partially have reflected a momentum effect. Since the borrowing

restriction did not influence income growth before the home purchase, as documented in columns (1)-(2), we can effectively rule out this concern.

Third, we homogenize the regression sample with respect to the ability to afford the down payment. As noted before, the selection concern arises since some workers who could purchase a home before the restriction may not be able to do so after the restriction. This argument means that if we can remove the workers who cannot afford the down payment from the pre-policy period, the remaining sample will be clean from the selection concern. Our data sets enable us to remove such workers since we observe both the home transaction values and holdings of bank deposits. Specifically, for home purchases before the restriction, we calculate a hypothetical down payment, which is 15 percent of the home value. Then, we exclude the workers who do not have enough bank deposits to cover the hypothetical down payment from the pre-policy period and rerun our main regression model using this subsample. If our results are not driven by the selection, we should estimate a similar finding in this subsample. Indeed, the last two columns of [Table 5](#) show that the estimated coefficients for this refined sample are nearly equal to our original estimates in [Table 4](#). This constitutes further evidence that we need not be concerned about a selection bias.

**External validity** The combination of our research design and detailed population registers from Norway enables us to uncover the causal effect of a macroprudential policy on labor market outcomes. Despite its considerable advantages, this combination may have a potential downside: it requires a narrower sample of workers who recently bought their homes before losing their jobs and are from Norway—a country with generous labor market policies. We now investigate to what extent our results generalize, considering both broader samples and labor market policies in Norway.

As explained in [Section 4](#), we use displaced workers who bought their homes up to 12 months before losing their jobs. This sample enables us to observe job search behavior clean from individual characteristics and to prevent the individual saving propensity from

Table 6: **External validity: relaxing sample filters and considering labor policies**

This table provides evidence for the external validity of our results in two ways. First, it documents that the negative relationship between wages and debt exists in broader samples as well, i.e., when we relax the home-ownership requirement, the mass layoff requirement, the timing around the introduction of the borrowing constraint and that the home should have been bought shortly before a layoff. Second, it documents that our results are not driven by labor market policies in Norway. The first three columns use individual-level data from 2000 to 2017. The fourth column uses data from 2003 to 2013. Column (1) uses the whole population. Column (2) uses all individuals who receive unemployment benefits. Column (3) uses individuals who lost their jobs due to mass layoffs. Column (4) uses individuals who lost their jobs due to mass layoffs and bought their houses up to 4 years before being laid off. Column (5) uses individuals who are recent homebuyers and received unemployment benefits. Column (6) restricts the sample by removing workers whose unemployment spell is longer than 500 days. Column (7) restricts the sample by removing workers whose unemployment spell is longer than 2 years. Column (8) restricts the sample by removing workers whose job tenure at their previous employer is longer than 5 years.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value.  $Post$  equals one for the years after 2012 and equals zero for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

Wage Growth	Full	Unemployed	Displaced	$\leq 4y$	Unemp.&HB	Spell		Tenure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
						< 500days	< 2 years	< 5 years
$\ln(\text{debt})_{t-1}$	-0.026*** (0.0001)	-0.052*** (0.0003)	-0.019*** (0.001)					
$d(\widehat{LTV} > 0.85) \times Post$				0.046** (0.021)	0.043*** (0.015)	0.048** (0.019)	0.045** (0.021)	0.041* (0.022)
$d(\widehat{LTV} > 0.85)$				-0.021*** (0.005)	-0.023*** (0.004)	-0.017** (0.008)	-0.017** (0.008)	-0.016* (0.009)
<i>Fixed Effects:</i>								
Individual FE	✓							
Wage bins FE	✓	✓	✓					
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Education FE				✓	✓	✓	✓	✓
Location FE				✓	✓	✓	✓	✓
Industry FE				✓	✓	✓	✓	✓
Obs.	33,421,099	1,880,454	148,875	9,027	19,651	1,912	1,982	1,647
R <sup>2</sup>	0.360	0.376	0.116	0.127	0.134	0.131	0.129	0.132

influencing the effect of policy on household leverage and housing wealth, helping us establish causality. Even though this sample is highly relevant for policy discussions, it does not represent the general population. In Table 6, we relax our sample selection criteria and assess the relationship between leverage and wages in broader samples.<sup>23</sup> In column (1), we use the whole Norwegian population and find that leverage is negatively associated with wage growth within the same individual, which is in line with our results. In column (2), we use

<sup>23</sup>In the first three columns of Table 6, we use  $\ln(\text{debt})$  instead of DTI as the main independent variable. The reason is that income appears both in wage growth and DTI, which creates a mechanical correlation. Instead of using income as the denominator in DTI, we create wage bins and include these bins as fixed effects.

unemployed workers who could be unemployed voluntarily or involuntarily. The coefficient is again negative but increases in magnitude. This increase may reflect the stickiness of wages due to wage contracts, suggesting that including employed workers in the sample can mask the effect of leverage on wages. In column (3), we use only displaced workers. The magnitude of the negative coefficient is smaller compared to the one in column (2). This decline in magnitude illustrates the importance of using workers who lose their jobs involuntarily since it indicates that selection into unemployment generates an upward bias. In column (4), we turn to home-buying displaced workers. Instead of 12 months, this column uses workers who bought their homes up to four years before losing their jobs. Note that, in this sample, leverage is not exogenous because workers may pay their mortgage at different speeds, making leverage at the time of job loss dependent on the individual savings rate. The difference in savings rate can create a bias if the rate is correlated with factors that influence labor market outcomes. Therefore, the change in coefficient size is informative about the magnitude of bias that individual savings rates may generate. Indeed, we estimate a larger coefficient in this wider sample, which indicates the importance of using recent homebuyers to establish causality. Column (5) relaxes the sample selection criteria by including individuals who bought a home and received UI payments afterward in the following year. While the job search of these individuals could be driven by their individual characteristics and hence endogenous, we again find a positive effect on wages.

Next, we consider the labor market policies in Norway. Section 2 points out that the generosity of unemployment insurance in Norway is mainly driven by its duration, which is two years, as the UI replacement rate is close to the OECD median. The long unemployment insurance duration may reduce our results' external validity if the workers fully exhaust unemployment insurance. We assess this possibility in Table 6. In columns (6) and (7), we remove the workers whose unemployment spell is longer than 500 days and two years, respectively. In line with the fact that the average unemployment spell in our sample is around five months, removing such workers does not affect our results. The second labor



market policy we consider is the notification period for job termination. If the worker’s tenure in the firm is longer than five years, the notification period increases to two months from one month, which can allow the worker to adjust her job search before the job contract ends. In column (8) of [Table 6](#), we show that this does not create a threat to our results since removing workers whose tenure is longer than five years does not change our findings.

Another concern for the limited external validity of our results can stem from displaced workers differing in a fundamental way from the whole population ([Caggese et al., 2019](#)). In [Table 7](#), we therefore test for the presence of such differences among *all* workers who are employed in our sample firms for the sample period by regressing worker characteristics on a dummy variable that takes the value of 1 if the worker is in our main regression sample. Column (1) in [Table 7](#) shows that workers in our sample are, on average, younger than other workers in the same firm. This is expected as the workers in our sample are first-time homebuyers. The other columns demonstrate that workers in our sample are not statistically different from other workers in terms of education, marital status, sex, immigration status, and wage growth once age is controlled for. Overall, our findings fail to suggest a threat to the external validity of our results.

**Alternative treatment classifications** Thanks to its out-of-sample prediction power, the RF helps us to classify workers into treatment and control groups with high accuracy. While our approach provides consistent estimates of the borrowing restriction’s effect on starting wages, the RF algorithm lacks transparency. Therefore, we use three alternative treatment classification methods that are commonly used and more transparent but probably have less out-of-sample predictive power. First, we use workers’ bank deposits before the home purchase to classify them as either treated or untreated. Deposits had the highest variable importance in RF ([Figure 1](#)). Intuitively, in the context of introducing an LTV policy, bank deposits are also likely to be correlated with LTV ratios, i.e., with higher deposits the same house can be bought with a lower LTV ratio, and an LTV policy is less likely to bind for households with more bank deposits. Yet, as house choice is endogenous

Table 7: **Comparing workers-in-sample to other workers**

This table compares the workers used in the regression sample with other workers in their previous firms. Each column uses individual level data, where observations between first and second policy implementation are excluded. The independent variable, *Workers – in – sample*, is a dummy variable that takes the value of 1 for workers who are in the regression sample. Column (1) uses workers’ age as the dependent variable. Column (2) uses a dummy variable that takes the value of 1 for workers who have higher education as the dependent variable. Column (3) uses a dummy variable that takes the value of 1 for workers who are married as the dependent variable. Column (4) uses a dummy variable that takes the value of 1 for workers who are female as the dependent variable. Column (5) uses a dummy variable that takes the value of 1 for workers who are immigrants as the dependent variable. Column (6) uses lagged wage growth as the dependent variable. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at firm and year level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Age	Education	Married	Female	Immigrant	Wage Gr.
	(1)	(2)	(3)	(4)	(5)	(6)
Workers-in-sample	-4.571*** (0.583)	0.0246 (0.0131)	0.0217 (0.0185)	0.0162 (0.0203)	0.021 (0.011)	0.0179 (0.0267)
<i>Fixed Effects:</i>						
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Age FE		✓	✓	✓	✓	✓
Obs.	201,452	201,452	201,452	201,452	201,452	201,452
R <sup>2</sup>	0.251	0.193	0.258	0.144	0.119	0.121

to liquidity and households with little liquidity are likely to buy smaller houses, using only deposits as a classification variable has limitations. In the first two columns of [Table 8](#), we classify the workers whose lagged deposits are below the sample median as treated. We obtain positive coefficients in both columns, yet only column (2) has a significant coefficient. The reduction in significance and magnitude is likely driven by the low classification power of a single variable that would increase the size of the attenuation bias. Indeed, [Figure 1](#) indicates that many variables contribute to the RF’s classification success.

Next, we use a linear probability model to classify the workers in columns (3)-(4). We obtain positive and significant coefficients, although again with smaller magnitudes than in our main results. This is in line with our expectation that linear probability models have lower classification power than the RF algorithm.

Finally, we use the LTV distribution before and after the restriction to classify workers

(Figure A9). The main idea is that workers who are affected by the restriction are more likely to choose LTV ratios close to the threshold after the implementation. Thus, another way to generate a treatment indicator would be to classify workers with LTV ratios above 85 percent before the restriction and workers whose LTV ratios are very close to but smaller than 85 percent (we choose 75 to 85 percent) *after* the restriction as treated. While this method provides a transparent classification method, it has two fundamental challenges. First, there is a sizable mass right below 85 percent before the restriction, suggesting that there would also have been workers with LTV ratios very close to 85 percent in the post-period if the restriction had not been implemented. Such workers will be misclassified by this method. Moreover, not all treated workers will have LTV ratios very close to 85 percent after the introduction of the restriction, since people may decide to buy a smaller or cheaper home. Ignoring these challenges, this bunching approach yields positive and significant coefficients in columns (5) and (6) of Table 8.

