Household Leverage and Labor Market Outcomes Evidence from a Macroprudential Mortgage Restriction

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Abstract

Does household leverage matter for job search, matching in the labor market, and pay? To answer this question we exploit a loan-to-value ratio restriction in Norway that exogenously reduces household leverage with a sample of displaced workers who lost their jobs due to mass layoffs. We find that a reduction in leverage improves the starting wages of displaced workers. Lower leverage allows workers to search longer, find jobs in higher paying firms, and switch into new occupations and industries. The positive effects are long-lasting and more pronounced for young and higher educated workers. Our results indicate that policies aimed at limiting household leverage have the potential to substantially improve workers' labor market outcomes by reducing the constraints that leverage creates in the job search.

JEL classification: E21, G21, G51, J21.

Keywords: Household Leverage, Household Debt, Job Displacement, Job Search, Macroprudential Policy.

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1 Introduction

Household leverage is an important micro-level driver of the economy. An increase in household leverage can fuel a housing boom, presage lower GDP growth and higher unemployment, as well as weaken financial stability.[1] Faced with such risks, many countries have introduced both microprudential and macroprudential measures to limit the impact of household leverage on the economy. These policies face the challenge of properly trading off the costs of restricting lending in good times against the benefits of a less pronounced decline in bad times. These and similar trade-offs have sparked a debate about the effectiveness and side effects of measures to restrict household borrowing.[2] While existing research has primarily focused on the effects of household leverage restrictions on housing-market outcomes, we instead focus on the interaction of household leverage and labor market outcomes. Specifically, we study how household leverage affects the starting wages and job search of workers who were displaced from their jobs.

Theoretically, household leverage can influence the starting wages through opposing channels. On the one hand, higher leverage increases the share of wages that is channeled to debt repayment. This may reduce people's willingness to work as it lowers the benefits they get from wages. To mitigate this debt-overhang problem, firms may raise the wages in their job postings to incentivize the workers (Donaldson et al., 2019). This channel implies a positive effect of household leverage on starting wages. On the other hand, higher household leverage may reduce starting wages. Workers with higher leverage may have different preferences regarding job offers, as higher leverage increases their default probability. For instance, to avoid costly defaults or to keep housing consumption unchanged, workers with high leverage

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

[2] Research has shown that these 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 Bekkum et al., 2019; Peydró et al., 2020). Tzur-Ilan (2020) and Aastveit et al. (2020) point out some negative side effects these policies may bring about. Galati and Moessner (2013) and Claessens (2015) provide a thorough discussion about macroprudential policy tools.

may prefer earlier but certain job offers to later offers with possibly higher wages (Chetty and Szeidl, 2007; Ji, 2020). Or, such workers can make suboptimal job search decisions due to higher debt (Gathergood et al., 2019; Martinez-Marquina and Shi, 2021), which can worsen their starting wages.

We test the effect of household leverage on starting wages and job search by exploiting a macroprudential policy implemented in Norway, which creates an exogenous variation in household leverage. Our main finding is that a reduction in household leverage improves the starting wages of workers who are looking for a job. Specifically, we show that a decline in household's debt-to-income (DTI) ratio by 25 percent reduces the decline in wages by 3.3 percentage points.[3] We explain our main result by documenting that the policy-induced reduction in leverage enables workers to perform a better job search in three ways. First, following the mandated reduction in their leverage, workers prolong their unemployment duration by approximately 2.5 months. Second, these workers with lower leverage find jobs in firms with a higher wage premium. Third, these workers with lower leverage are more likely to switch to other occupations and industries, which implies that their job search reach is broader. Furthermore, our results indicate that the improvement in the wages does not diminish over time. Overall, our results depict a clear picture, in which an exogenous reduction in leverage allows workers who are looking for a job to attain better matches in the labor market.

Studying how household leverage affects labor market outcomes and job search behavior faces two empirical hurdles. The first is an econometric one. Decisions on debt, job search, and job acceptance are likely to be made jointly. Studying the role of households' leverage in their labor market behavior thus depends on having an exogenous source of variation in debt and job search behavior. The second is the steep data requirement. Gaining a thorough understanding of how leverage affects households' labor market choices requires

[3]We measure the household leverage by DTI ratio. This ratio is households' total debt divided by households' total income before job displacement. The wages are measured at individual (worker) level. Section 3 explains the construction of these variables.

access to household-level loan data and granular labor market information.

