

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 workers' job search, matching in the labor market, and wages? Theoretically, household leverage has been shown to have opposing effects on the labor market through, among others, a debt-overhang and a liquidity constraint channel. To test which channels dominate empirically, we exploit the introduction of a macroprudential borrowing restriction that exogenously reduces household leverage in Norway. We study home-owners who lose their job and find that a reduction in leverage raises wages by 3.3 percentage points after unemployment. The mandated restriction of leverage enables workers to search longer for jobs, and thereby find positions in firms that pay higher wage premia and switch to new occupations and industries. We observe no evidence that a greater use of credit *during* unemployment drives the extended job search. The positive effect on wages is persistent and more pronounced for workers who are more likely to benefit from improved job search, such as young people. Our findings contribute to the debate on the costs and benefits of policies that constrain household leverage and show that such policies, while primarily aiming at enhancing financial stability, have other positive effects such as improved 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

Household leverage can pose a challenge to the economy through several channels. An increase in household leverage can fuel a housing boom, predict lower GDP growth and higher unemployment, or weaken financial stability.¹ In the wake of the global financial crisis, many countries therefore adopted policies to restrict household leverage. These policies face the challenge of properly trading off the costs of restricting borrowing in good times against the benefits of a less pronounced economic decline in bad times. Such trade-offs have sparked a debate about the effectiveness and side effects of measures to restrict household borrowing.² While existing research has primarily examined the effects of household leverage restrictions on the housing market and financial stability, we focus on the, yet unstudied, interaction of these restrictions with the labor market. Specifically, we study how a macroprudential borrowing restriction that exogenously reduces leverage affects the job search and wages of displaced workers who bought a house shortly before losing their jobs.

Theoretically, a borrowing restriction can affect the wages of displaced workers through multiple channels. First, a borrowing restriction can decrease wages through a debt-overhang channel. In this channel, 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, the borrowing restriction may lower wages to the extent of curtailing the debt-overhang. Second, a borrowing restriction may increase wages because an ex-ante reduction in leverage lowers the debt service pressure during the unemployment spell. Thanks to a lower debt service burden, workers can lengthen the duration or broaden the scope of their job search, enabling them to have higher wages in their new jobs (Chetty and Szeidl, 2007; Herkenhoff, 2019; Ji, 2021). The presence of these opposing channels makes the effect of a borrowing restriction on job search and subsequent wages an empirical question.

To test the relative strength of these opposing channels, we exploit the introduction of a loan-to-value (LTV) restriction that creates exogenous variation in household leverage and several administrative population registers in Norway. These population registers enable us to construct a unique data set, in which we observe workers' assets and liabilities, wages,

¹For housing booms, see Mian and Sufi (2011); Adelino et al. (2016) and Favilukis et al. (2017). For financial instability, see Schularick and Taylor (2012) and Reinhart and Rogoff (2008). For economic growth, see Mian et al. (2017).

²These policies can improve financial stability yet create some negative side effects for affected households (Farhi and Werning, 2016; Acharya et al., 2019; Araujo et al., 2019; Van Bakkum et al., 2019; Peydró et al., 2020). Tzur-Ilan (2020) and Aastveit et al. (2020) point out certain negative side effects that these policies may bring about.

employers, unemployment duration, job choice, housing transactions, and other individual characteristics such as education and immigration status. Using this setting, we find that the macroprudential borrowing restriction improves the wages of displaced workers in their new jobs. Specifically, we find a decline in a worker’s debt-to-income (DTI) ratio by 25 percent leads to a relative wage increase of 3.3 percentage points following displacement.³ 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 their unemployment duration by approximately 2.5 months. Second, they become more likely to switch to other occupations and industries, i.e, broaden their job search. Third, workers with lower leverage find jobs in firms that pay a higher wage premium. Finally, the improvement in wages does not wane over time and is not associated with a rise in income volatility. 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.⁴ Due to this feature, there is no variable distinguishing the affected workers, who take 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 the characteristics and LTV ratio decisions of homebuyers who bought their homes before the restriction. Since these homebuyers could take mortgages with LTV ratios either above or below the cap, we can correctly label them whether they would be affected by the restriction. We use these correctly classified homebuyers and their characteristics to classify our entire regression sample into treatment and control groups using a random forest (RF) algorithm, a machine learning method (Abadie, 2005).⁵ 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, while other workers are classified as control.

The second part considers the possibility that workers may endogenously alter the initiation of their job search as a reaction to the restriction. For instance, high-skilled workers

³Throughout the paper, we focus on the starting wage of displaced workers in their new job and refer to this as "starting wages" or "wages." Section 3 explains the construction of these variables.

⁴A small number of mortgages are exempted; see Section 4

⁵We train and validate the RF model with all first-time homebuyers before the policy, excluding the homebuyers who meet the criteria to be in the regression sample. Then, we use this trained RF algorithm to classify the regression sample. Section 4 explains the implementation of RF prediction model in detail.

may become more inclined to switch jobs to increase their earnings, skewing the skill distribution of job switchers. An analysis that does not properly control for this endogenous reaction may therefore produce biased estimates. We address this concern by using only displaced workers whose job search is not triggered by their individual characteristics since they lost their jobs due to mass layoffs. Furthermore, to avoid the accumulation of unobserved home equity prior to the layoff, we restrict our sample to those displaced workers who bought a house within 12 months before their displacement. With these two parts, our difference-in-differences setting compares those displaced workers who had recently bought a house and are likely affected by the LTV restriction (treatment group) to those unaffected by the restriction (control group) and provides three main results.

First, we show that the policy-induced decline in household leverage affects the labor market outcomes of displaced workers who recently bought a house before being displaced. Treated workers realize higher wage growth between the 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 3.3 percentage point smaller decline in wages compared to the 7.4 percentage point average reduction displaced workers in the control group experience. 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 or labor market policies in Norway. For instance, we show that the restriction does not affect the observable characteristics of the treatment group. In addition, removing all workers who would not have been able to make a down payment in the pre-restriction period or workers who make the most of the unemployment benefits and labor protection does not change our results.⁶

Second, we establish that the improvement in starting wages stems from the mitigation of the job search constraints higher leverage had created. This influences post-unemployment wages positively through several channels. Workers are able to extend their job search by 2.5 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 explains 20 percent of the estimated gain in wages. Workers also broaden the scope of their job search and are approximately 20 percent more likely to change their occupation or find a new employer in another industry. Changes in geographical labor mobility or investment in additional

⁶Because regulatory borrowing restrictions can affect house purchase decisions directly, i.e., not only through borrowing capacity, workers that cannot afford the down payment may decide to buy a house before the implementation of the LTV restriction or delay their purchase until after our sample period when they have saved for a down payment. We verify that this is not driving our findings.

education are not drivers of our results.

Third, we provide further support for the presence of the above mechanism that works through relaxing job search constraints by documenting how heterogeneity across the sample influences the effect on wages. In line with the mechanism, we find that workers younger than the median age, having a shorter job tenure with the previous employer, or with higher education drive the improvement in wages. This is consistent with the notion that it is easier for younger workers or workers with 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 better 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 larger for low-income workers

Finally, we find that the positive effect on wages is persistent over the four-year post-displacement period we observe. Treated workers also enjoyed lower wage volatility during these four years, indicating that the rise in wages is not attributable to taking jobs with high wages but with a large discontinuation risk. Moreover, we document that the negative relationship between household leverage and wages also holds in wider samples, even though these wider samples may be prone to bias due to the endogeneity of leverage and job search initiation.

In sum, we document that household leverage constrains job search, and a macroprudential policy that limits household leverage relaxes these constraints and generates unintended positive effects on labor market outcomes. Our results thus provide new insights into the way in which household leverage, through labor markets, interacts with the real economy. This direct effect of household leverage on the labor market is potentially important for policymakers as high household leverage has been a common characteristic of recent recessions, and household debt levels continue to be elevated in many countries.

The findings in our paper speak to at least three strands of the literature. Our first contribution is to the debate on the costs and benefits of macroprudential policies, which have become widely popular after the Global Financial Crisis. These policies can potentially curb credit booms and improve financial stability (Cerutti et al., 2017; Van Bakkum et al., 2019; Defusco et al., 2019; Araujo et al., 2019; Peydró et al., 2020) but can also generate adverse side effects, such as reducing access to housing (Acharya et al., 2019; Aastveit et al., 2020; Tzur-Ilan, 2020).⁷ We contribute to this discussion by documenting a previously overlooked

⁷See Farhi and Werning (2016) and Dávila and Korinek (2018) for theoretical justifications for macroprudential policies.

positive effect of these policies on labor market outcomes.⁸ Therefore, our findings indicate that the discussions on the welfare implications of macroprudential policies may benefit from incorporating the unintended effects shown in this paper.

Second, we add to the literature that studies how the interaction between household debt and access to credit affects labor markets through a demand channel. This channel originates in the detrimental effect of household leverage on credit availability via a deterioration in financial stability or in collateral values that subsequently triggers a deleveraging by households (Reinhart and Rogoff, 2008; Schularick and Taylor, 2012; Corbae and Quintin, 2015; Adelino et al., 2016). Due to the deleveraging, households cut their spending (Eggertsson and Krugman, 2012; Mian et al., 2013; Guerrieri and Lorenzoni, 2017), which weakens the 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 the job search and matching quality and demonstrating that a policy-induced reduction in household leverage can mitigate the large and long-lasting decline in earnings following a job loss that earlier research has found.⁹

Third, our paper relates to studies about the effect of household balance sheets on job search. This literature has found that negative home equity following a decline in house prices limits labor mobility and thereby impairs labor supply (Bernstein and Struyven, 2017; Brown and Matsa, 2019; Gopalan et al., 2020; Bernstein, 2020), and that access to credit via credit cards or home equity loans enables workers to have better job search (Herkenhoff, 2019; He and le Maire, 2020; Kumar and Liang, 2018).¹⁰ We contribute to this literature by, to the best of our knowledge, providing the first causal evidence of macroprudential constraints of household leverage on job search behavior and wages.¹¹ We identify a negative effect of household leverage generates on job search that is consistent with Ji (2021) rather than with the presence of a debt overhang channel as in Donaldson et al. (2019).

The rest of the paper is organized as follows: Section 2 provides information about

⁸In 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.

⁹See 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.

¹⁰This is similar to the liquidity effect of unemployment insurance (Chetty, 2008). Interest payments can also influence labor supply decisions through a consumption commitment channel (Chetty and Szeidl, 2007; Zator, 2019). See also Mulligan (2009, 2010); Li et al. (2020); Maggio et al. (2019); Fos et al. (2019) and Cespedes et al. (2020). Rothstein and Rouse (2011) find that student debt affects students' academic decisions, causes graduates to choose higher-salary jobs at the cost of taking fewer lower-paid "public interest" jobs. Sharing negative information about households' past credit market behavior has also been shown to reduce employment and mobility (Bos et al., 2018).

¹¹Bednarzik et al. (2017); Meekes and Hassink (2019), and Fontaine et al. (2020) document a correlation between household balance sheets and labor market outcomes.

economic conditions in Norway, Section 3 describes the data and variables constructed, Section 4 explains the empirical strategy, Section 5 presents the impact of 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 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, both of 2005. The Working Environment Act sets standards for working conditions and process 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.

Unemployment benefit coverage in Norway approximately equals the OECD average of 60 percent. 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) in 2010. Unemployment insurance can be obtained for up to 2 years, depending on workers’ previous earnings. Neither the coverage ratio nor the duration changed during our sample

period.¹²

Norwegian economy and macroprudential policy framework Norway’s economy has displayed stable economic growth, with inflation and average unemployment below four 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).¹³ Due to the steep rise in house prices and household indebtedness and the resulting financial stability risks, Norwegian policymakers implemented a series 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 a unique, anonymized, personal identifier. 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. 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 with data from the population register and official tax records. The population register includes background variables such as gender, 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,

¹²Workers 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. Only a small number of workers in our sample have longer unemployment duration than 2 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.