**Additional robustness checks** In Table A5, we document that the effect of household leverage on wages is robust to several modifications of our empirical specification. Columns (1) and (2) show that shortening or extending the sample period does not change our results. Removing people who receive large inheritances or gifts from parents or earn business income, and therefore may differ in their search, leaves our main finding unaffected. Treated workers may possibly react differently to macroeconomic conditions. If macroeconomic conditions change around the time that the LTV restriction is implemented, our estimates could pick up that differential response. Thus, we interact inflation, the unemployment rate, GDP growth, and the monetary policy rate with the treatment indicator. Doing so leaves the positive impact of leverage on displaced workers' wage growth unchanged. In column (6), we also saturate the model with separate education fixed effects for the treatment group and find that education does not affect treated workers differently. The last column performs a placebo test, in which we remove all post-policy observations and create the variable  $Placebo_t$  that takes the value of one for the two periods before the restriction and zero for the earlier

Table 8: **Wage effects with alternative treatment classification methods**

This table provides the wage growth results using alternative treatment classifications. Columns (1) and (2) classify workers whose household's deposits are below the sample median as treated. Columns (3) and (4) use a linear probability model instead of an RF to classify workers. Columns (5) and (6) classify workers as treated if they have LTV ratios above 85 percent before the restriction and between 75 and 85 percent after the restriction. Each column uses individual-level data, where observations between the first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is wage growth between the wage in the previous job and the wage in the new job. *Post* equals one for the years after 2012 and equals zero for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

Wage Growth	Deposits <sub>t-1</sub>		LPM		LTV Bunching	
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.029 (0.019)	0.027* (0.015)	0.034* (0.018)	0.031** (0.014)	0.034* (0.018)	0.032** (0.015)
$d(\widehat{LTV} > 0.85)$	-0.009 (0.011)	-0.012 (0.010)	-0.016** (0.008)	-0.018*** (0.006)	-0.020** (0.008)	-0.019*** (0.008)
<i>Fixed Effects:</i>						
Year FE		✓		✓		✓
Education FE		✓		✓		✓
Location FE		✓		✓		✓
Industry FE		✓		✓		✓
Obs.	2,088	2,088	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.003	0.121	0.009	0.122	0.008	0.124

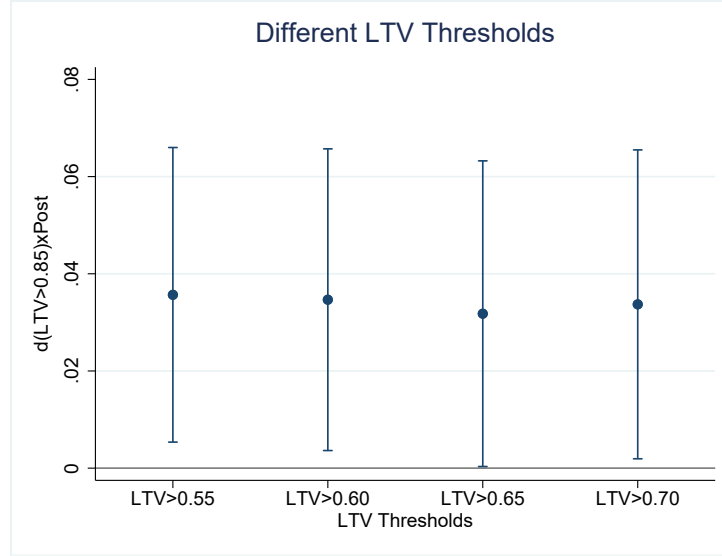
periods. In line with the parallel trends in [Figure 3](#), the placebo effect has no effect on wages, supporting our main identifying assumption. In addition, in [Table A6](#), we use alternative target variables instead of the dummy variable  $d(\widehat{LTV} > 0.85)$ . Columns (1) and (2) first predict the LTV ratios and use the predicted ratios to classify workers into treatment and control groups. The next two columns predict DTI ratios and use 35<sup>th</sup> percentile of the sample as the treatment threshold value. We obtain results similar to those of our main findings in both cases.

Intuitively, we expect treated workers to limit the impact of the borrowing constraint on their individual housing choice and, therefore, have LTV ratios just below the policy threshold in the post-treatment period. If true, then observations from the treated and

control groups with LTVs just below the policy threshold will make a better comparison since they are more similar in terms of the main selection criterion, the LTV ratio. In our baseline regressions, we set the lower bound for the LTV ratio equal to 50 percent and exclude workers with lower LTV ratios. If our choice of the lower bound for the selection criterion is reasonable, then raising it towards the policy threshold, i.e., removing the least similar observations, should not affect the estimated treatment effect. We demonstrate that this is the case. [Figure 4](#) plots the treatment effect where the x-axis indicates different lower bounds for the sample and the y-axis shows the coefficient of  $d(\widehat{LTV} > 0.85)_h \times Post_t$ . Moving rightward along the x-axis raises the sample's lower bound for the LTV ratio by 5 percent in each step. Since the coefficient estimate is virtually unchanged, we can conclude that the initial sample inclusion threshold of 50 percent yields comparable treatment and control groups with the additional benefit of providing a wider and hence more representative sample.

Figure 4: **Different thresholds for the LTV lower bound**

This figure provides a robustness check for the effect of LTV policy on wage growth for displaced workers. The sample is individual-level data, where leverage is measured at the household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The dependent variable is wage growth between the wage in the previous job and the wage in the new job.  $d(\hat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value.  $Post$  equals one for the years after 2012 and equals zero for earlier years. The x-axis indicates the value of the lower bound for the LTV ratio to be included in the estimation sample. The y-axis shows  $\beta$  from the Equation 2. Regression models include year, education, location, and industry-fixed effects. Standard errors are two-way clustered at location and industry level and bars indicate 90% confidence intervals.



### 5.3 Through what mechanism does leverage affect wages?

Having established the presence of a robust positive effect of the leverage restriction on the wage growth of displaced workers, we next investigate the mechanism through which this occurs. We start by inspecting job search behavior after displacement and look at the duration of the job search, debt utilization, spousal income, hours, and employer and occupation characteristics. Our findings indicate that the borrowing restriction relaxes the constraints that leverage puts on job search and enables workers to improve their search. We provide several additional tests that support the mechanism we uncover. We then discuss how this mechanism relates to the channels through which unemployment benefits (UI) and access to credit have been shown to affect job search. We end the section by considering

alternative mechanisms that might generate opposing effects.

Table 9: **Through what mechanism does leverage affect starting wages?**

This table documents that LTV ratio restriction increases the displaced workers' unemployment spells and firm wage premiums of their new employers but does not affect debt utilization, working hours, or spousal income during the unemployment spell. Columns (1)-(4) and (7)-(10) use individual-level and columns (5) and (6) use household-level data, where observations between the first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Columns (1) and (2) use  $\ln(\text{Unemployment Spell})$  as the dependent variable. Columns (3) and (4) use  $\Delta \text{Firm Wage Premium}$  (i.e., the difference of firm wage premiums between the old and new employer) as the dependent variable. Firm wage premium is estimated using the AKM method (Abowd et al., 1999). Columns (5) and (6) use  $\Delta \ln(\text{Ex} - \text{Post Debt})$  (i.e., log change in household level debt after the year of displacement) as the dependent variable. Columns (7) and (8) use the change in  $\ln(\text{Spousal Income})$  as the dependent variable. Columns (9) and (10) use the change in the displaced worker's *Hours* as the dependent variable.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. *Post* equals one for the years after 2012 and equals zero for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	ln(Unemp. Spell)		$\Delta \ln(\text{Firm Wage Pre.})$		$\Delta \ln(\text{Ex-Post Debt})$		$\Delta \ln(\text{Spousal Inc.})$		$\Delta \text{Hours}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.386** (0.171)	0.347** (0.163)	0.009* (0.005)	0.012** (0.005)	-0.046 (0.193)	-0.094 (0.285)	-0.028 (0.054)	-0.021 (0.129)	0.088 (0.081)	-0.018 (0.113)
$d(\widehat{LTV} > 0.85)$	0.021 (0.088)	0.027 (0.305)	0.013** (0.006)	0.008 (0.007)	-0.025 (0.031)	-0.057 (0.059)	0.053* (0.030)	0.032 (0.041)	0.004 (0.012)	-0.006 (0.018)
<i>Fixed Effects:</i>										
Year FE		✓		✓		✓		✓		✓
Education FE		✓		✓		✓		✓		✓
Location FE		✓		✓		✓		✓		✓
Industry FE		✓		✓		✓		✓		✓
Obs.	2,060	2,060	2,088	2,088	1,782	1,782	937	937	2,088	2,088
R <sup>2</sup>	0.007	0.171	0.000	0.391	0.003	0.098	0.007	0.142	0.016	0.159

The probability of default has a highly skewed distribution, with some borrowers exposed to large increases in risk from shocks that are relatively innocuous to others. A negative income shock from job loss will, for example, particularly raise the risk of loan default for workers with high leverage. As a result, such workers may become more willing to accept early but potentially worse-paying job offers to attenuate default risk and associated costs (Ji, 2021). We test the hypothesis that the restriction can extend job search duration by reducing leverage by estimating the difference-in-differences model, with the log of displaced workers' unemployment spells, measured in days, as the dependent variable. Column (1) of Table 9 shows that treated workers with lower leverage have 35 percent longer unemployment

spells, the equivalent of a 50-day increase. We also saturate the model with year, education, location, and industry fixed effects to control for local economic effects and aggregate changes in economic conditions, as well as industry and education-driven variation in labor demand, as in the main regressions. Column (2) shows that including these fixed effects does not change our results. One potential concern about our estimation of job search duration is the right censoring created by workers who do not find a new job. Although only less than two percent of our sample do not find a new job within our sample period, we assess the importance of such workers as a robustness test in [Table A7](#). Specifically, we estimate alternative survival time models and hazard ratios and document that our results are robust to explicitly accounting for right censoring.<sup>24</sup>

After documenting that the reduction in leverage before job loss enables workers to have longer job search duration, we now ask whether the reduction in leverage helps workers find better employers. To this end, we follow [Abowd et al. \(1999\)](#) (AKM) and estimate the firm wage premium, that is, the average wage firms pay after controlling for employee characteristics, for all firms in our sample.<sup>25</sup> As a measure of the match quality, we take the difference between the wage premia of workers’ new and old employers,  $\Delta Firm Wage Premium$ , and use it as the dependent variable in our difference-in-differences setting. Columns (3) and (4) of [Table 9](#) establish that treated workers find jobs at employers that offer significantly higher wage premia than workers in the control group when we control for differences between years, education levels, and location, and industry fixed effects. The size of the coefficient in column (6) implies that about 30 percent of the increase in workers’ wage growth is driven by their finding jobs in higher-paying firms.