We overcome these two challenges and attempt to take a step forward in identifying the role of household leverage for labor market outcomes. We tackle the econometric challenge in two steps. First, we make use of the introduction of a macroprudential policy that requires Norwegian banks to put a cap on maximum loan-to-value (LTV) ratio for home purchases, which effectively restricts households' leverage. This creates exogenous variation in households' DTI ratio. As the Norwegian LTV ratio restriction is applied to the all home-buyers, all LTV ratios after the restriction are below the cap. This feature of the restriction masks the unaffected households, which would obtain their mortgages with an LTV ratio below the cap even if the LTV restriction did not exist. To distinguish the households that are unaffected by the restriction from the affected ones, we employ a prediction model (Abadie, 2005).[4] More specifically, we use a Random Forest classifier, a machine learning method, to construct the treatment and control groups. Second, we restrict the worker sample with the displaced workers. The displaced workers lost their jobs due to a mass layoff–a case in which a firm loses at least 30% of its employees in a year. Therefore, the job search of these workers is not triggered by individual characteristics. Moreover, all of the displaced workers in our sample recently bought a house, up to 12 months before their displacement. Thus, heterogeneity in the ability to build up home equity does not intervene to the effect of LTV restriction on the household leverage since workers do not have enough time to build home equity. After constructing the sample and treatment and control groups, we employ a difference-in-differences analysis to causally identify the impact of household leverage on the labor market outcomes and job search.

We tackle the steep data requirement with the help of several population registers available in Norway. First, we obtain debt, income, and wealth data from the official tax filings for the entire adult population of Norway. Then, we merge these tax data with register

[4] Van Bekkum et al. (2019); Aastveit et al. (2020) have a similar strategy in an LTV ratio restriction setting.

data from the national Register of Employers and Employees, to which all employers and contractors are obliged to report their workers and freelancers as well as details on the employment relationship. Finally, we complement this with information on home purchases collected by the Norwegian Mapping Authority. This unique data set includes information about individuals' assets and liabilities, wages, unemployment duration, job choice as well as other individual characteristics such as education and immigration status.

We analyze the balance sheet and labor market outcomes of displaced workers who recently bought a house before their displacements and are affected by the macroprudential policy, with recent home-buyer displaced workers but are not affected by the policy. We obtain *3 main results*. Overall, our results show that curtailing household leverage provides lasting, positive, effects on displaced employees' labor market outcomes.

First, upon introduction of the LTV ratio restriction, the treated workers experience a drop in their household leverage. More specifically, the restriction reduces the treated workers' DTI ratios by 25 percent. This decline in the DTI ratio is realized by means of a reduction in the mortgage size and a downward adjustment in the house prices. Thus, these findings indicate that the LTV restriction was highly effective in constraining borrowing by households and therefore is an excellent experimental setting to investigate the implications of household leverage on job search behavior and related labor market outcomes.

Second, using the decline in household leverage caused by the LTV ratio restriction, we identify the effect of leverage on the labor market outcomes of displaced workers who recently bought a house before their displacement. We find that, after the reduction in leverage, workers with reduced leverage manage to realize a 45 percent higher wage growth after their displacements. This finding indicates that a 25 percent decline in household's DTI leads to a 3.3 percentage points smaller decline in wages compared to the 7.4 percentage points average reduction after a displacement. The improvement in starting wages is robust to controlling for individual, location, and industry specific characteristics, refining the worker sample by removing workers with possibly different attachment to the labor market, making the sample more similar along the LTV ratios, and controlling for macroeconomic conditions. More importantly, we show that the effect of leverage on starting wages is not driven by endogenous selection to the housing market. The LTV restriction can affect the house purchase decisions directly. Households that cannot afford the down payment can try to buy their houses before the implementation of the LTV restriction. Alternatively, such households can delay their purchase until they can afford the down payment. Due to the endogenous selection into the housing market, the treated workers may have different characteristics before and after the restriction, which can partially drive our results. We mitigate the selection concern in two ways. First, we show that the LTV restriction does not change observable characteristics of the treated workers. Second, we restrict our sample to the ones who are able to afford the down payment both before and after the restriction. Since the main reason for endogenous selection is ability to pay for the down payment, this restricted sample does not suffer from this selection concern. Our estimates obtained from this restricted sample are remarkably close to our main results, which indicates that our results are not driven by selection effects.

Third, we document that the improvement in starting wages stems from a decline in constraints in the job search that higher leverage creates. This reduction in the constraints influences starting wages positively through several channels. After the reduction in their leverage, workers are able to stay as unemployed for 2.5 months longer, which suggests that lower debt reduces the pressure on displaced workers to quickly find and accept a new job. Moreover, the reduction in leverage enables the displaced workers to search for job matches with firms that pay higher wages. Following Abowd et al. (1999), we estimate firm wage premiums using the whole Norwegian population and establish that treated displaced workers find jobs at firms that pay a 7.5 percent higher wage premium. This higher firm-specific wage premium explains 20 percent of the gain in starting wages. Reduction in leverage also facilitates workers in broadening the scope of their job search. Workers with lower leverage are around 20 percent more likely to change their occupation at a new employer and/or find a new employer in another industry. Our results show that changes in geographical labor mobility or investment in additional education are not a driver of our results.