¹³Figure A1 also illustrates other macroeconomics conditions in Norway.

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 both the buyers’ identifier, 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, we use the household as the unit of observation for variables that matter for the policy, e.g., household leverage. We also measure deposits, income, and interest payments at the household 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.

Due to Norway’s lack of a credit registry, we need to compute LTV ratios from official tax register data. 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 can therefore observe bank credit but not disentangle mortgages from other loans. The Mapping Authority’s register identifies all people who own (part of a) home. To avoid the inclusion of legacy debt in the calculation of LTV ratios, we define mortgage credit as the increase in the households’ total debt in the year of the home purchase. We divide the imputed mortgage debt by the house transaction value observed in the Mapping Authority’s housing transaction register. As a consequence, we may overestimate the LTV ratio if a household takes an unsecured loan or increases its utilization of an existing line of credit in the year of the home purchase. On average, households have an LTV ratio of 92 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 4.24 in our sample, with a standard deviation of 2.10.

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 to allow for labor market exit and limit the role of outliers.¹⁴ 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 132 days.

¹⁴We follow Davis et al. (1998) and compute the symmetric growth rates as

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

Our main sample period starts in 2006 and ends in 2013.¹⁵ To have a better identification, we limit our analysis to workers who bought a home and subsequently lost their jobs due to a mass layoff as explained in Section 4. Applying this filter yields 1880 workers who are displaced from 564 different firms. Of the workers in our regression sample, 15 percent reside in Oslo, close to 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.

Table 1: **Summary statistics**

This table provides summary statistics of the main variables for the period 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. $d(L\hat{T}V > 0.85)$ (treated) households are the ones whose predicted LTV ratios are larger than then the LTV threshold value. First two column use the overall sample. Second two columns use the control group. Third two columns use the treated group.

	Mean	Std. Dev.	25 th pctl	50 th pctl	75 th pctl
Loan-to-Value	0.92	0.22	0.77	0.96	1.06
Debt-to-Income	4.24	2.10	2.71	3.85	5.60
House Price (tho. NOK)	1956.41	1252.89	1200.00	1700.00	2450.00
Mortgage (tho. NOK)	1721.47	1008.26	1024.50	1507.00	2091.00
Interest Expense (tho. NOK)	91.46	70.44	44.44	73.53	119.29
Deposits (tho. NOK)	222.01	363.84	41.80	115.43	257.69
Income (tho. NOK)	706.64	710.71	392.45	591.83	875.10
Wage Growth Rate	-0.07	0.69	-0.23	-0.08	0.14
Unemployment Spell (days)	132.92	319.36	32.00	52.00	122.00
ln(Spell)	2.27	2.55	1.50	1.72	4.80
$\Delta \ln(\text{Ex} - \text{Post Debt})$	0.09	0.98	-0.05	0.00	0.07
$\Delta \ln(\text{Firm Wage Premium})$	-0.29	0.03	-0.30	-0.29	-0.27
Different Occupation	0.76	0.42	1.00	1.00	1.00
Different Industry	0.65	0.48	0.00	1.00	1.00
Different Job Location	0.45	0.50	0.00	0.00	1.00
Δ Education	0.06	0.24	0.00	0.00	0.00
Observations	1880				

¹⁵The reason why the sample period ends in 2013 is that a change in the enforcement of data reporting standards by multi-plant firms generates noise in the data, reducing the accuracy of the identification of displaced workers. However, we are able to observe workers’ labor market outcomes, such as wages and unemployment spell, until 2019.

4 Empirical strategy

Our objective is to estimate the effect of the macroprudential borrowing restriction through leverage on job search and subsequent wages. Reaching our objective entails an empirical strategy that has 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. Therefore, the empirical strategy should be able to identify the exogenous change in household leverage. Second, the empirical strategy should enable us to observe job search behavior clean from individual characteristics, since such characteristics are likely to 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.

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. Due to low compliance with the initial guideline, the FSA issued an update to reduce ambiguities and set a precise implementation period in December 2011.¹⁶ The updated 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.¹⁷ In our main regressions, we therefore remove all observations between the two guidelines and start the post-treatment period in 2012. 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.

One important feature of this LTV restriction 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 treats every homebuyer.¹⁸ Before

¹⁶The 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](#))."

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

¹⁸There 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

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 even though the LTV restriction were not implemented. Such workers who endogenously prefer to have low LTV ratios are natural candidates for the control group. Yet, we do not have a variable that enables us to separate these workers from the treated ones.

The common solution the literature applies to similar cases where the treatment status is missing is proxying the treatment status with one 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., 2019](#); [Aastveit et al., 2020](#)). We take a step forward and 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 main advantages. The first advantage is that by using many variables instead of a single variable, RF improves the accuracy of the treatment classification. This advantage is expected since a rich set of variables has more information compared to 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. 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 forecast power. This is crucial 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, RF alleviates the concerns regarding overfitting and provides a more robust classification performance for the post-treatment period.

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, wage, deposits, DTI, business income, education, age, location, immigration status, and information about the 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

leverage and are thus untreated, we remove these observations from our estimation sample. The placebo test in [Section A4](#) shows that this removal does not affect our results.

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

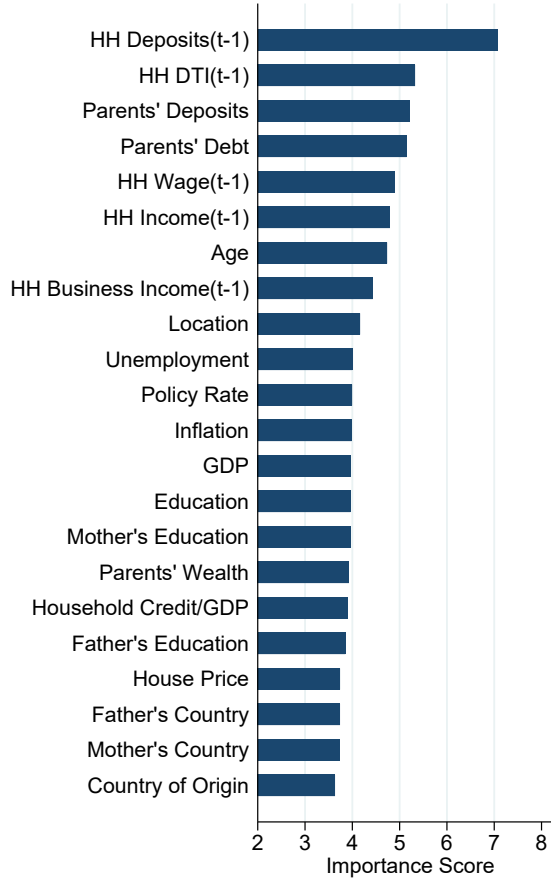
Thanks to the ample availability of household-level and worker-level data, the classification power of the RF is high. A common way to assess the performance of a binary classifier is by plotting its Receiver Operating Characteristics (ROC) curve and calculating the Area Under the Curve (AUC).¹⁹ In the literature, AUC values of 0.9 and higher are considered excellent (Hosmer Jr 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. 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.

¹⁹A 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 values of AUC 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 to the model with levels and changes.



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 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.

Mass layoffs The second part of our empirical strategy considers the possible influence of LTV restriction on job search initiation and its interaction with individual characteristics. The main concern is that LTV restriction can alter the characteristics of job-seeking workers. For instance, higher down payment requirement due to the LTV restriction may encourage workers to look for better-paying jobs. Alternatively, risk-averse workers may be less likely

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. 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 _{t-1}	1120.76	710.29	410.47	8.67
Wage _{t-1}	1065.95	687.38	378.57	8.31
Debt-to-Income _{t-1}	2.58	1.54	1.04	4.20
Deposits _{t-1}	869.19	156.09	713.10	28.61
Business Inc. _{t-1}	54.81	22.91	31.90	2.05
Parents' Debt _{t-1}	1898.84	1987.59	-88.75	-0.46
Parents' Dep. _{t-1}	1458.99	600.92	858.06	10.18
Parents' Wealth _{t-1}	1508.78	529.30	979.48	4.82
Age	36.09	32.39	3.70	5.58
Immigrant	0.18	0.20	-0.02	-0.90
Immigrant ^{Mot}	0.21	0.24	-0.03	-0.94
Immigrant ^{Fat}	0.29	0.30	-0.01	-0.27
College	0.73	0.39	0.34	10.68
College ^{Mot}	0.26	0.17	0.09	3.63
College ^{Fat}	0.33	0.18	0.15	5.66
Observations	1880			

to start a job search since a new job can be riskier, and losing a job may lower the ability to make enough savings for the down payment. These arguments indicate that job search not generated by individual-level decisions is essential to cleanly identify the policy's effect. Therefore, we use only displaced workers who lost their job 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 (Flaen et al., 2019). We define a mass layoff as a situation where a firm parts with at least 30 percent of its workers in a year or ceases its operation entirely. We follow the literature (e.g. Lachowska et al. (2020)) and use only firms with at least 50 employees to limit the risk that laying off small numbers of workers for idiosyncratic reasons is wrongfully treated as a mass layoff. In addition, we restrict displaced workers to the ones who bought their homes up to 12 months before losing their jobs. This 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 job loss.

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

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

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 1 after implementation of the policy, and $d(\widehat{LTV} > 0.85)_h$ is a dummy variable that takes the value of 1 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., 2017). Moreover, we use Murphy-Topel standards errors as we use predicted regressors (Murphy and Topel, 1985).

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 the treatment. A confirmation that the trends are parallel would provide strong support for the assumption that, absent treatment, 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 :

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

We omit $period = -1$ in Equation 3; the estimated γ_k coefficients therefore document the difference between treated and control groups at $period = k$ relative to that at $period = -1$.

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. Lewbel (2007) shows that misclassification of a binary treatment regressor creates an attenuation bias akin to standard measurement error bias. This implies that our parameter estimates would provide a *lower* bound for the effect of household leverage on labor market outcomes should misclassification be an issue. The high out-of-sample predictive power of our RF model mitigates such concerns about the risk of

misclassification, however. Moreover, as [Figure A4](#) shows, most of the misclassified workers in the pre-treatment period are clustered narrowly around the 85 percent LTV threshold. For workers whose LTV ratios are close to this policy threshold, the impact of the LTV restriction on household leverage will be smaller because the restriction forces such households to limit leverage only by a small amount. This suggests that the magnitude of such an attenuation bias, if any, will also be small.

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.

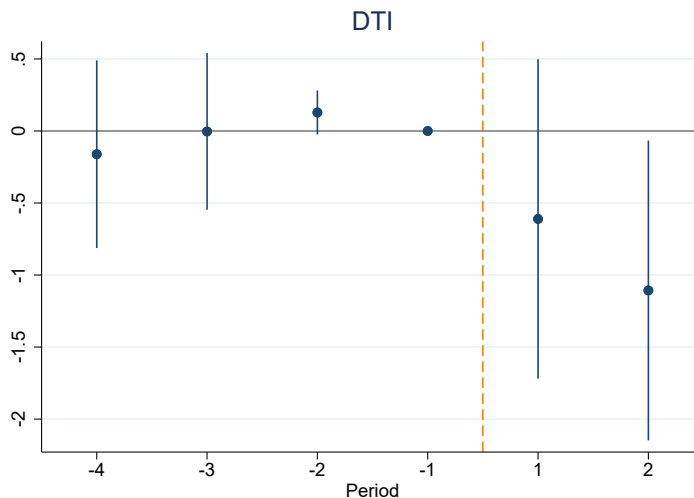
5.1 Impact of borrowing restriction on household leverage

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](#) depicts 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 a 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).