A channel through which household leverage could affect job search behavior is its influence on credit utilization during unemployment. [Herkenhoff et al. \(2024\)](#) and [Herkenhoff](#)

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<sup>24</sup>Note that the negative hazard ratio we estimate is in line with a longer job search.

<sup>25</sup>Specifically, we regress the log of wages on the employer, employee, and year fixed effects as well as employee characteristics. We remove the firms with fewer than five movers to reduce the labor mobility bias, and discard job seekers from our regression sample. The estimated firm fixed effects are then used as firm wage premia.



Table 10: **Impact of policy on job search breadth**

This table documents the effect of the LTV ratio policy on the job search breadth of displaced workers. Each column uses worker-level data. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable in columns (1) and (2) is a dummy variable, which takes the value of 1 if the worker changes her occupation in her new employer. The dependent variable in columns (3) and (4) is a dummy variable, which takes the value of 1 if the worker changes the industry in her new employer. The dependent variable in columns (5) and (6) is a dummy variable, which takes the value of 1 if the worker changes her job location in her new employer.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. *Post* equals one for the years after 2012 and equals zero for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Diff. Occupation		Diff. Industry		Diff. Job Location	
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.212**	0.286***	0.161*	0.227**	0.068	0.023
	(0.087)	(0.099)	(0.083)	(0.105)	(0.132)	(0.157)
$d(\widehat{LTV} > 0.85)$	0.029	0.014	0.041	0.021	0.064	0.043
	(0.024)	(0.026)	(0.029)	(0.024)	(0.049)	(0.054)
<i>Fixed Effects:</i>						
Year FE		✓		✓		✓
Education FE		✓		✓		✓
Location FE		✓		✓		✓
Industry FE		✓		✓		✓
Obs.	2,088	2,088	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.008	0.191	0.004	0.219	0.005	0.143

(2019) have documented that access to credit during the unemployment spell affects job search behavior and labor market outcomes. A policy-induced reduction of mortgage credit before the job displacement could facilitate the use of unsecured credit during unemployment since banks may consider workers with reduced leverage safer. Checking for the presence of such an effect is thus important for the interpretation of our findings. Our data set allows us to calculate the log change in ex-post debt using household balance sheet information. Columns (5) and (6) of Table 9 demonstrate that treated workers do not increase credit utilization during periods of unemployment, suggesting that the drivers of the job search and labor market effects we identify differ from those in Herkenhoff et al. (2024) and Herkenhoff (2019).

Next, we consider whether changes in spousal income could be a potential driver of displaced workers’ longer job search. Spouses of displaced workers may prefer to increase their labor supply to limit the decline in households’ total income. Columns (7) and (8) show there is no evidence supporting the presence of such a channel. We also investigate whether workers increase their working hours after a period of unemployment, thereby potentially explaining the rise in wages. Columns (9) and (10) establish there is no significant change in hours worked.

Another mechanism by which the leverage restriction influences job search is job search broadness. The restriction can enable workers to broaden their job search since it reduces risk aversion by decreasing consumption commitment (Chetty and Szeidl, 2007). For this purpose, we identify three intuitive proxies for job search broadness: switching to a different occupation and finding a new job in a different industry or in another location. The results in Table 10 indicate that the restriction induces workers to broaden their job search along some margins. Columns (1) and (2) show that displaced workers with lower leverage are 29 percent more likely to take a different occupation when starting at their new employer. Columns (3) and (4) demonstrate that these workers also have a 23 percent higher probability of finding their new jobs in another industry than they worked in before. One important consequence of the LTV restriction is that it can change housing consumption. For instance, workers may purchase smaller homes or homes located in cheaper areas, which can explain our finding of lower home purchase values after the restriction in Table A2. The main influence of the change in housing consumption on job search is labor mobility, as a different home location may alter where a worker looks for a new job. We investigate the change in labor mobility between commuter zones in the last two columns of Table 10 and do not find any effect. This result emphasizes that the mechanism we document is different from the effect of negative home equity on labor mobility in Bernstein (2020) and Gopalan et al. (2020).

**Comparison to other mechanisms** The mechanism through which the decline in household leverage before job loss affects job search and subsequent wages is related to but different

from what earlier research has found about the effect that UI and access to credit during unemployment have. Thus, we discuss how our estimates compare to those found in these strands of literature. The UI literature finds mixed effects of UI on wages. While several papers document insignificant effects (e.g., [Card et al. \(2007\)](#); [Johnston and Mas \(2018\)](#)), [Nekoei and Weber \(2017\)](#) estimate that a 30 percent increase in UI duration leads to a two percent longer spell and 0.5 percent higher wages. Among studies on the effect of credit access during unemployment, [Herkenhoff et al. \(2024\)](#) find that an increase in credit limits worth 10% of prior annual earnings extends unemployment spells by 2-3 days and wages by 1.85 percent. Similarly, [He and le Maire \(2023\)](#) find that liquidity-constrained unemployed increase their spells by 5.4 days, which leads to 3.6 percent higher wages. Our estimates on job search duration and wages are larger than what has been shown in these two strands of literature. The main difference between the UI’s mechanism and the one we document is that while UI may enable job seekers to have a better job search by providing additional liquidity, it is also prone to moral hazard, which can distort incentives and lower the starting wages ([Chetty, 2008](#)). The access to credit mechanism differs from ours due to its different implications on the default probability. Increasing debt use during unemployment relaxes, similar to UI, liquidity constraints but also increases default probabilities. Hence, a risk-averse job seeker may be conservative in relying on debt while being unemployed to extend her job search duration to avoid a costly default. Yet, a decline in leverage before job loss lowers default probabilities and relaxes liquidity constraints simultaneously, thanks to smaller debt-related payments. A reduced default probability, in turn, allows job seekers to have a longer job search duration, better matches in the labor market, and higher starting wages.

Although we provide evidence that leverage constraints improve job search, the LTV restriction could induce alternative channels that yield opposite effects. First, by directing a larger share of wages to debt-related payments, higher household leverage reduces workers’ willingness to work, leading workers to require higher wages ([Donaldson et al., 2019](#)).

Therefore, an LTV restriction could potentially lower wages by curtailing the debt overhang. As limited liability is required for this channel to work and mortgages in Norway are full recourse, debt overhang is not expected to influence wages in our setting. Second, the LTV restriction could also shorten the duration of the job search and hence lower the wages if workers use their liquid savings for the down payment requirements. We fail, however, to find a significant decline in deposits in [Figure A5c](#), indicating that workers respond to the restriction by purchasing cheaper homes and increasing their savings before making home purchases.

## 5.4 Heterogeneity

Together, our findings provide a clear picture of the mechanism through which a policy-induced borrowing constraint raises workers' starting wages. Lower leverage allows displaced workers to wait longer before accepting a new job. The longer and broader search leads them increasingly to new occupations at firms in different industries that pay higher wage premia. Intuitively, we expect this mechanism to be stronger for people who are in a position to benefit more from a less constrained job search. To examine this, we rerun the wage growth regressions on sub-samples using three criteria.

The first criterion is age. We expect younger people to respond strongly to a less-constrained job search since it is easier for them to switch to other occupations and industries. This can be due to the fact that acquiring new skills necessary for a new occupation or industry is easier for younger workers. The first two columns of [Table 11](#) confirm that the effect is stronger for younger workers. Next, we split our sample by the workers' tenure at their previous employer. Working for the same firm for a long time may diminish a worker's job search skills and lead to the accumulation of firm-specific human capital that is of limited value to new employers. For such workers, it may be challenging to exploit the opportunity of a less constrained job search. Columns (3) and (4) support this intuition. The wage growth of

workers with job tenure below the median is higher than in our main results, while the others experience a positive but insignificant change in their wages. Finally, we compare workers with educational attainment equal to or below upper secondary school with those who have a university degree. We expect higher education to facilitate switching to new occupations or industries, enabling workers with a high education level to benefit. [Eriksson and Rooth \(2014\)](#) have documented that longer unemployment spells diminish employers' return rates to job applications for medium- and low-skill jobs, suggestive of a negative correlation between search and starting wages for workers with lower education levels. Columns (5) and (6) show wage growth improves more for workers with a high education level, which is in line with our expectations.

Table 11: **Heterogeneous effects of policy on wage growth**

This table documents the heterogeneous effect of the LTV ratio policy on wage growth for displaced workers. Each column uses worker-level data. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is wage growth between the wage in the previous job and the wage in the new job. Columns (1) and (2) split the sample in terms of worker age, where "Low" refers to workers younger than the sample median. Columns (3) and (4) split the sample in terms of job tenure, where "Low" refers to tenures lower than the sample median. Columns (5) and (6) split the sample in terms of education, where "Low" refers to education levels upper secondary level and below, and "High" refers to education levels undergraduate level and above. Columns (7)-(9) split the sample in terms of worker income levels. Column (7) uses workers whose income lower than the sample's 25<sup>th</sup> percentile. Column (8) uses workers whose income levels are between the sample's 25<sup>th</sup> and 50<sup>th</sup> percentile. Column (9) uses workers whose income levels are higher than the sample's 75<sup>th</sup> percentile.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. *Post* equals one for the years after 2012 and equals zero for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

Wage Growth	Age		Tenure		Education		Income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Low	High	Low	High	Low	High	Low	Medium	High
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.048*** (0.017)	0.021 (0.024)	0.051** (0.022)	0.018 (0.024)	0.014 (0.026)	0.044** (0.017)	0.057* (0.029)	0.026 (0.025)	0.013 (0.018)
$d(\widehat{LTV} > 0.85)$	-0.018** (0.007)	-0.012 (0.009)	-0.021** (0.009)	-0.014 (0.010)	-0.024** (0.011)	-0.027** (0.012)	-0.021** (0.009)	-0.016* (0.007)	-0.009 (0.012)
<i>Fixed Effects:</i>									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Education FE	✓	✓	✓	✓			✓	✓	✓
Location FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Obs.	1,189	899	925	1163	657	1431	543	1,018	527
R <sup>2</sup>	0.165	0.224	0.162	0.175	0.112	0.121	0.183	0.174	0.234

Discussions about borrowing constraining policies point out that they affect the households with lower income more strongly, since affording the down payment is more demanding for such households (van Bekkum et al., 2024). However, because of the non-linearity in default risk, the same reduction in leverage can generate larger reductions in default risk for lower-income households and thereby bigger improvements in their wages. The last three columns of Table 11 document that the improvement in starting wages is significantly stronger for workers from low-income households. This finding suggests that even though a borrowing restriction can affect the low-income household negatively during the home-purchasing process, it may allow them to improve their wages after a job loss.