We support the mechanism we document by recording how heterogeneity across the sample affects the positive effect of reducing household leverage. If a reduction in leverage improves workers' starting wages through improved job search, we may expect to find greater gains for subsamples who have more potential to benefit from a better job search. We find that workers younger than age 33, with shorter job tenure with the previous employer or with higher education drive the improvement in starting wages. This is consistent with the notion that it is easier for younger workers or workers with higher education to invest in human capital required for a different occupation or industry. Longer previous job tenure with the same firm also tends to make human capital more firm-specific and limit the value of a better job search. In addition, further heterogeneity tests indicate that the improvement in starting wages is particularly stronger for female and low-income workers. These findings imply that a reduction in household leverage and related improvements in the job search may be important for workers who are disadvantaged in the labor market.

Finally, our ex-post displacement analysis reveals that the positive impact of lower leverage on starting wages is persistent. Four years after their displacement, the LTV restriction allows the workers with reduced leverage to maintain a significant wage growth advantage. More specifically, these workers have a 4.7 percentage points higher wages at the end of the four-year post displacement period. The same workers also enjoyed lower wage volatility during the four years after their displacement, indicating that the rise in wages is not attributed to jobs with greater hours volatility or greater risk for discontinuation.

In sum, our paper documents the constraints that household leverage creates in the job search of displaced workers and a reduction in these constraints improves the starting wages of these workers. Our results depict a clear picture in which macroprudential policies that aim to limit household leverage can have positive side effects for the labor market. One concern about such macroprudential policies is that due to the down payment requirement that they introduce, these policies can delay households' house purchases, which can decrease affected households' utility. We show that, in case of a job loss, thanks to the macroprudential policies, these households may have better prospects in the labor market. In addition, our results help us to understand the nature of an economy that enters into a recession with high household leverage. These results suggest that developing labor market tools to facilitate matching quality can be important to cope with such recessions.

The findings in our paper speak to at least four strands of the literature. First, our paper adds to the literature which shows that households' debt and credit access affects labor markets through a demand channel. This channel starts with the negative effect of household leverage on credit availability. This negative effect can occur due to detrimental effect of leverage on financial stability (Reinhart and Rogoff, 2008; Schularick and Taylor, 2012; Corbae and Quintin, 2015), or on collateral values (Adelino et al., 2016). The decline in credit availability may entail deleveraging by households. Due to the required deleveraging, households may have to cut their spending (Eggertsson and Krugman, 2012; Mian et al., 2013; Guerrieri and Lorenzoni, 2017), which in turn puts a pressure on aggregate demand and increases unemployment (Mian and Sufi, 2014; Mian et al., 2017). Our findings complete these analyses by documenting the direct effect of household leverage on the labor markets. In addition to the indirect demand channel, household leverage affects the workers' labor market outcomes and job searches by introducing restrictions that reduces matching quality in the labor market.

Second, our paper is related to the literature that studies the determinants of labor supply and job search. Unemployment insurance and severance pay are considered to be the main determinants of labor supply of the unemployed.[5] Bernstein and Struyven (2017); Brown and Matsa (2019); Bernstein (2020) and Gopalan et al. (2020) document how negative

[5]The relation between unemployment insurance, severance payments and labor supply has been studied extensively. See Lalive et al. (2006); Chetty (2008); Rothstein (2011); Card et al. (2007) and Basten et al. (2014).

home equity following from a decline in house prices hurts labor mobility and labor supply. Liquidity constraints (He and le Maire, 2020; Kumar and Liang, 2018), credit access during unemployment (Herkenhoff, 2019), mortgage payments (Zator, 2019), and wealth shocks (Cesarini et al., 2017; Bernstein and Koudijs, 2021) also influence individual labor supply decisions.[6] We contribute to this growing literature in three ways. First, we document that, in addition to the mentioned variables, household leverage is an important determinant of the labor supply.[7] Our first contribution complements the existing papers by providing new insights in the following way. Positive effect of access to credit and mortgage payments on wages documented by earlier papers suggests a positive effect of household leverage on wages as well, since household leverage is positively correlated with access to credit and mortgage payments. However, contrary to this suggestion, we find a negative effect of leverage on wages, which improves our understanding about how household leverage interacts with the economy in general. Our second contribution is that we focus on displaced workers who lost their jobs due to mass layoffs, instead of the whole population or all home buyers. Our sample design allows us to cleanly estimate the changes in starting wages and job search as job search behavior of the displaced workers is not triggered by any individual characteristics. Third, using detailed individual labor market data, we can lay bare the exact mechanisms through which changes in the job search affect the starting wages. This allows us to measure the influence of job search on the starting wages.