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 between 2006 and 2013, 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. Dependent variable is DTI ratio calculated from tax filings and is the ratio of total debt to total income. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. Figure shows the β s on the y-axis of the regression model, $DTI_{ht} = \sum_{k=-4}^2 \gamma_k D_k \times d(L\hat{T}V > 0.85)_h + d(L\hat{T}V > 0.85)_h + \epsilon_{ht}$. 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.



A potential concern about our model specification could be that mass layoffs may not occur randomly but occur 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 that 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.

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 between 2006 and 2013, 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 then the LTV threshold value. $Post$ is equal to 1 for the years 2012 and 2013 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)
$d(\widehat{LTV} > 0.85) \times Post$	-1.094*** (0.372)	-1.058*** (0.348)	-1.138*** (0.394)	-1.108*** (0.358)	-1.148*** (0.353)	-1.017** (0.401)
$d(\widehat{LTV} > 0.85)$	0.895*** (0.284)	0.858*** (0.256)	1.192*** (0.304)	1.206*** (0.268)	1.188*** (0.234)	1.193*** (0.250)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location \times Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R ²	0.023	0.029	0.163	0.187	0.211	0.265
Mean($\frac{Debt}{Income}$)	4.241					

After introduction of the policy, treated households take on mortgages that are on average NOK 603,000 smaller and to pay for homes that are NOK 503,000 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.

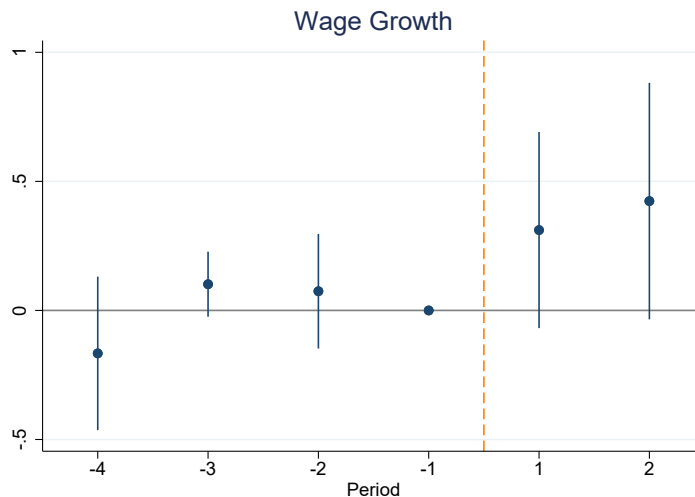
5.2 Impact of borrowing restriction on wages

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 policy can affect wages through opposing channels. On the one hand, the leverage restriction can curtail debt overhang—a channel through which leverage lowers displaced workers' appetite to work since a larger fraction of earnings will go to their lenders (Donaldson et al., 2019). Therefore, workers whose leverage is lower due to the restriction suffer less from debt overhang and demand lower wages, predicting an adverse effect of the restriction on wages.

On the other hand, the leverage restriction can increase the wages of displaced workers. Household leverage can, for instance, create pressure on workers because they need to service their debt since (mortgage) loan defaults are associated with substantial costs, limited access to credit markets, worsened labor market prospects, forced moves, and even elevated divorce rates, particularly among marginal homeowners (Ji, 2021).²⁰ Alternatively, leverage can influence job search broadness. By increasing consumption commitments, leverage can induce higher risk-version, which may 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 between 2006 and 2013, 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. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. Figure shows the β s on the y-axis of the regression model $wage\ growth_{ht} = \sum_{k=-4}^2 \gamma_k D_k \times d(L\hat{T}V > 0.85)_h + d(L\hat{T}V > 0.85)_h + \epsilon_{ht}$. 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.



We first estimate the dynamic effect of the policy on wages to investigate which of these channels empirically dominates. Thereby, Figure 3 depicts γ_k from Equation 3, where the

²⁰Dobbie 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); Maggio et al. (2019) document the impact on labor market prospects and Diamond et al. (2020) find non-pecuniary costs of foreclosures on forced moves and divorce rates.

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 follow parallel trends. However, after the introduction of the borrowing constraint, treated workers experience higher wage growth after being displaced, indicating that reducing leverage improves displaced workers’ wage prospects.

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 therefore further saturate the model with location and industry fixed effects in Columns (4) and (5).

An ideal comparison would be between two workers who are displaced from the same firm. However, in our sample, there are no firms with a mass layoff in both the pre- and post-treatment period. As a consequence, $d(\widehat{LTV} > 0.85)_h \times Post_t$ would not be identified if we were to include firm fixed effects. The closest we can get to this is to saturate the model with *Location* \times *Industry* fixed effects, as in Column (6). In this tight specification, $d(\widehat{LTV} > 0.85)_h \times Post_t$ still has a positive and statistically significant coefficient. The economic meaning of the positive coefficients can be interpreted by considering the average change in wages in our sample. In line with literature, displaced workers in our sample experience a 7.6pp reduction in their wages in their new jobs. Therefore, the estimate of 44.9 percent higher growth rate in Column (5), our preferred specification, indicates that treated workers achieve a 3.3 percentage points smaller reduction in their new wages.

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 that 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 3.3 percentage point improvement in wages could be partly driven by changes in characteristics of the treated group induced by the policy itself. Several analysis

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 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. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. *Treated* takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value ($d(\widehat{LTV} > 0.85)$). *Post* equals 1 for the years 2012 and 2013 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					
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.335** (0.154)	0.343** (0.153)	0.482*** (0.161)	0.495*** (0.158)	0.449** (0.160)	0.390* (0.187)
$d(\widehat{LTV} > 0.85)$	-0.102*** (0.010)	-0.109*** (0.027)	-0.129*** (0.033)	-0.125*** (0.036)	-0.123*** (0.031)	-0.120*** (0.028)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location \times Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R ²	0.008	0.014	0.091	0.107	0.121	0.183
Mean(Wage Growth)	-0.074					

suggest 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 the 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.²¹ A plot of the homeownership transition rate in Figure A10 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 if the borrowing restriction alters the observable characteristics of the treated workers in our sample using a similar strategy as Bernstein and Koudijs (2021). To this end, we rerun the difference-in-differences model in Equation 2 but use log changes 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

²¹Official 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 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. 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) removes households who do not have enough deposits for 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 then the LTV threshold value. $Post$ equals 1 for the years 2012 and 2013 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.

<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)
$d(\widehat{LTV} > 0.85) \times Post$	0.042 (0.191)	0.061 (0.195)	0.183 (0.141)	-0.311 (0.426)	-0.043 (0.243)	0.031 (0.071)	0.289* (0.156)	0.373* (0.213)
$d(\widehat{LTV} > 0.85)$	0.064 (0.055)	0.060 (0.055)	-0.050 (0.055)	0.022 (0.085)	0.105** (0.047)	0.004 (0.019)	-0.055* (0.031)	-0.056 (0.048)
<i>Fixed Effects:</i>								
Year FE	✓	✓	✓	✓	✓	✓		✓
Education FE	✓	✓	✓	✓	✓			✓
Location FE	✓	✓	✓	✓	✓	✓		✓
Industry FE	✓	✓	✓	✓	✓	✓		✓
Obs.	1,833	1,833	1,833	1,833	1,833	1,876	941	927
R ²	0.110	0.109	0.080	0.120	0.093	0.083	0.014	0.181
Mean(Dependent Var.)	0.361	0.369	0.092	0.333	0.050	0.777	-0.074	

or economically significant effect on these variables, which indicates that the restriction does not change the observable characteristics of treated workers. Columns (1) and (2) also mitigate another potential concern, namely that workers might try to increase their earnings when down payment requirements increase. This could have generated momentum that helps displaced workers in the job search process. 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 does not influence same-employer income growth, we can effectively eliminate this concern.

Third, we homogenize the regression sample regarding 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 enables us to remove such workers since we observe both the home transaction values and bank deposit holdings. 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 both considering wider samples and labor market 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 influencing the effect of policy on household leverage, helping us establish causality. Even though this sample is highly relevant for policy discussions²², it does not represent the general population. In [Table 6](#), we relax our sample criteria and assess the relationship between leverage and wages in wider samples.²³ In [Column \(1\)](#), we use the whole 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 unemployed workers who could be unemployed voluntarily or involuntarily. The coefficient is again negative but increases in magnitude. This increase may reflect the fact that wages are sticky due to wage contracts. Therefore, 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

²²See [Ganong and Noel \(2020\)](#) for the importance of negative labor shocks and high leverage on mortgage defaults.

²³In 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.

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

This table provides evidence for 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 homeownership 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) restricts the sample by removing workers whose unemployment spell is longer than 500 days. Column (6) restricts the sample by removing workers whose unemployment spell is longer than 2 years. Column (7) 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 1 for the years 2012 and 2013 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	Full	Unemployed	Displaced	$\leq 4y$	Spell		Tenure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
					< 500days	< 2 years	< 5 years
$\ln(\text{debt})_{t-1}$	-0.026*** (0.0001)	-0.052*** (0.0003)	-0.019*** (0.001)				
$d(\widehat{LTV} > 0.85) \times \text{Post}$				0.415*** (0.228)	0.539*** (0.172)	0.494** (0.178)	0.362* (0.189)
$d(\widehat{LTV} > 0.85)$				-0.125*** (0.0178)	-0.129*** (0.0374)	-0.131*** (0.0374)	-0.152*** (0.0509)
<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	8,361	1,700	1,756	1,453
R ²	0.360	0.376	0.116	0.015	0.132	0.124	0.133

up to 4 years before losing their jobs. Note that, in this sample, the leverage is not exogenous because workers may pay their mortgage at different speeds, making the leverage at the 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 for establishing causality.

Next, we consider the labor market policies in Norway. Section 2 points out that the unemployment insurance generosity in Norway is mainly driven by its duration, which is two years, as the replacement rate is close to the OECD median. The long unemployment insurance duration may reduce our results' external validity if the workers exhaust unemployment insurance fully. We assess this possibility in Table 6. In Columns (5) and (6), 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 shorter than 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 2 months from 1 month, which can allow the worker to adjust her job search before the job contract ends. In Column (7) 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 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, gender, immigration status, and wage growth once the age is controlled for. Overall, our findings fail to suggest a threat to the external validity of our results.

Additional robustness checks In [Table 8](#), 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 had 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. We therefore 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.²⁴ 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 periods.²⁵ In line with the parallel trends in [Figure 3](#), the placebo effect has no effect on

²⁴[Table A6](#) shows that none of the interaction terms is significant.

²⁵To better mimic the composition of the main sample, we remove workers with LTV ratios above 85 percent from the placebo’s post sample. In [Section A4](#), we use a simulation exercise and show that this

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

This table compares the workers used in the regression sample to other workers in their previous firms. Each column uses individual level data between 2006 and 2013, where observations between first and second policy implementation are excluded. 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	-4.759***	0.0231	0.0227	0.0125	0.023	0.0183
	(0.591)	(0.0144)	(0.0165)	(0.0194)	(0.008)	(0.0273)
<i>Fixed Effects:</i>						
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Age FE		✓	✓	✓	✓	✓
Obs.	200,411	200,411	200,411	200,411	200,411	200,411
R ²	0.247	0.186	0.265	0.139	0.104	0.106
Mean(Dependent Var.)	38.471	0.763	0.386	0.424	0.185	0.205

wages, supporting our main identifying assumption.

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 LTVs. 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 observed wage growth difference between treated and control workers is not driven by the threshold for inclusion in the regression sample.

removal does not create a bias in our estimations.