## 5.5 Longer-term effects

The effect of leverage on starting wages that we identified in section 5.2 could be temporary. If previously displaced workers whose starting wage is lower continue to search for better-paying jobs after accepting an initial job offer, then the effect of leverage on wages would be attenuated over time. If, however, search intensity falls after job acceptance or when human capital quickly becomes firm-specific, the effect could be long-lasting. To document the persistence of the effect we estimate, we track workers' annual wages for four years after their displacement. Then, we calculate the growth rates of wages during these four years and use this variable as the dependent variable in the difference-in-differences model. We report the results in Table 12. The policy-induced reduction in leverage raises the four-year wage growth by 2.5 pp. The magnitude of the effect is robust to saturating the model with year, education, location, and industry fixed effects. Together, these findings establish that the increase in wage growth is robust and not short-lived.

Finally, we consider the volatility of the treated workers' wages. Section 5.3 establishes that the policy facilitates switching to other occupations and industries. Shifting to other occupations or industries may increase wage volatility due to a lack of appropriate experience

Table 12: **Long-term effects**

This table documents the long-term effects of the LTV ratio policy on wage growth for displaced workers. Each column uses worker-level data, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Columns (1)-(2) use four-year wage growth as the dependent variable. Columns (3)-(4) use four-year wage volatility as the dependent variable.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value.  $Post$  equals 1 for the years after 2012 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth		Wage Volatility	
	(1)	(2)	(3)	(4)
$d(\widehat{LTV} > 0.85) \times Post$	0.028*** (0.011)	0.025*** (0.009)	-27.276*** (9.423)	-25.264** (11.743)
$d(\widehat{LTV} > 0.85)$	0.014 (0.022)	0.011 (0.026)	2.419 (4.257)	5.514* (2.543)
<i>Fixed Effects:</i>				
Year FE		✓		✓
Education FE		✓		✓
Location FE		✓		✓
Industry FE		✓		✓
Obs.	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.012	0.121	0.008	0.164

in these new occupations or industries. If, on the other hand, matching quality improves thanks to a less-constrained job search, then we expect to observe that treated workers have lower wage volatility after the restriction. To test how wage volatility is affected by the reduction in leverage, we calculate the standard deviation of annual wages for four years post-displacement and use it as the dependent variable in the last two columns of Table 12. Our results show that treated workers show lower wage volatility, suggesting that, in addition to improving wages, the restriction makes wages more stable. This finding supports our interpretation that treated workers have better matches in the labor market.

## 6 Discussion

Household leverage, wages, and job search are important drivers of the economy. Spurred by the Global Financial Crisis, many countries have implemented macroprudential policies to mitigate the undesirable consequences of high household leverage on the economy. While the literature has studied the implications of such policies on financial stability and housing decisions, empirical evidence on the labor market is scarce. We combine individual-level labor market, balance sheet, and housing transactions data from Norway to study how an LTV restriction affects job search and subsequent wages of displaced workers. We find that job-seeking workers with lower leverage, as a consequence of the borrowing constraint, earn higher wages in their new jobs. The restriction improves wages by enabling workers to have longer and broader job search and find jobs in better-paying firms. Moreover, the improvement in wage growth persists over time, implying that displaced workers do not achieve higher wages by accepting riskier jobs.

Documenting these previously unnoticed effects of household leverage on labor market outcomes is important for at least two reasons. First, our findings contribute to the discussions about the costs and benefits of macroprudential borrowing restrictions. While borrowing restriction policies may adversely influence access to housing markets and housing consumption, our findings indicate that they have, likely unintended, positive effects on labor market outcomes and mitigate the negative externalities of credit ([Farhi and Werning, 2016](#); [Dávila and Korinek, 2018](#); [Badarinza, 2019](#)). Thus, incorporating the unintended positive effects on labor market outcomes into the welfare calculations of macroprudential policy should yield more accurate conclusions. Second, our results highlight an additional adverse effect of household leverage on the economy. By restricting the duration and quality of job search, household leverage impairs the match between workers and firms. Thus, besides affecting financial stability, high household leverage worsens the functioning of labor markets. This suggests that policy discussions about (the risk of) recessions with high levels



of household leverage should also consider the negative effects of debt on households' job search.

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## Internet Appendix

## A1 Additional information on the data

This section provides additional information about the data sets we use in our paper. We use the Norwegian Standard Classification of Education at the three-digit level to measure educational attainment. Our education variable captures both the level and the broad field of education. The level indicates if a person has completed compulsory, intermediate, or higher education. The broad field refers to a general classification of academic content. There are 142 unique education levels in our sample. The levels are primary, lower secondary, upper secondary, post-secondary, the first stage of higher education, and second stage of higher education. The broad fields are humanities and arts, teacher training and pedagogy, social sciences and law, business administration, natural sciences, health, primary industries, and transport and communications. At the three-digit detail we can determine if a person with a social sciences and law background studied sociology or psychology. To capture changes in profession we use Statistics Norway’s seven-digit occupational information that builds on the EU ISCO-88 (COM) four-level classification system. The first digit defines 10 major groups that combine broad professions and inform about the level of competence. The upper ten classes are (1) legislators, senior officials and managers, (2) professionals, (3) technicians and associate professionals, (4) clerks, (5) service workers and shop and market sales workers, (6) skilled agricultural and fishery workers, (7) craft and related trades workers, (8) plant and machine operators and assemblers, (9) elementary occupations, and (10) armed forces and unspecified. The remaining digits break down each main occupational category into further subgroups.

## A2 Impact of the LTV constraint on household leverage

In this section, we detail the direct effect of the policy on households’ LTV, interest expenses, and home purchases. [Figure A4](#) shows  $\gamma_k$  from [Equation 3](#), where we use the LTV ratio as the dependent variable and provides visual evidence on the validity of the parallel trends assumptions and the effectiveness of the policy. Relative to the pre-policy baseline year of 2009, the LTV ratio of the treated and control groups evolves similarly in the pre-treatment period, supporting the parallel trends assumption. After implementation of the macroprudential policy, the LTV ratios of treated households’ fall significantly relative to the control group. [Table A1](#), presents the estimation results from the corresponding difference-in-differences model in [Equation 2](#) and confirms the visual intuition from [Figure A4](#). Column (1) of [Table A1](#) contains the parameter estimates from a regression without any controls. The estimated treatment effect is highly significant and negative. The negative coefficient on the term  $d(\widehat{LTV} > 0.85)_h \times Post_t$  implies that treated households have a 23 percentage points lower LTV ratio after the policy. When we include, in columns (2)-(6), year, education, location, industry, and location $\times$ industry fixed effects respectively to control for unobservables, the estimated remains virtually unchanged. The  $d(\widehat{LTV} > 0.85)_h$  coefficient reflects a 23 percentage points higher LTV ratio before the introduction of the policy. Post-treatment, the



treated and control groups have equal LTV ratios on average.

The treatment effect we find is larger than what other studies, like [van Bekkum et al. \(2024\)](#) and [Aastveit et al. \(2020\)](#) find. Two circumstances account for this difference. First, we removed households that obtain mortgages above the LTV threshold from the post-treatment period, because they must be part of the exemption quota and therefore aren't affected by the treatment. Second, our baseline sample selection we allowed for a wider LTV ratio distribution. Both effects work in the direction of increasing the relative decline in the LTV ratio of treated households.

Next we investigate how the macroprudential policy achieves this debt-reducing effect. We therefore examine how mortgage size, the price of purchased homes, and deposits change and again start by considering the year-by-year effects in [Figure A5](#). The visual evidence again supports the presence of parallel trends, for all three variables. We reconfirm the finding in the literature ([Tzur-Ilan, 2023](#); [van Bekkum et al., 2024](#); [Aastveit et al., 2020](#)) that LTV constraints reduce mortgage size ([Figure A5a](#)) and the cost of homes treated households buy ([Figure A5b](#)). A tighter borrowing constraint does not reduce treated households' liquidity by draining deposits [Figure A5c](#). [Table A2](#) indicates that treated household take on mortgages that are NOK 636,000 smaller to pay for homes that are NOK 516,000 cheaper.<sup>26</sup> In line with the lack of decline in deposits, we find that the LTV restriction has a similar negative impact on household leverage when it is calculated as (Total Debt - Deposits)/Income ([Table A4](#)).

Finally, we look into the policy's influence on households' cash flow. With smaller mortgages, we expect interest payments to decrease mechanically. A reduction in risk may also induce banks to charge a lower risk premium ([Elul et al., 2010](#); [Fuster and Willen, 2017](#); [Gupta and Hansman, 2022](#)). [Figure A6](#) confirms that treated and control groups behave similarly before the treatment and that the treated group significantly reduces interest expenses after the restriction. [Table A3](#) indicates that interest payments fall by NOK 36,000 due to the policy. Including principal repayments, we estimate that households' annual cash outflow improves by NOK 65,000. This is economically sizable and equivalent to about 10 percent of treated households' wages before displacement and 65 percent of the median households' deposits.

## A3 Random Forest algorithm

This section explains how we implement RF classification model. First, we describe data collecting process. Then, we explain how we select the model parameters and hyperparameter tuning.

As explained in [Section 3](#), we use several population registers. Merging these registers, we obtain the following variables: income, wage, deposits, debt, unemployment benefits, business income,

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<sup>26</sup>Households may borrow less for the renovation of purchased homes, or reduce consumption to finance home related expenditures.

age, education, location, and immigration status. Our data set allows us to observe the parents identifiers. Thus, we include parents' income, wealth, deposits, debt, education, and immigration status. Finally, to allow the model to consider macroeconomic conditions, we include GDP, inflation, monetary policy rate, unemployment rate, and regional and national house prices. For balance sheet variables (i.e. income, wage, deposits, unemployment benefits, business income, debt-to-income ratio), we use household level information, which means that we use the total values of these variables within the same household. For age, education, and immigration status, we use information pertaining to the household's main earner. Categorical variables (location, education, and immigration status) are used as dummy variables for each category. Macro variables enter the model both in levels and changes. We use national house price index to capture general housing conditions. Moreover, using transactions data, we calculate the mean and median house prices for each county and include both the levels and log changes of these values into the classification model.

The data period for the classification model is between 2002 and 2010. In this data period, households can obtain mortgages without any restriction on LTV ratios. This allows us to label the households as treated and control correctly (i.e. a household that obtains a mortgage with an LTV ratio above 85 percent is classified as treated, vice versa). Moreover, we keep the first-time home buyers whose LTV ratios are between 50 percent and 150 percent. Lastly, to reduce the overfitting problem, we remove the regression sample from the classification sample. Overall, there are 261,151 observations used in the RF classification estimation.