Third, our paper contributes to the discussions about how macroprudential policy tools are affecting the economy by investigating the influence of an LTV ratio restriction, one of the most popular macroprudential tools, on the labor markets. On the one hand, these policy tools are able to curb the credit booms and improve financial stability (Borio, 2003; Igan and Kang, 2011; Claessens et al., 2013; Cerutti et al., 2017; Van Bekkum et al., 2019; Defusco

[6] See also Mulligan (2009, 2010); Li et al. (2020) 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).

[7] Bednarzik et al. (2017); Meekes and Hassink (2019); Fontaine et al. (2020) document correlations between household balance sheets and labor market outcomes.

et al., 2019; Araujo et al., 2019; Peydró et al., 2020).[8] On the other hand, these tools can generate some negative unintended side effects (Acharya et al., 2019; Aastveit et al., 2020; Tzur-Ilan, 2020). We document an additional, but positive, unintended effect for an LTV ratio restriction on the labor markets. Having a similar research question to ours, Pizzinelli (2018) develops a life-cycle model with LTV and LTI restrictions to study second earners' labor supply. While Pizzinelli (2018) does not find any effect of an LTV restriction on female employment in his model, we empirically document that, by reducing household leverage, the LTV restriction increases displaced workers' starting wages.

Fourth, our paper adds to the papers that investigate the consequences of job displacement. This literature has found that the decline in earnings after being displaced can be large and long-lasting (Jacobson et al., 1993; Couch and Placzek, 2010; Davis and Von Wachter, 2011; Lachowska et al., 2020) and depend on the business cycle and employer characteristics (Schmieder et al., 2018; Moore and Scott-Clayton, 2019).[9] Losing one's work in a mass layoff also affects the private sphere through increased mortality and divorce rates (Charles and Stephens, 2004; Sullivan and von Wachter, 2009). We contribute to this literature by demonstrating that policy-induced reductions of household leverage can mitigate the loss of income after following job loss.

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

[8] See Farhi and Werning (2016); Dávila and Korinek (2018) for theoretical justifications for macroprudential policies.

[9] The decline in earnings after a mass layoff is not age-dependent (Ichino et al., 2017). Halla et al. (2020) show that intra-household insurance may not be sufficient to cover this income loss.

2 Institutional Background

2.1 The Norwegian economy

Norway's economy has displayed stable economic growth, with both inflation and average unemployment below four percent during the past 30 years. During the Financial Crisis of 2008-2009 GDP fell by only 1.7 percent. During the full sample period, Norges Bank's policy rate varied between 5.75 and 1.25 percent (Figure A1). As shown in Figure 1a house prices have nearly tripled since 2000. Norwegian households' debt to GDP ratio has simultaneously increased from 50 percent to 105 percent (Figure 1b). The home ownership rate in Norway is above 80 percent and among the highest in advanced countries. Nine out of ten mortgages have floating interest rates. Mortgages are full recourse.

2.2 Macroprudential policy

Norway is not a member of the EU but has committed to implementing the relevant EU financial sector directives through the EEA-agreement. The main legal basis for regulating financial undertakings and credit markets is the Act on Financial Undertakings and Financial Groups (Lov om Finansforetak og Finanskonsern, henceforth AFU) which has been in effect since 1 January 2016. In Norway, the main responsibility for financial stability lies with the government, and the Ministry of Finance has been the designated macroprudential authority since 2016. The financial stability instruments are shared among the Ministry of Finance (MoF), the Financial Supervisory Authority (Finanstilsynet, FSA) and the Central Bank (Norges Bank). While the FSA advises the MoF on desirable regulations under AFU, decisions on new regulations are taken by the Ministry.

The steep rise in house prices and household indebtedness and possible spillover effects on financial stability raised concerns with the Norwegian policymakers already before 2010. To reduce households' vulnerability and banks' exposure to housing markets, the FSA issued

"Guidelines for prudent lending practices for loans for residential purposes" in both 2010 and 2011. The March 2010 guidelines established a maximum permissible LTV ratio of 90 percent. Compliance with the new guidelines was expected by the fall of 2010. Through the December 2011 update the FSA reduced the LTV to 85 percent and specified that mortgages granted on the same property by other lenders shall also be included in the LTV ratio.[10] Interest only mortgages and collateralized lines of credit were restricted by an LTV-ratio of 70 percent. The guidelines permitted banks to deviate from the target ratios if borrowers could pledge additional collateral or the bank performed a special prudential assessment. The guidelines further specified that each lender's board of directors was responsible for establishing criteria for the special prudential assessments and taking action on any deviation from the guidelines. Financial institutions were instructed to immediately start incorporating the guidelines in their internal guidelines.[11] In our analysis, we focus on the joint effect of the two LTV restrictions and consider 2012 the first year in the post-period, while excluding 2010 from the pre-period.