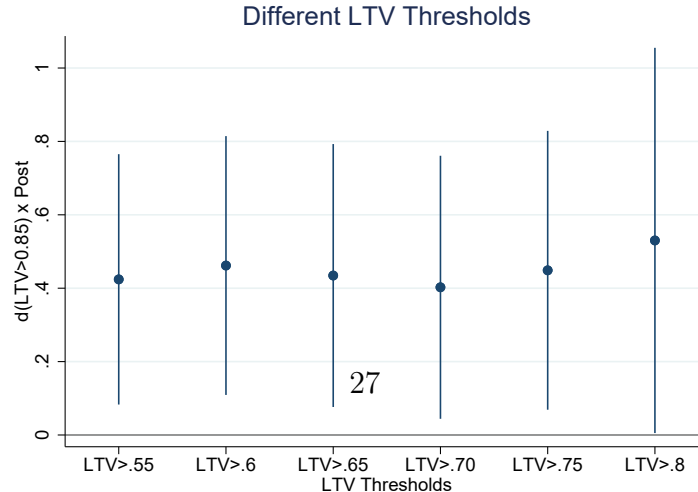
Table 8: Robustness checks for wage growth

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 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. 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 then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 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						
	(1) 2005	(2) 2007	(3) No Transf.	(4) No Bus. Inc.	(5) Macro	(6) Education	(7) Placebo
$d(\widehat{LTV} > 0.85) \times Post$	0.426** (0.183)	0.449** (0.186)	0.409** (0.180)	0.430** (0.183)	0.983*** (0.329)	0.423* (0.205)	
$d(\widehat{LTV} > 0.85) \times Placebo$							-0.039 (0.131)
$d(\widehat{LTV} > 0.85)$	-0.108** (0.040)	-0.096*** (0.033)	-0.088** (0.038)	-0.126*** (0.037)	-5.076 (3.510)	0.703*** (0.184)	0.027 (0.117)
<i>Fixed Effects:</i>							
Year FE	✓	✓	✓	✓	✓	✓	✓
Education FE	✓	✓	✓	✓	✓	✓	✓
Location FE	✓	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓	✓
Treated \times Macro Var.					✓		
Treated \times Education FE						✓	
Obs.	2,016	1,614	1,649	1,737	1,833	1,833	1,029
R ²	0.124	0.124	0.138	0.122	0.124	0.171	0.169
Mean(Wage Growth)	-0.074						

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 between 2006 and 2013, 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. 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 then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 0 for earlier years. x-axis indicates the value of the lower bound for the LTV ratio to be included in the estimation sample. y-axis shows β 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 95% confidence intervals.



5.3 Through what mechanism does leverage affect wages?

Having established the presence of a robust positive effect of 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, and employer and occupation characteristics. Our findings indicate that the leverage restriction relaxes the constraints that leverage put on job search, enabling workers to have improved job search. We provide several further heterogeneity tests that support the mechanism that we reveal.

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 during the unemployment spell. Columns (1)-(2) and (5)-(6) use individual level and Columns (3) and (4) use 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. Columns (1) and (2) use $\ln(\text{Unemployment Spell})$ as the dependent variable. Columns (3) and (4) 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 (5) and (6) 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). $d(\widehat{LTV} > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 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.

	ln(Unemp. Spell)		$\Delta \ln(\text{Ex-Post Debt})$		$\Delta \ln(\text{Firm Wage Pre.})$	
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.608*** (0.205)	0.567* (0.281)	-0.067 (0.244)	-0.114 (0.313)	0.004 (0.023)	0.058** (0.027)
$d(\widehat{LTV} > 0.85)$	0.019 (0.091)	0.017 (0.110)	-0.023 (0.024)	-0.063 (0.057)	0.029*** (0.007)	0.009 (0.008)
<i>Fixed Effects:</i>						
Year FE		✓		✓		✓
Education FE		✓		✓		✓
Location FE		✓		✓		✓
Industry FE		✓		✓		✓
Obs.	1,876	1,833	1,876	1,833	1,672	1,637
R ²	0.006	0.160	0.000	0.096	0.002	0.386
Mean(Dependent Var.)	2.270		0.085		-0.286	

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. Such workers may therefore become more willing to accept early but potentially worse-paying job offers to attenuate default risk and associated costs. We test the hypothesis that the restriction can lengthen the 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](#) show that treated workers with lower leverage have 60 percent longer unemployment spells, the equivalent of a 79 days 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 fixed effects does not change our results, although we obtain slightly larger standard errors.

A channel through which household leverage could affect job search behavior is its influence on credit utilization during unemployment. [Herkenhoff et al. \(2016\)](#) and [Herkenhoff \(2019\)](#) 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 the household balance sheet information. Columns (3) and (4) of [Table 9](#) demonstrate that the treated workers do not increase credit utilization during periods of unemployment, suggesting that the drivers of the job search and labor market effects that we identify differ from those in [Herkenhoff et al. \(2016\)](#) and [Herkenhoff \(2019\)](#).

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, i.e., the average wage firms pay after controlling for employee characteristics, for all firms in our sample.²⁶ 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 (5) and (6) 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 20 percent of the increase in workers' wage growth is driven by their finding jobs in higher-paying firms.

²⁶Specifically, 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 broadness**

This table documents the effect of the LTV ratio policy on the job search breadth of displaced workers. Each column uses worker level data between 2006 and 2013. 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 in Columns (1) and (2) is a dummy variable, which takes the value of 1 if worker changes her occupation in her new employer. Dependent variable in Columns (3) and (4) is a dummy variable, which takes the value of 1 if worker changes the industry in her new employer. Dependent variable in Columns (5) and (6) is a dummy variable, which takes the value of 1 if 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 then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 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.

	Diff. Occupation		Diff. Industry		Diff. Job Location	
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.202**	0.293***	0.155*	0.233**	0.066	0.024
	(0.088)	(0.097)	(0.082)	(0.105)	(0.132)	(0.157)
$d(\widehat{LTV} > 0.85)$	0.032	0.012	0.038	0.020	0.067	0.065
	(0.025)	(0.025)	(0.024)	(0.023)	(0.043)	(0.044)
<i>Fixed Effects:</i>						
Year FE		✓		✓		✓
Education FE		✓		✓		✓
Location FE		✓		✓		✓
Industry FE		✓		✓		✓
Obs.	1,876	1,833	1,876	1,833	1,876	1,833
R ²	0.009	0.183	0.005	0.222	0.005	0.142
Mean(Different Job)	0.764		0.650		0.448	

Another mechanism that the leverage restriction influences job search is job search broadness. The restriction can enable workers to broaden their job 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: doing a different occupation and finding a new job in a different industry or 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 20 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 15 percent higher probability of finding their new jobs in another industry than they worked in before. Geographical labor mobility, an important determinant of labor supply, has been shown to be adversely affected when households have negative home equity. As shown in Columns (5) and (6), the leverage restriction does not influence geographic labor mobility. This complements the findings of Bernstein (2020) and Gopalan et al. (2020).

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 makes it possible for 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 improved matching. To examine this, we re-run the wage growth regressions on sub-samples using three criteria.

The first criterion is age. We expect younger people to respond stronger to a less-constrained job search since it is likely 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 improvement in wage is stronger for younger workers. Next, we split up workers by the duration of 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. Workers with job tenure below the median have wage growths higher than our main finding, while the others have 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 high education to benefit. [Eriksson and Rooth \(2014\)](#) have also 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 lower education workers. Columns (5) and (6) show wage growth rises more for the higher educated, in line with our expectations.

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 Bakkum et al., 2019](#)). 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-

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 between 2006 and 2013. 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. 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 25th percentile. Column (8) uses workers whose income are between sample's 25th and 50th percentile. Column (9) uses workers whose income are higher than sample's 75th percentile. $d(\widehat{LTV} > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 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	Age		Tenure		Education		Income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Low	High	Low	High	Low	High	Low	Medium	High
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.700*** (0.210)	0.126 (0.277)	0.609** (0.227)	0.433 (0.423)	0.101 (0.260)	0.402** (0.173)	0.833* (0.475)	0.268 (0.264)	0.193 (0.244)
$d(\widehat{LTV} > 0.85)$	-0.195** (0.069)	-0.024 (0.049)	-0.160** (0.072)	-0.054 (0.040)	-0.161*** (0.036)	-0.026 (0.030)	-0.209*** (0.061)	-0.102* (0.052)	-0.044 (0.058)
<i>Fixed Effects:</i>									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Education FE	✓	✓	✓	✓			✓	✓	✓
Location FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Obs.	1,044	789	866	967	419	882	432	911	490
R ²	0.170	0.219	0.159	0.195	0.096	0.062	0.312	0.176	0.261
Mean(Wage Growth)	-0.074								

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 28 percent. This number indicates a 4.7 percentage points improvement in the annual wages during this 4-year period. The magnitude of the effect is robust to

saturation of the model with year, education, location, industry, and location \times industry fixed effects. Together, these findings establish that the increase in wage growth is robust and not short-lived.

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 between 2006 and 2017, 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 4-year wage growth as the dependent variable. Columns (3)-(4) use 4-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 the LTV threshold value. $Post$ equals 1 for the years 2012 and 2013 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)
$d(\widehat{LTV} > 0.85) \times Post$	0.257*** (0.061)	0.182** (0.080)	-26.274*** (5.917)	-24.719* (12.988)
$d(\widehat{LTV} > 0.85)$	0.032 (0.025)	0.012 (0.025)	1.033 (3.270)	5.183* (2.635)
<i>Fixed Effects:</i>				
Year FE		✓		✓
Education FE		✓		✓
Location FE		✓		✓
Industry FE		✓		✓
Obs.	1,856	1,815	1,856	1,815
R ²	0.010	0.115	0.008	0.178
Mean(Dep. Var.)	0.182		82.757	

Finally, we consider the treated workers' wage volatility. 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 in these new occupations or industries. If, on the other hand, matching quality improves thanks to less-constrained job search, then we expect to observe that treated workers have lower wage volatility after the restriction. To test how the 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 have lower wage volatility, suggesting that, in addition to improving

wages, the policy makes the 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 who have lower leverage due to the restriction have 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 identified rise in wage growth persists over time, implying that displaced workers do not achieve higher wages by accepting riskier jobs.

By revealing these previously unnoticed effects on labor market outcomes, we believe our results are important for at least two main reasons. First, while borrowing restriction policies may adversely influence access to housing markets, they have unintended positive effects on labor markets. Therefore, our results indicate that the discussions about the welfare implications of borrowing restriction policies should also incorporate their effect on labor markets. Second, our results illustrate one additional adverse effect of household leverage on the economy. By restricting job search, household leverage impairs the match between the workers and firms. Thus, besides financial stability, high household leverage worsens the functioning of labor markets in a recession, suggesting that policy discussions about recessions with high household leverage should also consider the negative effects on job search.

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Online Appendix

A1 Additional information on data

This section provides additional information about data sets we use in our paper. We use the Norwegian Standard Classification of Education at the 3-digit level to measure education. Our education variable captures both the level and the broad field of education. The level indicates if a person has 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 3-digit detail we can determine if a person with a social sciences and law background studies sociology or psychology. To capture changes in the profession we use Statistics Norway’s seven-digit occupational information that builds on the EU’s 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 A6](#) shows γ_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 A6](#). Column (1) of [Table A1](#) contains the parameter estimates from a regression without any controls. The estimated treatment effect is highly significant and negative treatment effect. The 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 that treated households had a 23 percentage points higher LTV ratios 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 Bakkum et al. \(2019\)](#) 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 examine how the macroprudential policy achieves this debt-reducing effect. We therefore inspect how mortgage size, the price of purchased homes, and deposits change and again start by considering the year-by-year effects in [Figure A7](#). The visual evidence again supports the presence of parallel trends, for all three variables. We re-confirm the finding in the literature ([Tzur-Ilan, 2020](#); [Van Bakkum et al., 2019](#); [Aastveit et al., 2020](#)) that LTV constraints reduce mortgage size ([Figure A7a](#)) and the cost of homes treated households buy ([Figure A7b](#)). A tighter borrowing constraint does not reduce treated households' liquidity by draining deposits [Figure A7c](#). [Table A2](#) indicates that treated household take on mortgages that are NOK 603,000 smaller mortgages to pay for homes that are NOK 503,000 cheaper.²⁷ 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 $(\text{Total Debt} - \text{Deposits})/\text{Income}$ ([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](#); [Ganong and Noel, 2020](#); [Gupta and Hansman, 2020](#)). [Figure A8](#) 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 45,000 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

²⁷Households may be borrow less for the renovation of purchased homes, or reduce consumption to finance home related expenditures.

the following variables: income, wage, deposits, debt, unemployment benefits, business income, 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.