The RF model is estimated by *scikit – learn* machine learning library for the Python programming language. To select the model parameters, we use *RandomizedSearchCV* method for hyperparameter tuning. In a nutshell, this method tries random values from a specified value set and assigns score to these random values. Then, as a output, this method gives the parameters that produce best out-of-sample results. In our case, the best parameters are  $n\_estimators=200$ ,  $max\_features=sqrt$ ,  $min\_samples\_split=2$ ,  $min\_samples\_leaf=8$ . After fitting the model, the trained RF model is used to classify the regression sample.

An *ROC curve* plot is a popular method for evaluating the performance of classification models for binary labels. This plot has a true positive rate (proportion of treated units that are correctly identified) on the y-axis, and a false positive rate (ratio of false treated to total control units). Each dot represents the true and false positive rates for different probability thresholds for treatment assignment. For instance, if this threshold is set to 0, then every unit is classified as treated. This means that the false positive rate is one since all negative events are classified as treated. Also, the true positive rate is one since all true treated units are classified as treated. A successful classification model has a lower decline in the true positive rate than the false positive rate as we lower the probability threshold. In other words, closer a ROC curve is to the northwest of the plot, the more successful it is. AUC is used to measure this success. Higher AUC values indicate that the model is better at classifying the units, and a perfect model has AUC value of 1.

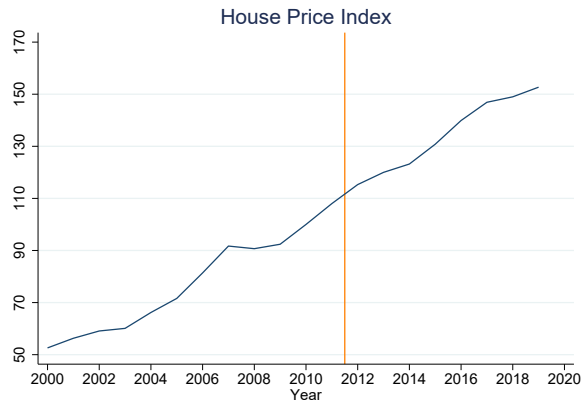
The *scikit – learn* library has a built-in *variable importance* feature, which calculates the importance by looking at the decrease in the mean impurity. However, this method can overstate the importance of categorical variables with higher cardinality.<sup>27</sup> Thus, we use permutation based variable importance. The basic idea of this method is that a variable is more important if the absence of this variable worsens the model’s performance more. First, we calculate the accuracy of the classification model with all variables. Then, we remove each variable and calculate the new accuracy. The reported scores are the percentage decline in the model’s accuracy when the variable is removed (i.e., the model’s accuracy is 7 percent lower when the household deposits variable is removed). Macro variables enter to the model with levels and changes. House price variable includes national house price index, mean and median of the regional house prices and their log changes of these variables. The scores of the categorical variables are calculated by removing all the dummy variables for that categorical variable.

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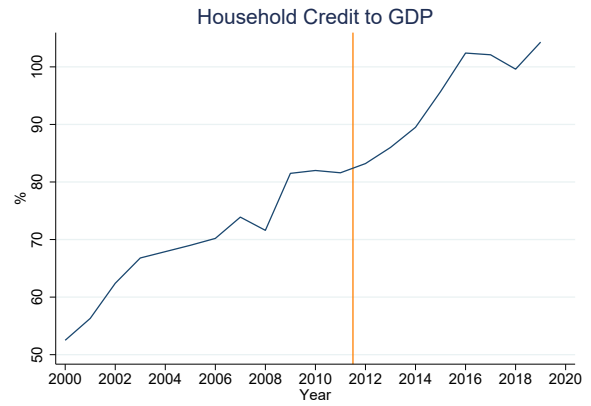
<sup>27</sup>We plot the variable importance that uses built-in function in [Figure A7](#).

Figure A1: Macroeconomic conditions

This figure shows the macroeconomic conditions in Norway between 2000 and 2019. [Figure A1a](#) plots the house price index, [Figure A1b](#) plots the household credit to GDP ratio [Figure A1c](#) plots the GDP growth, [Figure A1d](#) plots the unemployment rate, [Figure A1e](#) plots inflation, and [Figure A1f](#) plots monetary policy rate. The orange line indicates the date of the LTV ratio restriction.



(a)



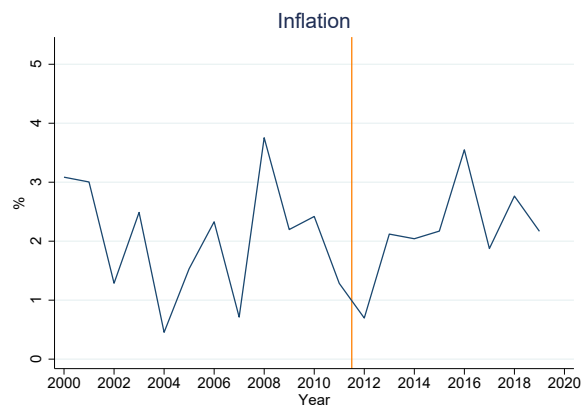
(b)



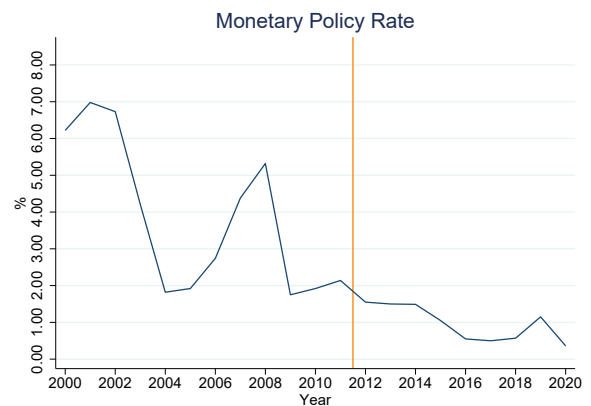
(c)



(d)



(e)



(f)

Figure A2: **Receiver Operating Characteristic curve**

This figure shows the Receiver Operating Characteristic (ROC) curve for the regression sample. The x-axis shows the false positive rate and the y-axis shows the true positive rate. The orange line shows the false positive rate and true positive rate of a random classifier. The blue line shows the false positive rate and true positive rate of the Random Forest model for the regression sample. Each dot on these curves represents false positive rate and true positive rate for different classification thresholds. The area under the curve (AUC) summarize the success of the classification model.

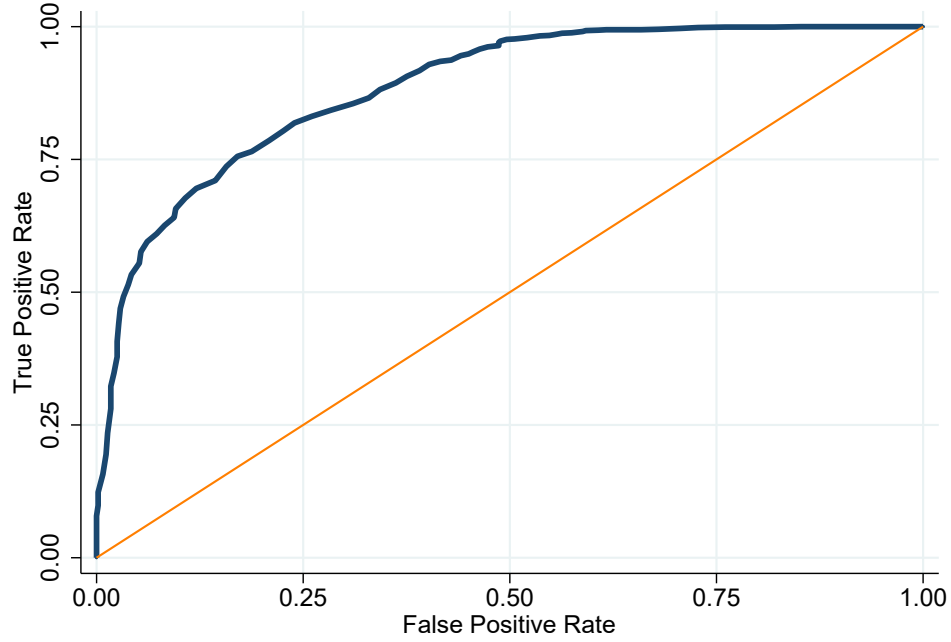


Figure A3: **Regional house prices**

This figure plots the regional house price growth rates for the nine largest counties. The blue dots show the house price growth rates before the LTV restriction for four years. The orange dots show the house price growth rates after the LTV restriction for two years.