2.3 Labor market regulation

The labor market in Norway is governed by the Working Environment Act and the Labor Market Act, both of 2005.[12,13] 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, i.e., situations where notice of dismissal is given (a) to at least 10 employees within a 30 day period (b) *without grounds related to the individual employees*. An employer contemplating collective redundancies needs to start consultations with employees' elected

[10]The motivation for the update was that "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." See Finanstilsynet (2011)

[11]In July 2015, after adoption of the AFU, the MoF converted the guidelines into a regulation (Ministry of Finance, 2021).

[12]Act relating to working environment, working hours and employment protection.

[13]Lov om arbeidsmarkedstjenester

representatives at the earliest possible opportunity. The notification period for job termination depends on the employee's job tenure and age and can range from a minimum of 30 days up to six months.[14]

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 for a period of 52 or 104 weeks. In 2010 the annual basic amount was NOK 75,641 (USD 12,712), compared to NOK 101,351 (USD 11,852) in 2021. Employees needed 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.[15] To be entitled to the maximum of 104 weeks of unemployment benefits a person had to earn 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. In 2010, an employee thus had to earn at least NOK 151,282 (USD 25,425) to be eligible for two years of unemployment benefits. No person could receive annual unemployment benefits in excess of NOK 238,199.[16] The law requires people to be an active job seeker and to inform the Labor and Welfare Service office about ongoing job search activity every fortnight. If a person cannot search for a job search due to illness or other circumstances, benefits can be reduced or discontinued. Most importantly for our purposes, the unemployment benefits scheme did not change during our sample period.

3 Data and Sample Construction

We combine several official Norwegian population registers. Each data set covers the full adult population of Norway and we link the data sets with a unique, anonymized, personal

[14]The shipping, hunting and fishing, military aviation and public service sectors are excluded from the Working Environment legislation.

[15]NAV, the Norwegian Labour and Welfare Administration, disregards any income above six basic amounts per period of 12 months

[16]62.4 percent of six times the basic amount (75,641)

identifier. Below, we introduce the data sets and describe how we construct our sample and variables.

3.1 Data sets

We obtain the labor market data for our study from the official employer-employee register (Aa registeret) administered by the Norwegian Labour and Welfare Administration (NAV). All employers and contractors are obliged by law to report their employees and freelancers as well as details on the employment relationship. In this register, we can track for which employer an employee 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 administrative data from the population register as well as 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, 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 seller and the buyer, the transaction value, and a location identifier.

3.2 Sample construction

Our main sample period starts in 2006 and ends in 2013.[17] We analyze workers who have experienced both an exogenous change in their leverage and who were displaced during the sample period due to a mass layoff. From the full Norwegian population, we therefore draw

[17] Because of a change in the enforcement of data reporting standards, we have excluded employment data from 2014 and onward. Before 2014 reporting workers and labor contract data at the branch or head office was condoned by NAV. The 20014 reporting change generates noise in the data because of large numbers of "within concern" job changes.

workers who satisfies two criteria: (a) job loss due to a mass layoff, (b) bought a house before the job loss. To avoid a bias that an unobserved ability to building up home equity, we restrict our sample to workers who bought their homes no more than 12 months before their displacement.

Mass layoffs provide an appropriate setting for our research because they trigger job displacement exogenously, i.e., job displacement that is not caused by worker-specific characteristics (Flaaen et al., 2019). We define a mass layoff as a situation where a firm parts from at least 30 percent of its workers in a year or ceases its operation entirely. We follow the literature (Lachowska et al., 2020; Von Wachter et al., 2009; Sullivan and Von Wachter, 2009) and use only firms with at least 50 employees to limit the risk that we mistake laying off smaller numbers of workers for idiosyncratic reasons for a mass layoff. Applying the above two criteria yields us 1880 workers who are displaced between 2006 and 2013 from 564 different firms.

3.3 Variable construction

In this project, we use variables at two levels. Since the LTV ratio restriction applies to debt at the household level, we use the household as the unit of observation for variables that matter for the policy, i.e., household leverage. We also measure deposits, income, and interest costs at the household level. When considering labor market outcomes and job search behavior, we switch to the individual (worker) level. In this part of the paper, we introduce the main variables we use and provide summary statistics in Table 1.