ROC curve plot is a popular method to evaluate 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 threshold 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 1 since all negative events are classified as treated. Also, the true positive rate is 1 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 in 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.²⁸ 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 percent decline in model’s accuracy when the variable is removed (i. e. the model’s accuracy is 7 percent lower when 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.

A4 Placebo test

As explained in Section 4, we remove the households that are able to obtain mortgages with LTV ratios above the threshold after the LTV restriction. The reason is that these households do not experience a reduction in their leverage, and they should thus not experience a change in their starting wages. Even though this argument is reasonable, it might introduce bias into our estimations. We are able to observe and remove these households that are not affected by the restriction. However, we cannot observe such households before the LTV restriction. Therefore, this removal can change the composition of the treated group before and after the restriction. In Table 5, we show that the characteristics of the treated group is not affected by the LTV restriction. However, there can still be concerns about how this removal affects our estimations for starting wages.

To mitigate these concerns, we adopt a conservative strategy in our placebo test reported in ???. That is, we remove the households that obtain mortgages with LTV ratio above the threshold from the placebo post period. Doing this effectively removes the households that would not be affected by the policy and obtain a mortgage with an LTV ratio above the threshold. However, the removed part also includes the households whose LTV ratio would be lowered below the threshold. Thus, there can be still concerns about how this removal affects the estimated coefficients.

We further mitigate the remaining concerns with a simulation-alike exercise. The ratio of households whose LTV ratios above the threshold after the policy is 20%. It is reasonable to assume that the such households occur in the pre-treatment period with a similar ratio. To mimic the actual sample in a more refined way, we randomly drop 20% of households whose LTV ratio above the

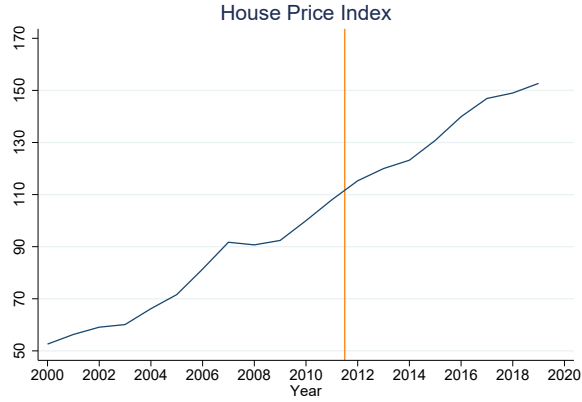
²⁸We plot the variable importance that uses built in function in Figure A9.

threshold from the placebo post period and repeat this for 10,000 times. If this removal creates a bias in the starting wages regressions, then we should observe that a fraction of the estimations in the simulation exercise has positive and significant. On the other hand, finding small and insignificant coefficients in this simulation exercise strongly support that this removal does not create a bias in our results.

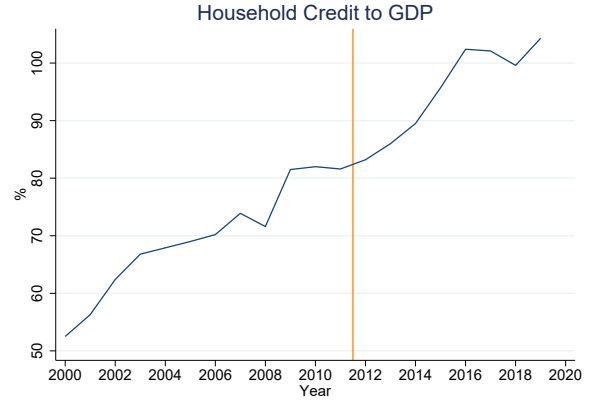
Figure A11 plots the distribution of the coefficients of $d(L\hat{T}V > 0.85) \times Placebo$ from this simulation exercise. First observation is that the coefficients are centered around -0.03, which clearly indicates that this removal does not create a bias. If anything, this exercise suggests a small and negative bias that can attenuate our results. This finding holds for both a difference-in-differences model without any controls (plain model) and a model with year, education, location, and industry fixed effects (saturated model). Second, out of 10,000 estimations, none of them are significant at 10 percent for the plain model and only 4 of them are significant for the saturated model. Thus, we conclude that this removal does not pose a threat for our estimations.

Figure A1: Macroeconomic conditions

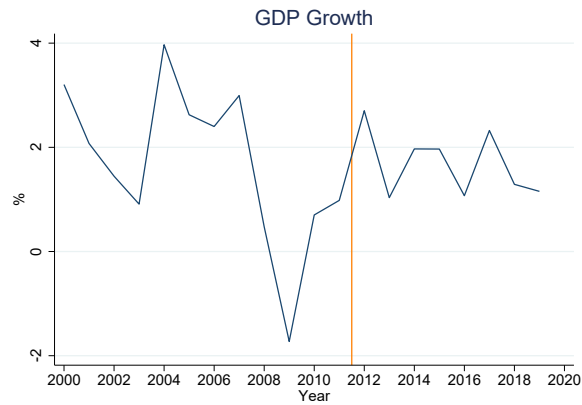
This figure shows the macroeconomic conditions in Norway between 2000 and 2019. [Figure A1a](#) plots house price index, [Figure A1b](#) plots household credit to GDP ratio, [Figure A1c](#) plots GDP growth, [Figure A1d](#) plots unemployment rate, [Figure A1e](#) plots inflation, and [Figure A1f](#)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



(a)



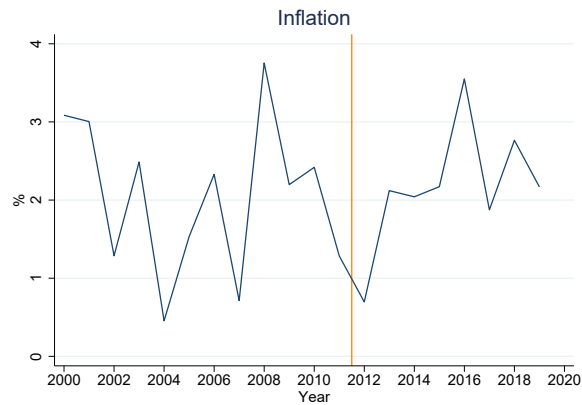
(b)



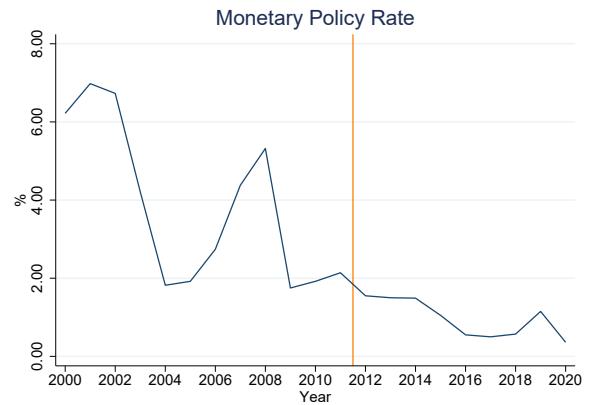
(c)



(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. Orange line shows the false positive rate and true positive rate of a random classifier. 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 summarize the success of the classification model.

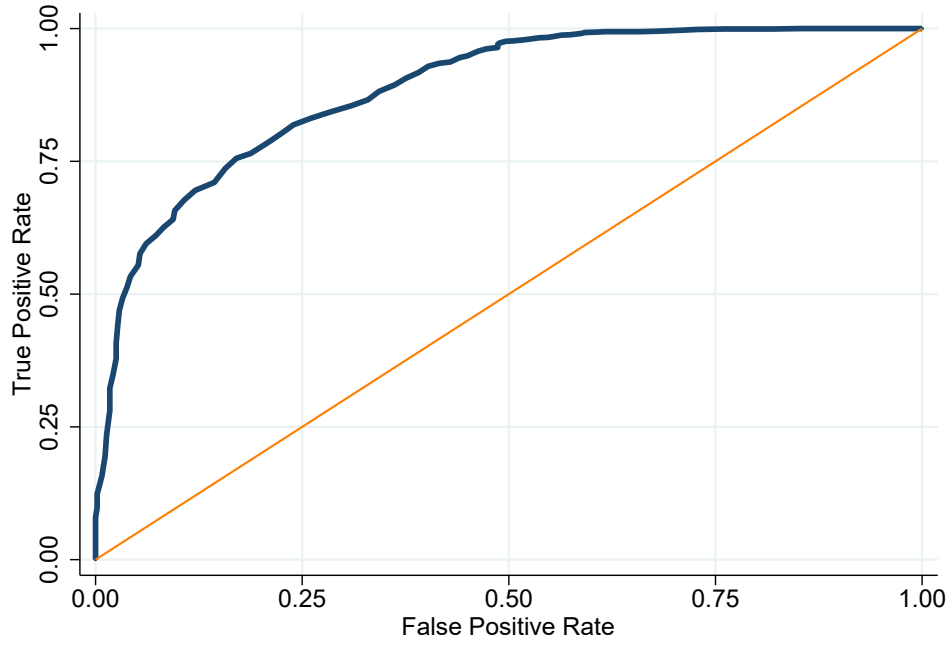


Figure A3: Regional house prices

This figure plots the regional house price growth rates for 9 largest counties. Blue dots show the house price growths rates before the LTV restriction for 4 years. Orange dots show the house price growths rates after the LTV restriction for 2 years.

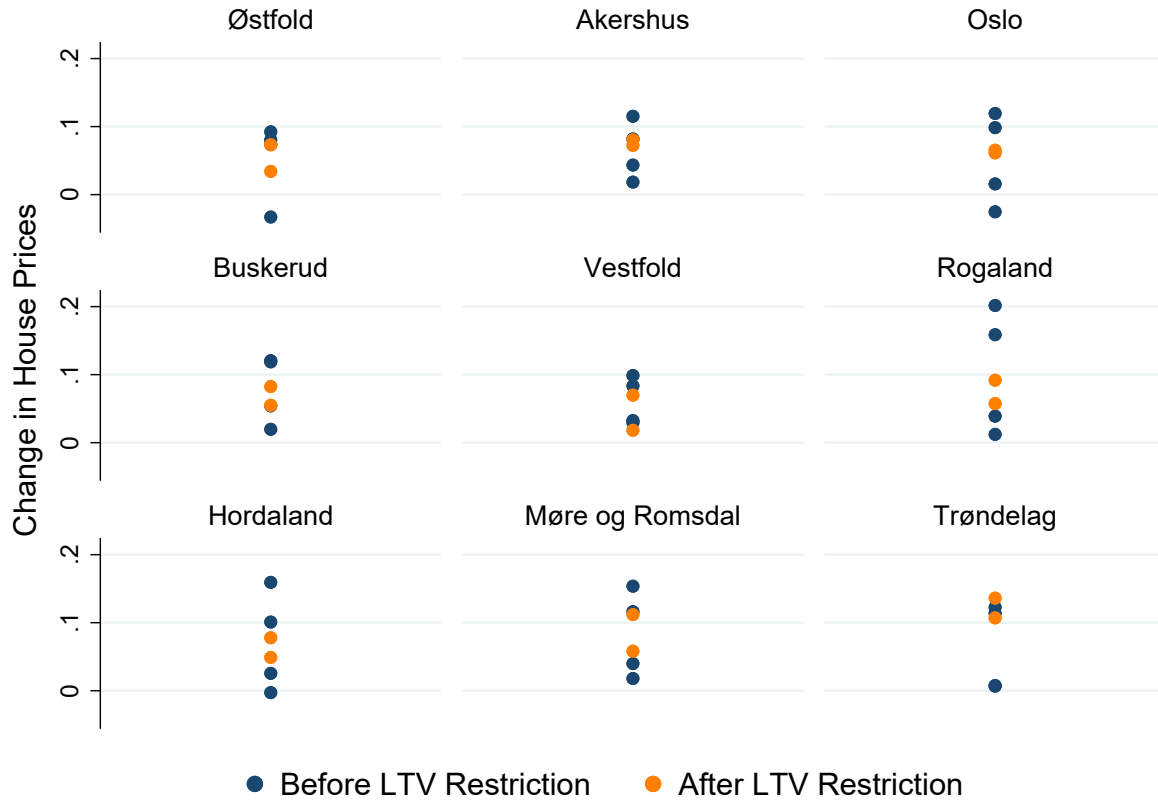


Figure A4: **Classification performance before the LTV ratio restriction**

This figure plots the distribution of correctly and incorrectly classified households with respect to LTV ratios. Plot uses the sample before the LTV ratio restriction in which the correct treatment status is observed. Orange bars indicate the correctly classified households. Blue bars indicate the incorrectly classified households