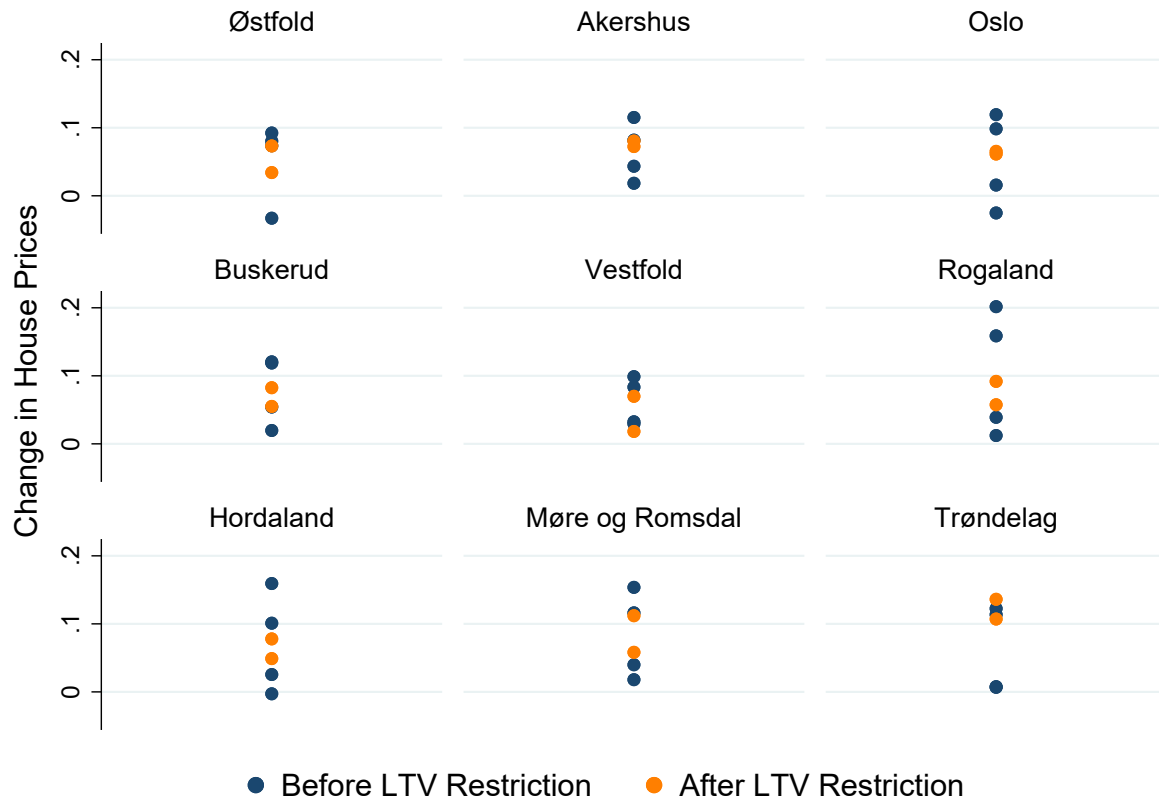
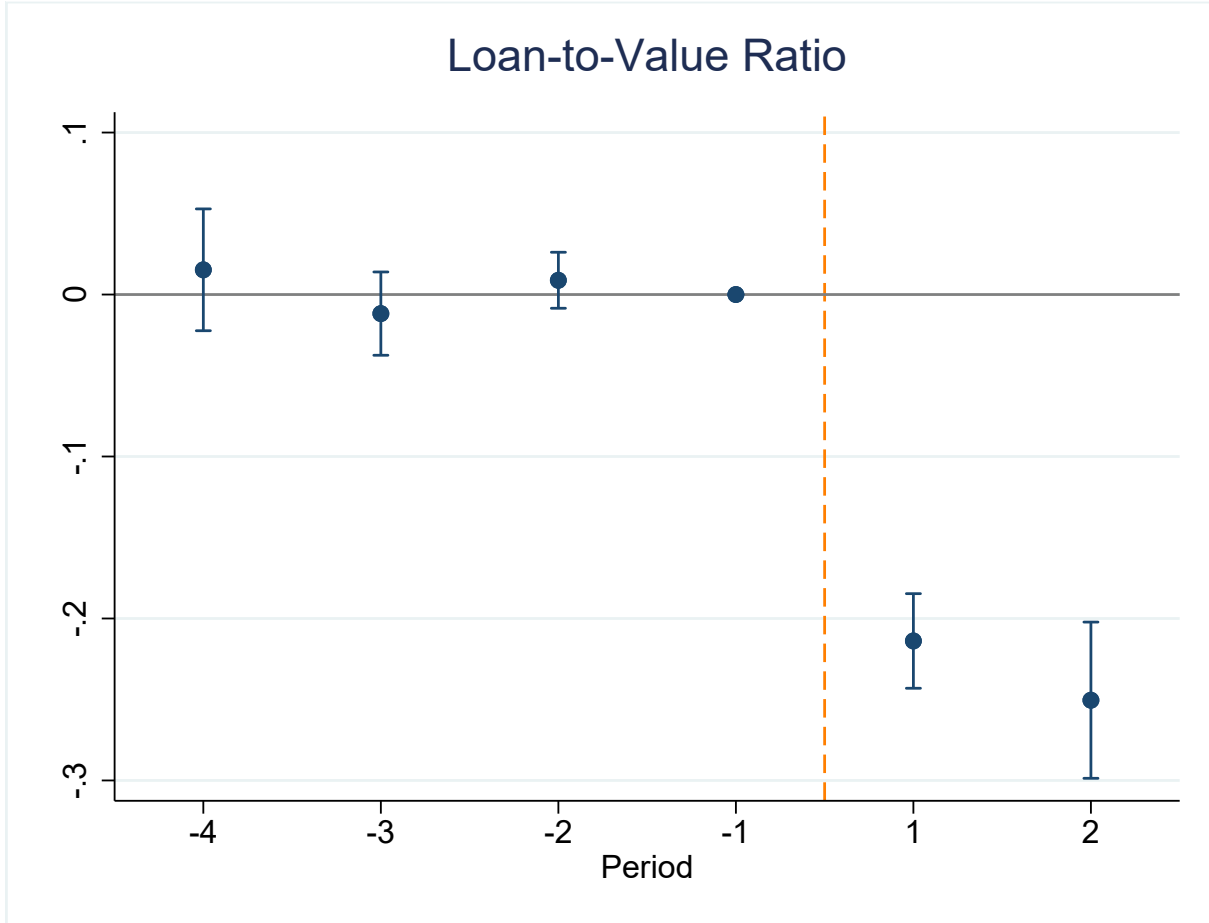


Figure A4: **Dynamic impact of the LTV policy on the LTV ratio**

This figure shows the dynamic effect of the LTV policy on the LTV ratio. The sample is worker-level data, where LTV is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is the LTV ratio calculated from tax filings and housing transactions register at household level.  $d(\hat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. The figure shows the  $\beta$ s on the y-axis of the regression model,  $LTV_{ht} = \sum_{k=-4}^2 \gamma_k D_k \times Treated_{ht} + Treated_{ht} + \epsilon_{ht}$ . Baseline event period is  $k = -1$ . Regression model includes year fixed effects. Orange bar specifies the implementation of LTV restriction. Standard errors are two-way clustered at location and industry level and bars indicate 95% confidence intervals.



**Figure A5: Dynamic impact of the policy change on mortgages, house prices and deposits**

This figure shows the dynamic effect of the LTV policy on mortgages, house prices, and deposits. The sample is household-level data, where mortgages, house prices, and deposits are measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variables are mortgages, house prices, and deposits. All dependent variables are measured in NOK 1000.  $d(\hat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. Figure shows the  $\beta$ s on the y-axis of the regression models,  $y_{ht} = \sum_{k=-4}^2 \gamma_k D_k \times d(\hat{LTV} > 0.85)_h + d(\hat{LTV} > 0.85)_h + \epsilon_{ht}$ . Baseline event period is  $k = -1$ . Regression models include year-fixed effects. Orange bar specifies the implementation of LTV restriction. Standard errors are two-way clustered at location and industry level and bars indicate 95% confidence intervals.

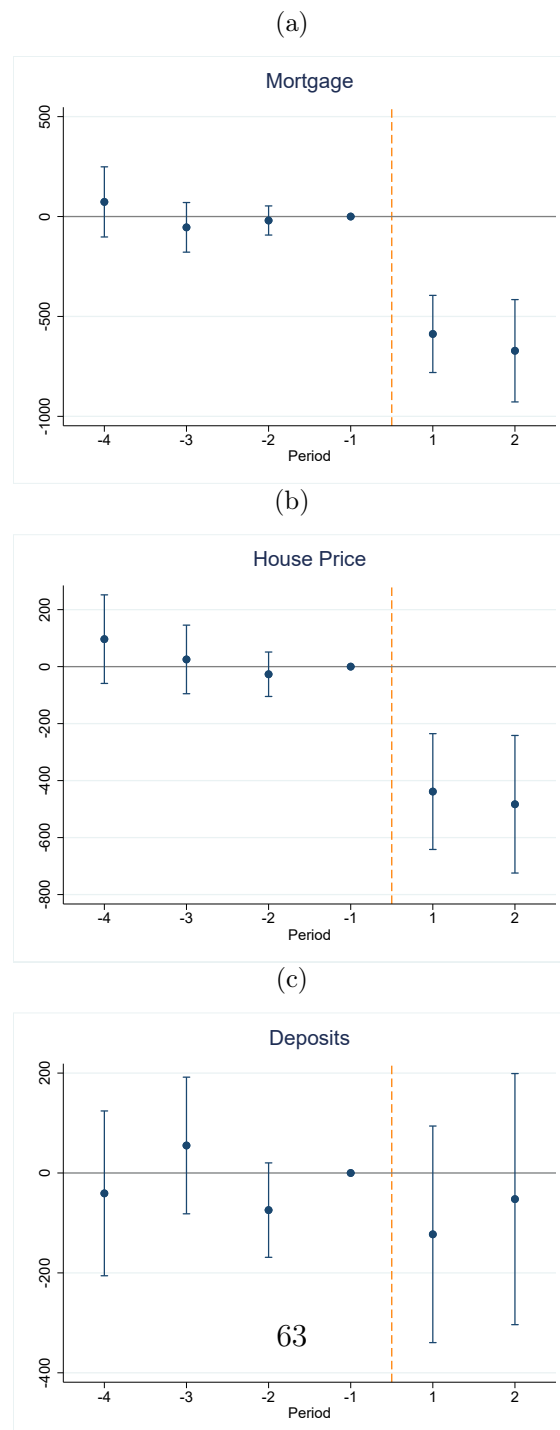




Figure A6: **Dynamic impact of the policy change on interest expenses**

This figure shows the dynamic effect of the LTV policy on workers' interest expense. The sample is worker-level data, where interest expense is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is interest expense, measured in NOK 1000.  $d(\hat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. Figure shows the  $\beta$ s on the y-axis of the regression model,  $interest\ expense_{ht} = \sum_{k=-4}^2 \gamma_k D_k \times d(\hat{LTV} > 0.85)_h + d(\hat{LTV} > 0.85)_h + \epsilon_{ht}$ . Baseline event period is  $k = -1$ . Regression models includes year fixed effects. Orange bar specifies the implementation of LTV restriction. Standard errors are two-way clustered at location and industry level and bars indicate 95% confidence intervals.