Due to Norway's lack of a credit registry during our sample period, we cannot disentangle mortgage credit from other loans using a credit type identifier. We therefore infer the LTV ratios by using official tax register data. All Norwegian banks report individual data on debt, deposits and interest received and paid to the Norwegian Tax Administration for the purpose of producing pre-filled personal tax filings. By checking the Mapping Authority's register, we can identify people who did not own (part of a) house in the previous year. To reduce the influence of the existing debt on LTV ratio calculation, 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. This means we will slightly overestimate the LTV ratio if a household takes an additional unsecured loan or increases its utilization of an existing line of credit in the year of the home purchase. The average of LTV ratio is 92% in our sample. Unlike the LTV ratio, we can measure the DTI ratio exactly as the tax filings provide exact information about the total debt and total income. We calculate the DTI ratio by dividing household's total debt to household's total income before the mass layoff. The average value of this ratio is 4.24 in our sample with a standard deviation of 2.10.

To assess the impact of the LTV ratio restriction on the starting wages of the displaced workers, we use the wage growth between the job that workers are displaced from and the next job that they find as the wage variable. We follow the literature and use symmetric growth rate to allow for labor market exit and limit the role of outliers.[18] In line with the job displacement literature, the average wage growth for displaced workers is negative in our sample. Our data set allows us observe the job start and job end dates. Using these dates, we measure the unemployment spell as the number of days between the two jobs. On average, the displaced workers experience 132 days of unemployment spell in our sample.

To measure education, we use the Norwegian Standard Classification of Education at the 3-digit level. 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 the academic content. There are 142 unique education

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

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

levels in our sample.[19] To capture changes in profession, we use Statistics Norway's seven-digit occupational information that builds on the EU's ISCO-88 (COM) (Statistics Norway, 1998) four-level classification system. The first digit defines 10 major groups that combine broad professions and inform about the level of competence.[20] The remaining digits break down each main occupational category into further subgroups.

Of the workers in our sample, 15 percent resides in Oslo, close to the city's population weight in Norway. Roughly half of our sample was displaced from firms in the services industry, while the remaining half is evenly distributed among the other industries.

4 Empirical Strategy

Our objective is to causally identify the effect of household leverage on households' job search behavior and labor market outcomes. Because debt and labor choices are likely to be taken jointly, merely regressing labor market outcomes on household leverage will potentially yield a biased estimate of the coefficient on leverage. For instance, an employee with low labor skills that are not observed by the econometrician might assume higher debt since due to lower skills she relies on debt to sustain consumption. In addition, these lower labor skills could decrease the starting wage of this employee, which creates a spurious correlation between indebtedness and wages. We address this endogeneity challenge by exploiting the legally imposed LTV restriction as a source of exogenous variation in household leverage.

[19] The levels are primary, lower secondary, upper secondary, post secondary, 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. As an example, with 3-digit detail, we can differentiate whether a person with social sciences and law background studies in sociology or psychology.

[20] 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

4.1 Using macroprudential policy as an experiment

The FSA issued its first mortgage guidelines in 2010 and shortly after updated them in 2011. In our analysis, we use the date of the 2011 guidelines as the timing of the policy experiment. The initial 2010 guidelines proved to be ambiguous about the precise implementation period, and the FSA reported low compliance with the policy (Finanstilsynet, 2011). The updated guidelines are stricter and cover loans with collateral claims on a particular property from all lenders. In our main regressions, we therefore remove all observations between the two guidelines and let the post-treatment period start in 2012. Thanks to the absence of other regulations or policy changes that could affect labor markets, the LTV restriction policy provides a clean experimental setting in which we can study the impact of household leverage on labor markets.

One important feature of this LTV restriction policy is that it covers the whole population. Therefore, all observed LTV ratios are below the threshold after the policy.[21] However, this does not mean that every household is treated by the policy. The reason is that 35 percent of the population obtains mortgages with LTV ratios lower than the threshold before the policy, which implies that without the policy, approximately one-thirds of the households would obtain mortgages with LTV ratios lower than the threshold. These households are natural candidate for the control group. However, there is not a single variable that allows us to separate these households from the treated ones.

The common solution the literature applies to similar cases where the treatment status is missing after the policy is proxying the treatment status with one variable which is positively correlated with the treatment.[22] Facing a similar problem, recent papers on the effects of LTV

[21]There are few households whose LTV ratios are larger than the threshold after the policy. The reason is that lenders could grant loans with LTVs in excess of 85 percent if additional collateral was pledged or a special prudential assessment was performed. Anecdotal evidence indicates that collateral pledged by parents is the most common justification for a higher LTV. Since these households do not experience a change in their leverage and thus untreated, we remove these few observations from our estimation sample. The placebo test in Section 5.2 show that this removal does not create a bias in the wage growth regressions.