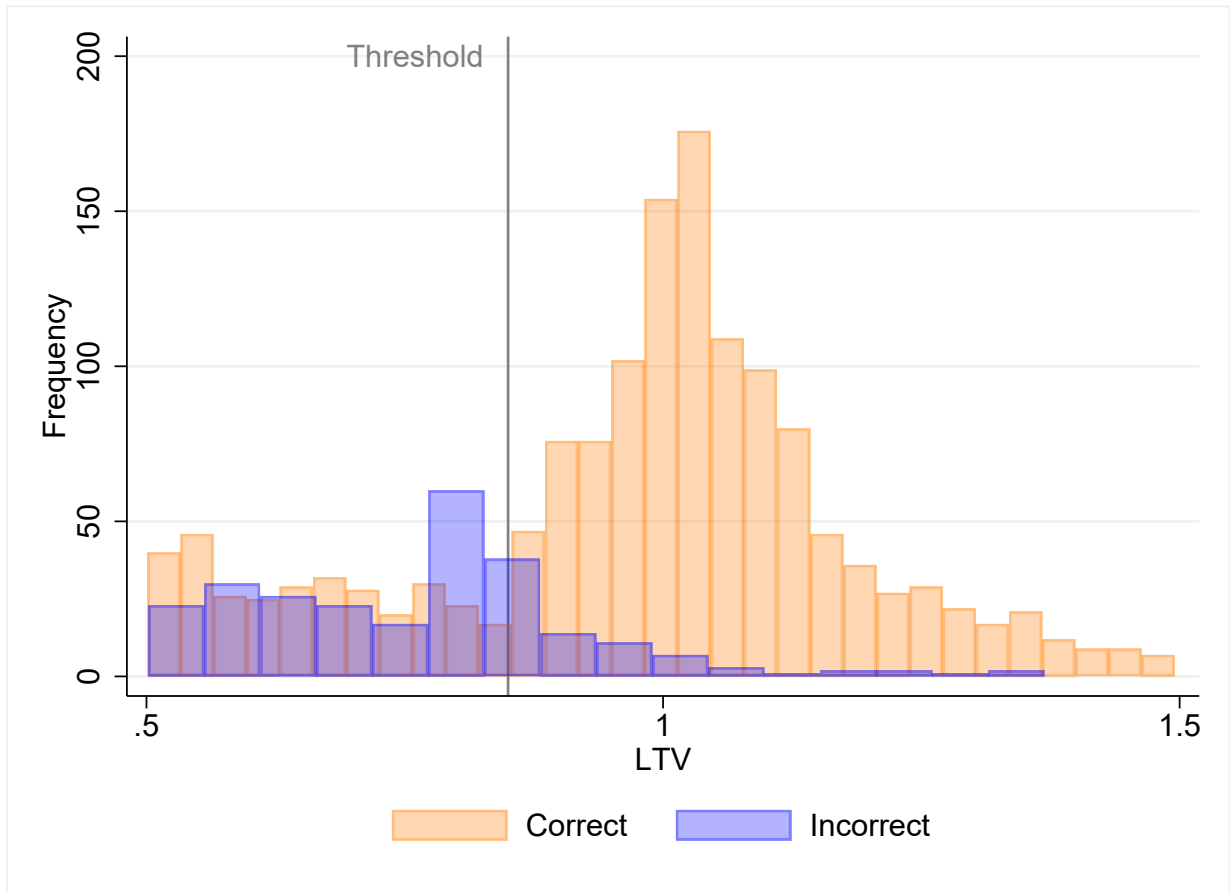


Figure A5: Introduction of the macroprudential policy and house transaction volume

This figure plots the house transaction volume over time. Vertical black dashed line indicates the announcement of the LTV restriction. Vertical orange dashed line indicates the implementation of the LTV restriction.

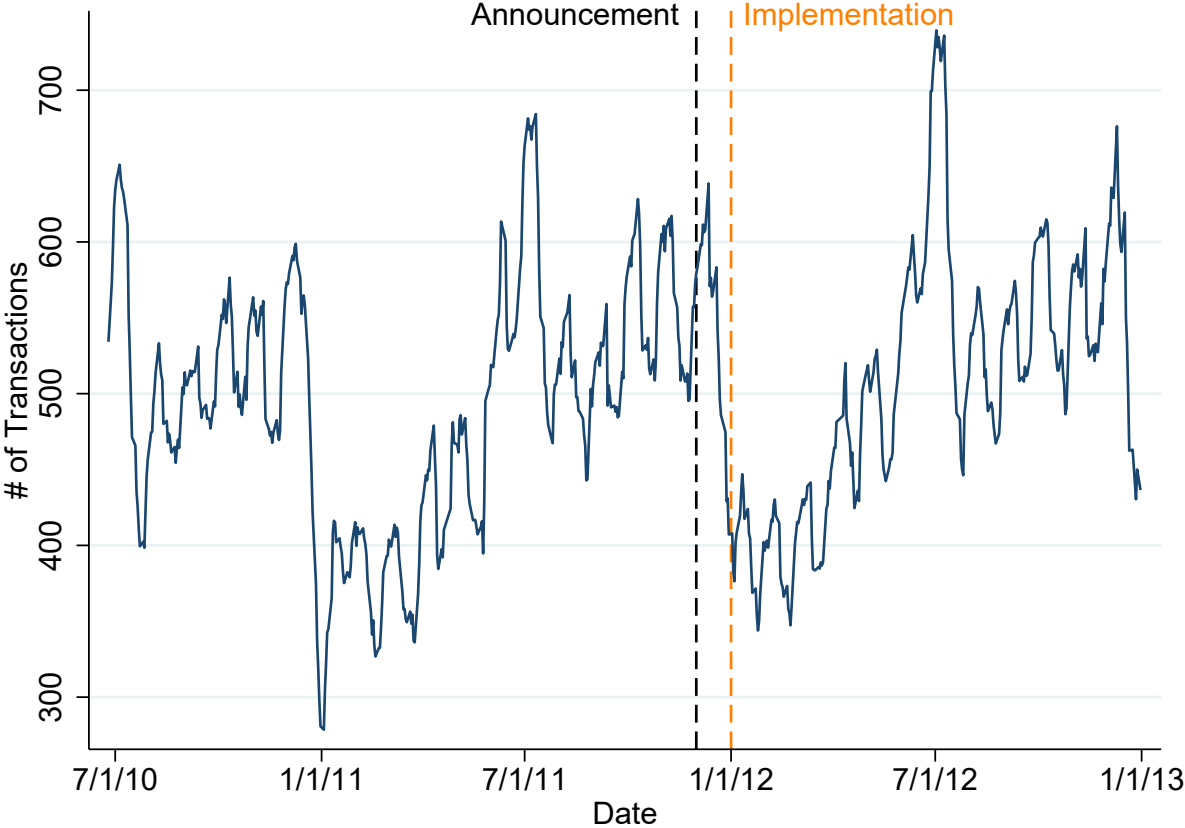


Figure A6: **Dynamic impact of LTV policy on LTV ratio**

This figure shows the dynamic effect of the LTV policy on LTV ratio. The sample is worker level data between 2006 and 2013, 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. Dependent variable is 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 then the LTV threshold value. Figure shows the β 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.

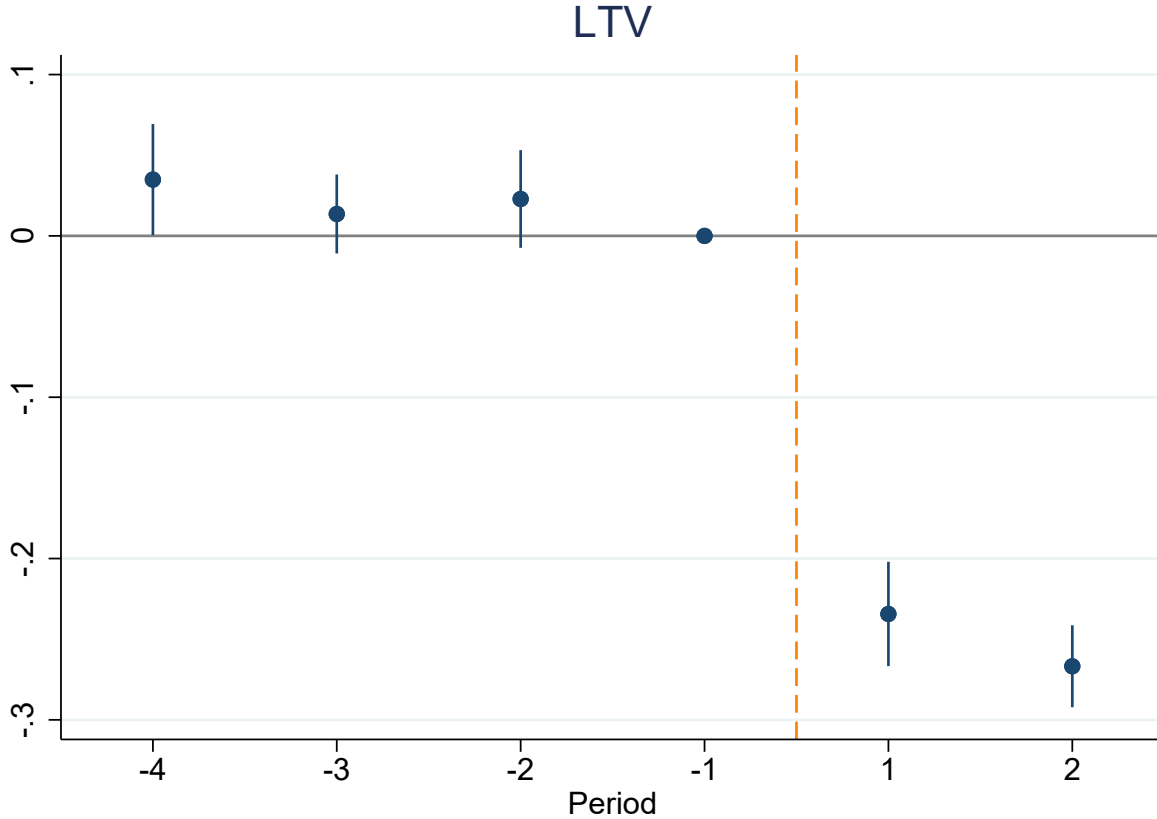


Figure A7: Dynamic impact of macroprudential policy on mortgages, house prices, and deposits

This figure shows the dynamic effect of the LTV policy on mortgages, house prices, and deposits. The sample is worker level data between 2006 and 2013, 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 then the LTV threshold value. Figure shows the β 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.