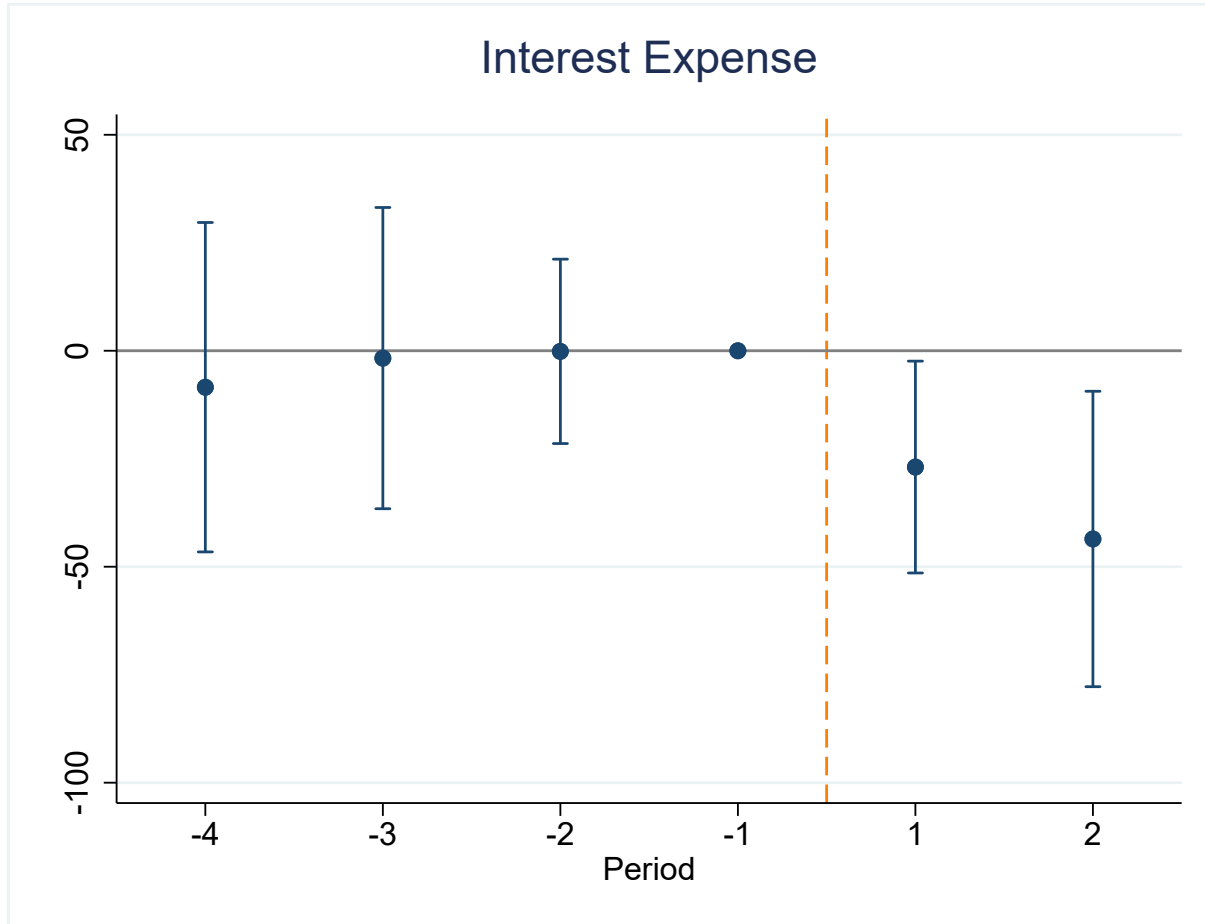


Figure A7: **Variable importance**

This figure shows the variable importance for the variables used in RF classification model. Variable importance is calculated by feature importance, which evaluates the variable importance by the decrease in mean impurity.

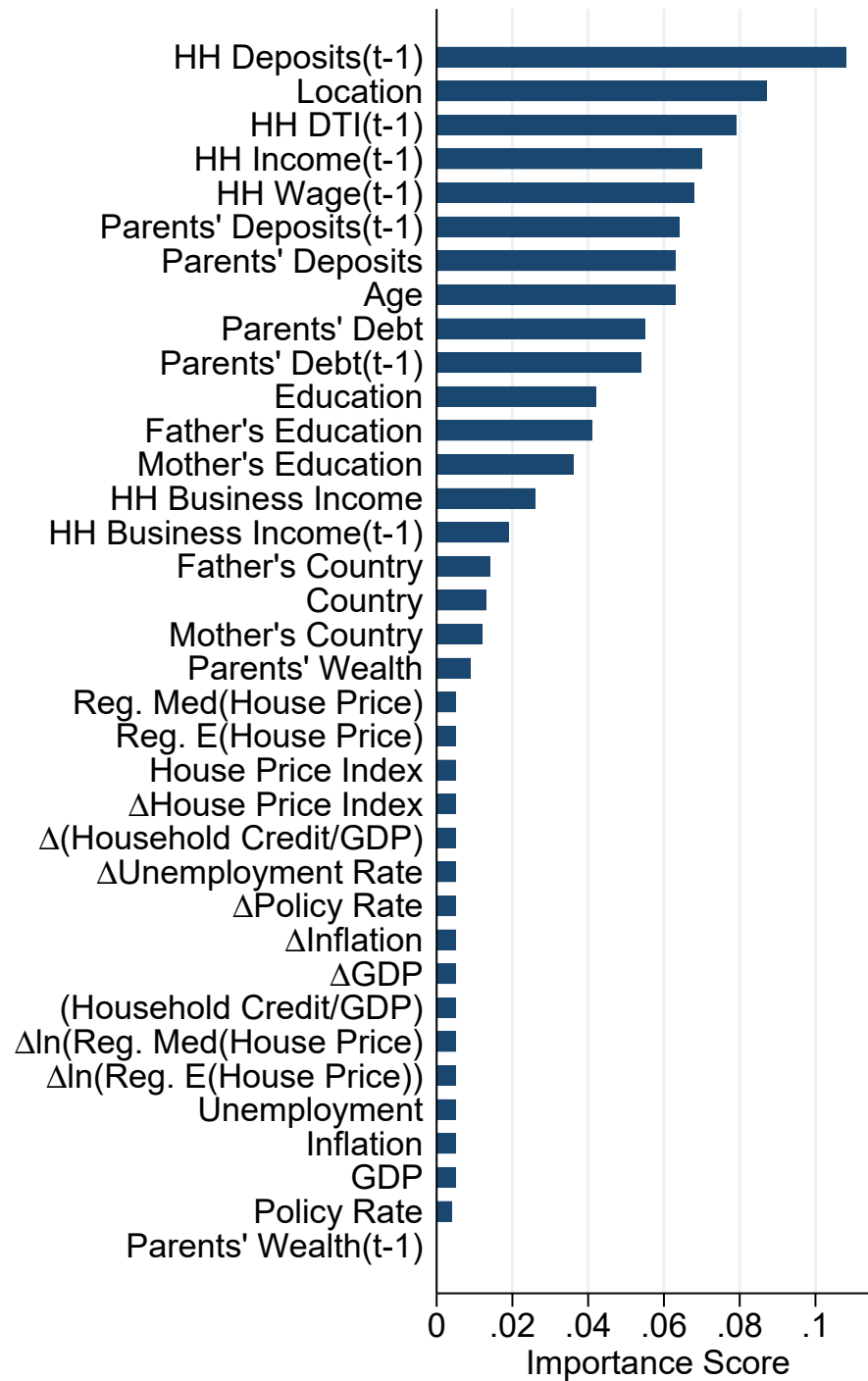


Figure A8: **Transition into homeownership rate**

This figure plots the percentage of first time homebuyers over the total population. The x-axis shows the years. The y-axis shows the detrended ratio of first time homebuyers divided by total population. The vertical lines indicate the implementation of the LTV restrictions.

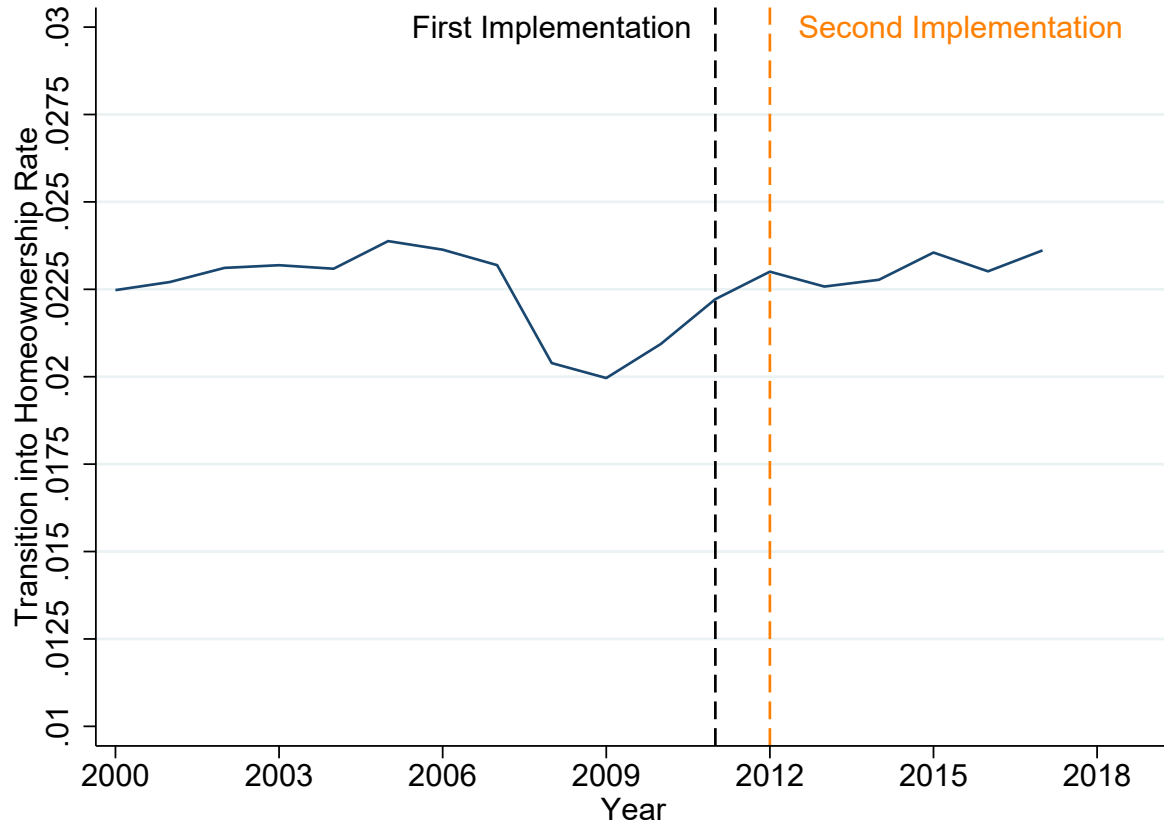


Figure A9: **LTV ratio distribution before and after the restriction**

This figure plots the distribution of LTV ratios before and after the restriction. Orange bars indicate the period after the restriction. Blue bars indicate the period before the restriction.