[22]For instance, He and le Maire (2020) use previous liquidity to construct the treated and the control groups.

restrictions follow Abadie (2005) and use linear probability prediction models to construct the treatment groups (Van Bekkum et al., 2019; Aastveit et al., 2020). In our paper, we take a step forward and use machine learning (ML) methods. More specifically, we use Random Forest (RF) method to classify households 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 intuitive since a rich set of variables has more information comparing to a single variable. Athey and Imbens (2019); Calvi et al. (2021) discuss how beneficial ML methods can be for the cases similar to ours. The second advantage is that unlike linear probability models, RF does not impose any functional form to the classification. Therefore, RF is capable of capturing the true data generating process better. Third, given that RF is designed to maximize out-of-sample forecast power with enough power to avoid overfitting, RF's classification is more robust for post-treatment period and inclusion of many variables.

We use RF to classify the households as treated and control units in three steps.[23] In the first step, we collect a rich set of household level data from the period between 2002 and 2010. The variables in this data set include household level income, wage, deposits, DTI, business income, education, age, location, immigration status. Moreover, we add parents' deposits, debt, wealth, education, and immigration status to the data.[24] To incorporate the influence of the macroeconomic conditions and house prices, we use GDP, inflation, unemployment, monetary policy rate, and regional and national house prices. Since there is no restriction for LTV ratios in this time period, we label the households with LTV ratio above the threshold as treated and the others as control.[25] In the second step, we use unconstrained treatment status and the variables from the first step to estimate the model's parameters.[26]. In the

[23]Details about the application can be found in section A2.
[24]All of the balance sheet items are lagged by one period.
[25]The sample used in the regression models is removed from the RF sample. This mitigates the concerns about overfitting.
[26]Pruning and how the parameters are chosen are explained in section A2.

last step, we classify the households in the regression sample into treated and control groups using the trained RF model.

Thanks to the data availability, the classification power of the RF is high. It correctly classifies 82% of regression sample before the policy. Another way to evaluate the classification performance is plotting Receiver Operating Characteristics (ROC) curve and calculating the Area Under the Curve (AUC). ROC curve shows the true positive rate and false positive rate for different probability thresholds to classify a household to be treated and AUC, which summarizes the information on the ROC curve, is the measure of the area under the ROC curve (Bradley, 1997). As a perfect classification model has a true positive rate of 1 and false positive rate of 0, an AUC closer to 1 indicates a successful classification model (James et al., 2013). In our case, the AUC is 0.88, which means that with an 88% probability, a randomly chosen treated household will have higher estimated treatment probability than a randomly chosen control household.[27]

Figure 3 shows how much each variable contributes to the performance of the classification model. One striking finding from this figure is that none of the variables dominates the improvement in the model. This implies that using one variable to proxy the treatment status would miss an important fraction of the information available to the researcher, which indicates the advantage of a prediction model over a single variable strategy. According to this figure, household balance sheet items, location, age, and parents' financials are important features that are related with the probability of being affected by the LTV ratio restriction. Table 2 lays out these differences between the treated and control groups. For instance, households that are classified into the treatment group have lower income and deposits. Moreover, their parents have lower deposits and wealth. Both groups share a similar immigration status. However, the treated group is less likely to have a college degree.

One concern about the classification model could be that the LTV restriction policy can

[27]Classification models with AUC larger than 0.90 are considered as excellent (Hosmer Jr et al., 2013), which implies that our model classifies the households successfully.

affect the house prices and this may reduce the model's accuracy after the policy. Figure A2 shows the house price growth rates for 9 largest counties. This figure documents that house price growth rates after the policy indicated by orange dots are between house price growth rates before the policy indicated by blue dots. As the after-policy house prices are in the support of before-policy house prices, the classification model is able to incorporate the effect of the LTV restriction on the house prices.

4.2 Empirical specification

After classifying households 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_{it} is either household balance sheet variables such as debt-to-income ratio or labor market variables such as wage growth. $d(\widehat{LTV} > 0.85)_h$ is a dummy variable that takes the value of 1 if a household is predicted to have an LTV ratio larger above the threshold. We saturate the difference-in-differences model with year, education, location, and industry fixed effects. Given that our sample consists of people who are displaced in mass layoffs, which 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 weren't implemented. The standard way to test this identifying assumption is to look the trends of the treated and control groups before the treatment. A confirmation that the trends are parallel provides 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:

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

where we replace $post_t$ with period dummies. Since we omit $period = -1$ in Equation 3, the estimated γ_k coefficients document the difference between treated and control groups at $period = k$ relative to that of at $period = -1$.