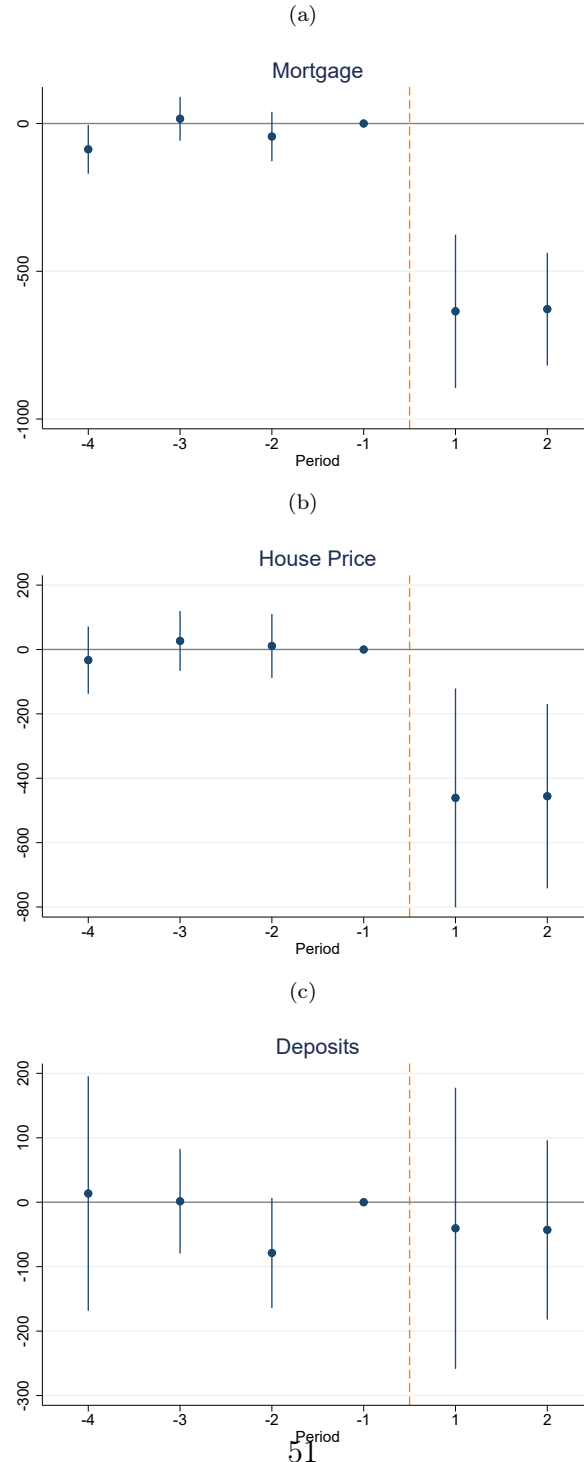


Figure A8: **Dynamic impact of macroprudential policy on interest expense**

This figure shows the dynamic effect of the LTV policy on workers' interest expense. The sample is worker level data between 2006 and 2013, 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 then the LTV threshold value. Figure shows the β 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.

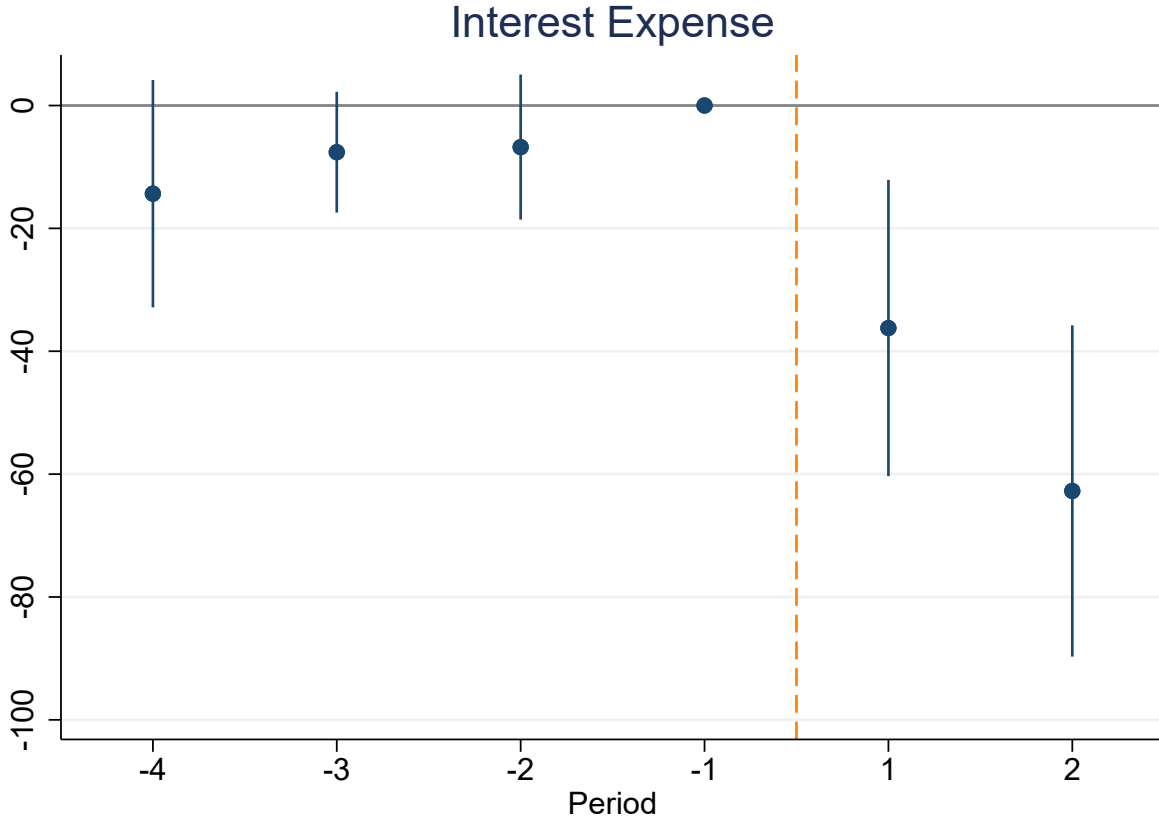


Figure A9: Variable Importance

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.

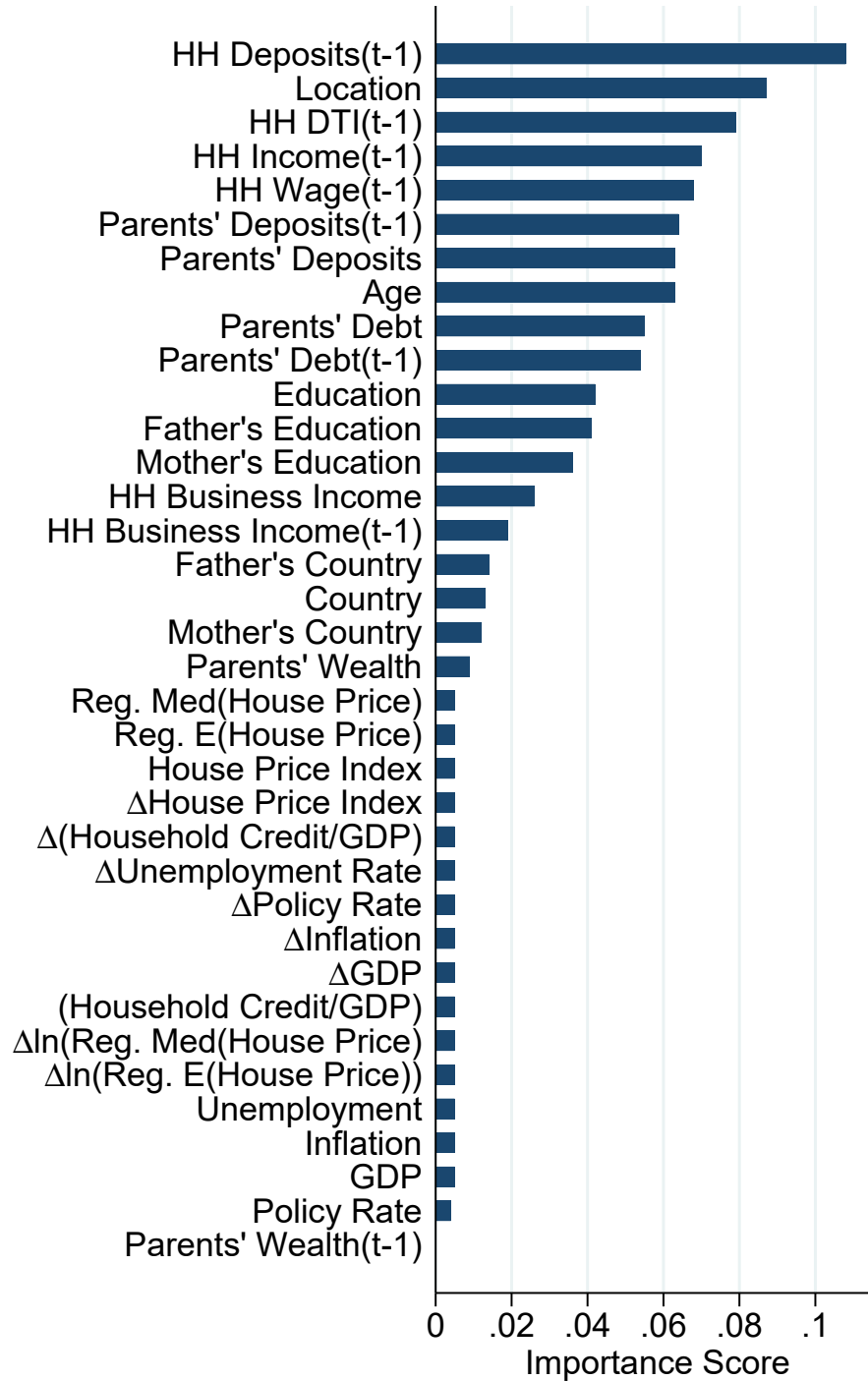


Figure A10: **Transition into Homeownership Rate**

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.