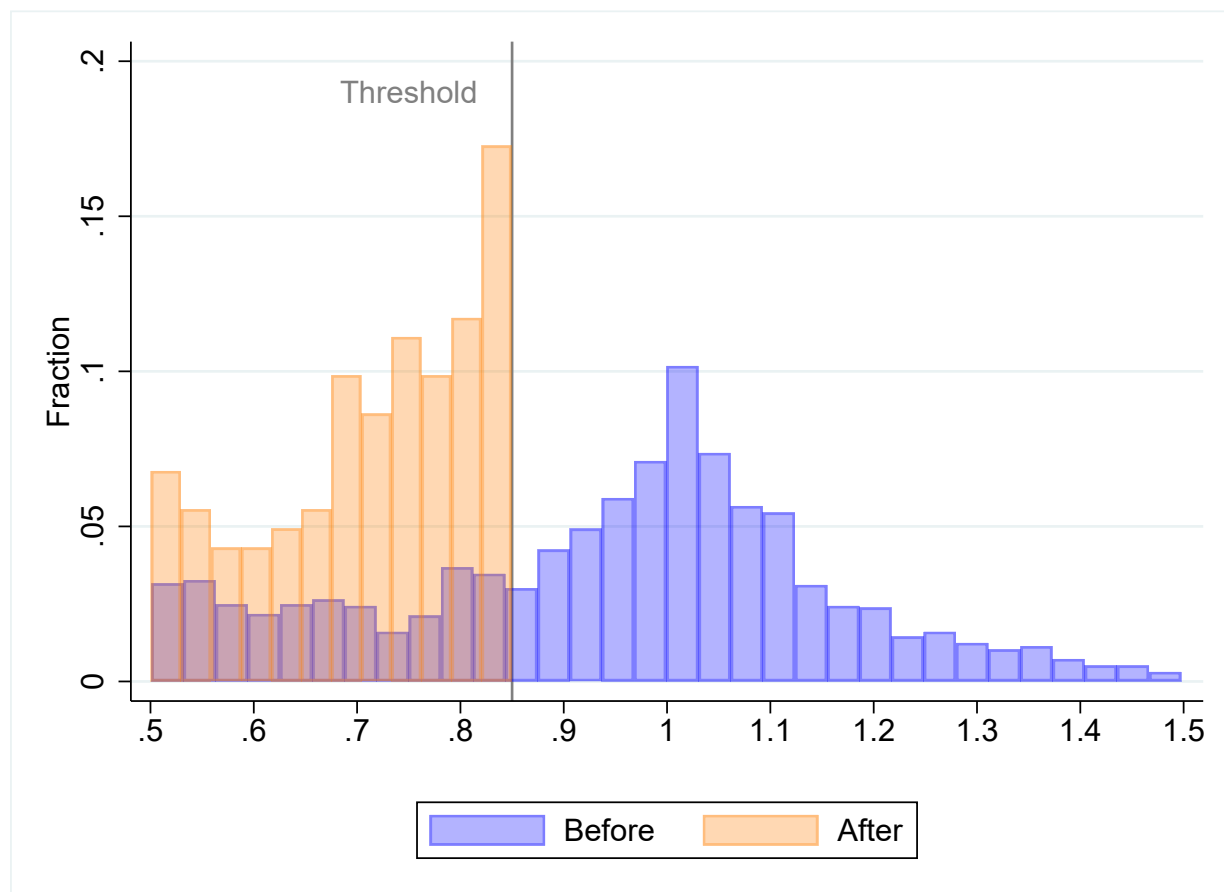


Table A1: **Impact of the policy change on the LTV ratio**

This table documents the effectiveness of the LTV ratio policy on the LTV ratios. Each column uses worker-level data, where LTV is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is LTV ratio calculated from tax filings and housing transactions register at household level.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value.  $Post$  equals 1 for the years after 2012 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	LTV					
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times Post$	-0.229*** (0.018)	-0.232*** (0.021)	-0.231*** (0.022)	-0.224*** (0.018)	-0.228*** (0.015)	-0.221*** (0.022)
$d(\widehat{LTV} > 0.85)$	0.231*** (0.013)	0.234*** (0.012)	0.224*** (0.016)	0.214*** (0.014)	0.217*** (0.014)	0.219*** (0.021)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location $\times$ Industry FE						✓
Obs.	2,088	2,088	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.212	0.221	0.279	0.287	0.298	0.351

Table A2: **Impact of macroprudential policy on mortgages, house prices, and deposits**

This table documents the effect of the LTV ratio policy on mortgages, house prices, and deposits. Each column uses worker-level data, where mortgages, house prices, and deposits are measured at household level and observations between the first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Columns (1)-(2) use mortgage size as the dependent variable. Columns (3)-(4) use house price as the dependent variable. Columns (5)-(6) use deposits as the dependent variable. All dependent variables are measured in NOK 1000.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. *Post* equals 1 for the years after 2012 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Mortgage		House Price		Deposits	
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	-597.148*** (105.254)	-636.102*** (126.417)	-442.287*** (122.914)	-515.945*** (126.128)	-72.562 (83.651)	-103.991 (117.376)
$d(\widehat{LTV} > 0.85)$	-113.817* (62.176)	82.156 (52.379)	-492.756*** (94.157)	-231.554** (101.254)	-187.653*** (13.951)	-172.536*** (48.431)
<i>Fixed Effects:</i>						
Year FE		✓		✓		✓
Education FE		✓		✓		✓
Location FE		✓		✓		✓
Industry FE		✓		✓		✓
Location $\times$ Industry FE						✓
Obs.	2,088	2,088	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.041	0.248	0.122	0.317	0.098	0.251

Table A3: **Impact of macroprudential policy on interest expense**

This table documents the effect of the LTV ratio policy on the workers' interest expense. Each column uses worker-level data, where interest expense is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is interest expense, measured in NOK 1000.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value.  $Post$  equals 1 for the years after 2012 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Interest Expense					
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times Post$	-41.231*** (9.903)	-40.357*** (8.751)	-39.365*** (12.482)	-36.504** (13.213)	-36.101** (14.647)	-38.201*** (13.325)
$d(\widehat{LTV} > 0.85)$	-7.038** (2.894)	-6.637*** (2.731)	-3.976 (3.742)	-2.521 (3.182)	-1.956 (5.156)	1.851 (4.024)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location $\times$ Industry FE						✓
Obs.	2,088	2,088	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.015	0.112	0.219	0.234	0.271	0.314

Table A4: **Impact of policy on DTI ratio**

This table documents the effectiveness of the LTV ratio policy on debt (net of deposits)-to-income (DTI) ratios. Each column uses household-level data between 2006 and 2013, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is DTI ratio calculated from tax filings and is the ratio of total debt minus deposits to total income.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value.  $Post$  equals 1 for the years after 2012 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	$\frac{Debt-Dep.}{Income}$					
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times Post$	-1.027*** (0.327)	-1.014*** (0.337)	-0.974*** (0.316)	-0.876** (0.342)	-0.921** (0.351)	-0.897** (0.419)
$d(\widehat{LTV} > 0.85)$	0.784*** (0.142)	0.765*** (0.122)	0.874*** (0.131)	0.892*** (0.152)	0.893*** (0.153)	0.883*** (0.161)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location $\times$ Industry FE						✓
Obs.	2,088	2,088	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.024	0.034	0.142	0.174	0.212	0.257



Table A5: **Robustness checks for wage growth**

This table provides additional robustness checks for the effect of the LTV ratio policy on wage growth. Unless reported otherwise, columns use household-level data, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The dependent variable is wage growth between the wage in the previous job and the wage in the new job. Column (1) uses 2004 as the starting year. Column (2) uses 2007 as the starting year. Column (3) excludes households that obtain transfers larger than NOK 10,000 in the sample period. Column (4) excludes households that obtain positive business income between 2000 and 2017. Column (5) interacts  $d(\widehat{LTV} > 0.85)$  with four main macro variables: inflation, unemployment, GDP growth, and monetary policy rate. Column (6) interacts  $d(\widehat{LTV} > 0.85)$  with education levels. Column (7) does a placebo test, in which *Placebo* is equal to 1 for the years 2009 and 2010 and equals 0 for earlier years.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. *Post* equals one for the years after 2012 and equals zero for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	2005	2007	No Transf.	No Bus. Inc.	Macro	Education	Placebo
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.042** (0.017)	0.043** (0.021)	0.040** (0.018)	0.043** (0.019)	0.046** (0.021)	0.041* (0.022)	
$d(\widehat{LTV} > 0.85) \times \text{Placebo}$							-0.011 (0.084)
$d(\widehat{LTV} > 0.85)$	-0.17** (0.006)	-0.020*** (0.009)	-0.017** (0.007)	-0.018*** (0.006)	-0.022* (0.012)	-0.012* (0.007)	0.009 (0.082)
<i>Fixed Effects:</i>							
Year FE	✓	✓	✓	✓	✓	✓	✓
Education FE	✓	✓	✓	✓	✓		✓
Location FE	✓	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓	✓
Treated $\times$ Macro Var.					✓		
Treated $\times$ Education FE						✓	
Obs.	2,402	1,727	1,875	1,928	2,088	2,088	1,353
R <sup>2</sup>	0.119	0.125	0.132	0.126	0.132	0.182	0.153

Table A6: **Alternative target variables for assignment to treatment**

This table uses different target variables to classify workers into treatment and control groups. The first two columns use the LTV ratios in continuous form as the target variable and classify workers as treated if the predicted LTV ratio is above 85 percent. The last two columns use predicted DTI ratios as the target variable and classify workers as treated if the predicted DTI ratio is above the 35<sup>th</sup> percentile of the sample. *Post* equals one for the years after 2012 and equals zero for earlier years. Each column uses worker-level data. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth			
	(1)	(2)	(3)	(4)
$\mathbb{1}(\widehat{LTV} > 0.85) \times \text{Post}$	0.049** (0.026)	0.041** (0.021)		
$\mathbb{1}(\widehat{LTV} > 0.85)$	-0.023*** (0.006)	-0.021*** (0.007)		
$\mathbb{d}(\widehat{DTI} > 3.15) \times \text{Post}$			0.051* (0.028)	0.046** (0.025)
$\mathbb{d}(\widehat{DTI} > 3.15)$			-0.027*** (0.009)	-0.024*** (0.008)
<i>Fixed Effects:</i>				
Year FE		✓		✓
Education FE		✓		✓
Location FE		✓		✓
Industry FE		✓		✓
Obs.	2,088	2,088	2,088	2,088
R <sup>2</sup>	0.007	0.118	0.008	0.122

Table A7: **Job search duration**

This table uses different survival models to assess the importance of right censoring for the job search duration result. The first column uses a parametric survival-time model with lognormal survival distribution. The second column uses a loglogistic survival distribution. The third column uses exponential survival distribution. The fourth column uses exponential survival distribution and reports the hazard ratio.  $d(\widehat{LTV} > 0.85)$  takes the value of 1 if the predicted LTV ratio is larger than the LTV threshold value. *Post* equals one for the years after 2012 and equals zero for earlier years. Each column uses worker-level data. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location level and reported in parentheses. \*, \*\*, and \*\*\* indicate significance at 10% level, 5% level, and 1% level, respectively.

	Unemployment Spell			
	(1)	(2)	(3)	(4)
	log-normal	log-logistic	Exponential	Exponential
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.335** (0.151)	0.296** (0.139)	0.745** (0.135)	-0.294** (0.135)
$d(\widehat{LTV} > 0.85)$	0.021 (0.091)	0.026 (0.087)	0.993 (0.053)	-0.006 (0.053)
<i>Fixed Effects:</i>				
Year FE	✓	✓	✓	✓
Education FE	✓	✓	✓	✓
Location FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
Obs.	2,060	2,060	2,060	2,060