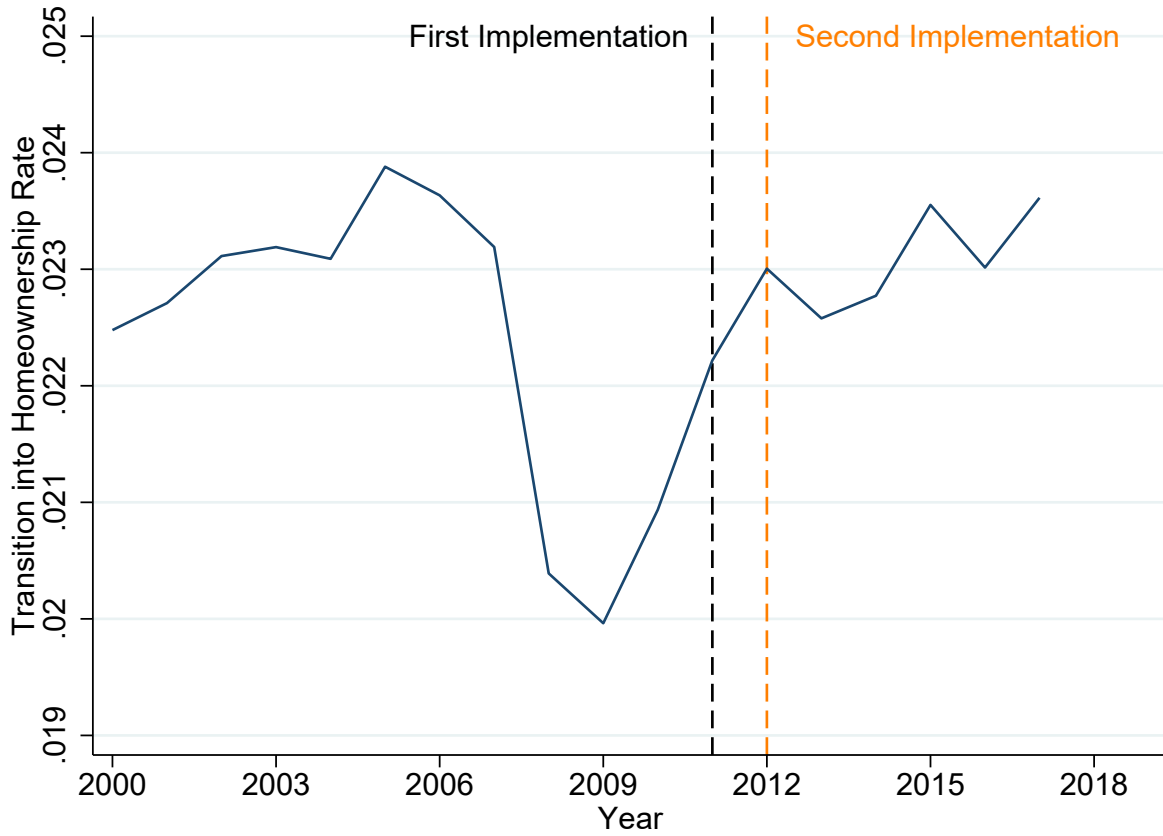


Figure A11: **Simulation exercise for placebo test**

This figure plots the coefficient distribution of $d(L\hat{T}V > 0.85) \times Placebo$. In the placebo-post period, 20% of the households are removed randomly to mimic the design the main sample. Each histogram uses 10,000 draws. Orange bars use a plain model without any fixed effects. Blue bars use a model with year, education, location, and industry fixed effects. None of the estimated coefficients is significant in the plain model. Only four estimated coefficients are significant in the saturated model.

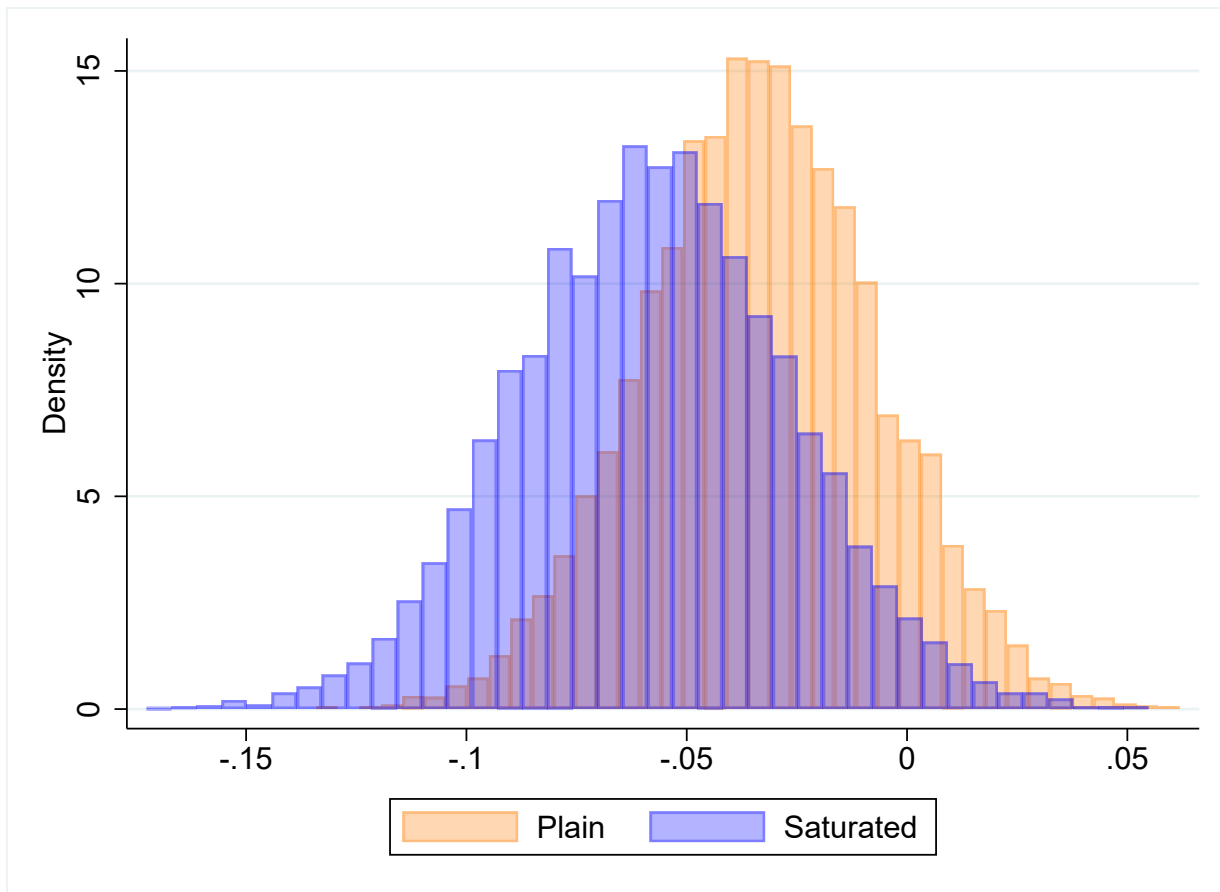


Table A1: **Impact of macroprudential policy on LTV ratio**

This table documents the effectiveness of the LTV ratio policy on the LTV ratios. Each column uses worker level data between 2006 and 2013, 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. 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 then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 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)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	-0.235*** (0.021)	-0.234*** (0.021)	-0.229*** (0.021)	-0.225*** (0.017)	-0.226*** (0.018)	-0.218*** (0.030)
$d(\widehat{LTV} > 0.85)$	0.234*** (0.014)	0.233*** (0.014)	0.221*** (0.015)	0.216*** (0.015)	0.216*** (0.014)	0.212*** (0.019)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location \times Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R ²	0.211	0.213	0.278	0.290	0.291	0.343
Mean(LTV)	0.924					

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 between 2006 and 2013, 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. 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 then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 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)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	-603.153*** (114.309)	-667.540*** (126.417)	-436.306** (156.551)	-503.119*** (150.137)	-69.821 (81.675)	-109.932 (137.884)
$d(\widehat{LTV} > 0.85)$	-119.832* (65.223)	90.282 (61.379)	-486.696*** (93.149)	-229.524** (81.908)	-198.473*** (12.966)	-176.430*** (45.433)
<i>Fixed Effects:</i>						
Year FE		✓		✓		✓
Education FE		✓		✓		✓
Location FE		✓		✓		✓
Industry FE		✓		✓		✓
Location × Industry FE						✓
Obs.	1,876	1,833	1,876	1,833	1,876	1,833
R ²	0.034	0.256	0.114	0.323	0.096	0.247
Mean(Dependent Var.)	1721.468		1956.405		222.015	

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 between 2006 and 2013, 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. $d(\widehat{LTV} > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 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)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	-45.875*** (10.390)	-44.626*** (9.821)	-41.265*** (13.315)	-36.504** (14.011)	-31.523** (13.681)	-37.456** (16.988)
$d(\widehat{LTV} > 0.85)$	-7.803** (2.769)	-8.570*** (2.173)	-4.688 (3.609)	-2.726 (4.285)	-2.684 (4.278)	-0.780 (5.007)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location \times Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R ²	0.014	0.106	0.224	0.249	0.267	0.316
Mean(Interest Expense)	91.489					

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. 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 then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 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)
$d(\widehat{LTV} > 0.85) \times Post$	-1.035*** (0.323)	-1.023*** (0.339)	-0.986*** (0.320)	-0.788** (0.337)	-0.934** (0.380)	-0.796 (0.480)
$d(\widehat{LTV} > 0.85)$	0.793*** (0.119)	0.778*** (0.115)	0.866*** (0.127)	0.890*** (0.147)	0.884*** (0.143)	0.883*** (0.159)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location \times Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R ²	0.030	0.035	0.152	0.177	0.200	0.253
Mean($\frac{Debt-Dep.}{Income}$)	3.911					

Table A5: Removing treated households that cannot afford the down payment before the policy

This table documents that removing the households that cannot afford the down payment does not affect the impact of the LTV ratio policy on wage growth for displaced workers, after controlling for the available liquidity by including a cubic function of the available liquidity. Each column uses individual 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 households that purchase a house before the policy, obtain a mortgage with an LTV ratio higher than the threshold and do not have enough deposits for the hypothetical down payment are removed from the sample. 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 then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. *Liquidity* is calculated as the deposits after taking out the down payment required by the LTV ratio restriction for pre- and post-treatment periods. 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)
$d(\widehat{LTV} > 0.85) \times \text{Post}$	0.265*	0.274*	0.403**	0.397**	0.327*	0.193
	(0.142)	(0.135)	(0.160)	(0.164)	(0.183)	(0.219)
$d(\widehat{LTV} > 0.85)$	-0.033	-0.041	-0.030	-0.013	-0.013	0.033
	(0.053)	(0.052)	(0.048)	(0.050)	(0.047)	(0.062)
$\ln(\text{liq.})_{t-1}$	0.248	0.204	0.287*	0.278*	0.345**	0.124
	(0.163)	(0.161)	(0.158)	(0.151)	(0.152)	(0.144)
$\ln(\text{liq.})_{t-1} \times \ln(\text{liq.})_{t-1}$	-0.044	-0.037	-0.051*	-0.049*	-0.060**	-0.025
	(0.026)	(0.026)	(0.026)	(0.024)	(0.025)	(0.023)
$\ln(\text{liq.})_{t-1} \times \ln(\text{liq.})_{t-1} \times \ln(\text{liq.})_{t-1}$	0.002*	0.002	0.002**	0.002**	0.003**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location \times Industry FE						✓
Obs.	941	941	927	927	927	927
R ²	0.018	0.032	0.147	0.165	0.187	0.298
Mean(Wage Growth)	-0.074					

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Table A6: Impact of policy on wage growth

This table documents that wage growth of treated and control groups do not react to macroeconomic variables differently. Each column uses worker 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. 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 then the LTV threshold value. $Post$ equals 1 for the years 2012 and 2013 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					
	(1)	(2)	(3)	(4)	(5)	(6)
$d(\widehat{LTV} > 0.85) \times Post$	0.744*** (0.154)	0.744*** (0.154)	1.030*** (0.325)	1.053*** (0.284)	0.983*** (0.329)	1.025* (0.555)
$d(\widehat{LTV} > 0.85) \times Inflation$	-0.300** (0.142)	-0.300** (0.142)	-0.462 (0.272)	-0.476* (0.249)	-0.478* (0.269)	-0.589 (0.522)
$d(\widehat{LTV} > 0.85) \times Unemployment$	0.833 (0.541)	0.833 (0.541)	1.421 (1.032)	1.419 (0.931)	1.429 (1.018)	1.808 (1.975)
$d(\widehat{LTV} > 0.85) \times GDP$	-0.185** (0.081)	-0.185** (0.081)	-0.278* (0.159)	-0.287* (0.144)	-0.280* (0.160)	-0.343 (0.294)
$d(\widehat{LTV} > 0.85) \times Policy\ Rate$	0.395* (0.193)	0.395* (0.193)	0.611 (0.378)	0.616* (0.335)	0.610 (0.372)	0.754 (0.692)
$d(\widehat{LTV} > 0.85)$	-3.074 (1.855)	-3.074 (1.855)	-5.102 (3.560)	-5.073 (3.182)	-5.076 (3.510)	-6.370 (6.698)
<i>Fixed Effects:</i>						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location \times Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R ²	0.017	0.017	0.095	0.111	0.124	0.186
Mean(Wage Growth)	-0.074					