Our construction of the treated and control groups has two implications. First, we may incorrectly classify the treatment status. Lewbel (2007) documents that misclassification of a binary regressor creates an attenuation bias, akin to standard measurement error bias. This implies that our parameter estimates, if misclassification were an issue, will provide a lower bound for the effect of household leverage on labor market outcomes. The out-of-sample predictive power of our RF model being above 80 percent provides additional reassurance about the risk of misclassification. Moreover, as depicted in Figure A3, majority of the misclassified households are clustered around the threshold.[28] The impact of the LTV policy on household leverage is smaller for the households whose LTV ratios are closer to the thresholds since the policy reduces the leverage with a smaller amount. Therefore, having majority of the misclassified households clustered around the threshold suggests that the magnitude of the attenuation bias is limited. Second, since we use several proxy variables to assign households to the treated or control group, the groups are likely to differ along these variables. These differences can lead to different labor market outcomes without an exogenous change in household leverage.[29] While the proxy variables are allowed to affect the *levels* of the labor market outcomes, the above procedure rests on the assumption that

[28]Note that before the implementation of the restriction, we observe both the realized and predicted treatment status.

[29]Our regression results in Section 5 indicate that treated households have lower starting wages before the LTV restriction.

the proxy variables do not affect the *change* in labor market outcomes. This rules out the possibility that the influence of these proxy variables on labor market outcomes changes at the same time as the LTV restriction is introduced. Parallel trends graphs in Section 5 make it clear that the difference in labor market outcomes between the treated and control groups are stable before introduction of the LTV restriction, which confirms there is no change in levels of labor market outcomes in this time period.

5 The Impact of the LTV Restriction

Our analysis builds on the fact that a macroprudential policy aimed at reducing households' ability to borrow against collateral creates an exogenous reduction in affected households' indebtedness. The immediate effect of the macroprudential policy was to limit households loan-to-value ratios. In Section A1 we document that the LTV restriction is well-behaved and affects the LTV, balance sheet components, interest payments and the value of purchased houses in line with expectations. In this section we start by detailing the direct effect of the policy on households leverage, measured as the debt-to-income ratio, in Section 5.1. Section 5.2 describes the impact of the policy on wages while section 5.3 lays out the mechanism through which lower leverage affects wages and other labor market outcomes. Section 5.4 contains estimates of the longer term effects of the policy.

5.1 Impact of LTV restriction on household leverage

We expect the LTV restriction to limit affected households indebtedness. To provide visual evidence of how the LTV restriction reduces household DTI ratio, we estimate Equation 3 with the DTI ratio as the dependent variable. Figure 5 depicts the estimated coefficients. The difference between the DTI ratios of treated and control groups is essentially constant during the pre-treatment period, which lends support to the underlying assumption that the

DTI ratios of the two groups would follow parallel trends in the absence of the restriction. After the restriction, the treated group has substantially lower leverage. Table 3 displays the parameter estimates from the corresponding difference-in-differences model (Equation 2) of Section 4 and confirms the implications of Figure 5. In the baseline regression without any fixed effects, the LTV restriction reduces treated households' DTI by 109 percentage points. 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).

A potential concern about our model specification could be that mass layoffs may not occur randomly, which could bias our parameter estimates. To tackle the concern that layoffs may occur due to location or industry specific shocks, we also include location, industry, and location×industry fixed effects in Columns (4)-(6) to control for the selection problem that unobservables might generate. In all specifications, $d(\widehat{LTV} > 0.85)_h \times Post_t$ has a highly significant and negative coefficient that is quantitatively close to the estimate from the model without any fixed effects. The LTV restriction thus reduces treated households' leverage by on average 105 to 115 percentage points, which is 25 percent decline at the mean value.

In Section A1, we further document *how* the restriction reduces household leverage. Treated households take smaller mortgages, which they use to buy cheaper houses. We find that they, after introduction of the policy, on average take mortgages that are NOK 603,000 smaller to pay for homes that are NOK 503,000 cheaper. According to our estimations, the restriction restriction reduces households' liquidity, however, the estimated effect is not statistically significant. We complement these findings by showing that the LTV restriction also eases the interest expenses of treated households. This decline in interest expense and lower mortgage amortization expenses, thanks to smaller mortgages, together cut the cash outflow by approximately 10 percent of the household's wages before displacement.

5.2 Impact of household leverage on starting wages

After establishing the negative impact of the LTV restriction on household leverage, we next investigate how household leverage affects displaced workers' starting wages in their new employers. In principle, household leverage can have opposing effects on starting wages. On the one hand, it can create a debt overhang problem that lowers displaced workers' appetite to work since a larger fraction of earnings will go to their lenders. To attract workers, firms then have to post vacancies with higher wages (Donaldson et al., 2019). Therefore, this mechanism predicts that displaced workers with high household leverage find jobs with higher wages. On the other hand, household leverage can reduce the starting wages of the displaced workers. For instance, in one channel, household leverage creates a pressure on displaced workers because they need to service their debt (Ji, 2020). The reason is that defaulting on loans has been shown to be associated with substantial costs such as deteriorated credit scores or worsen labor market prospects.[30] Or, workers may not be willing to make adjustments in their housing consumption due to consumption commitments(Chetty and Szeidl, 2007). They may therefore decide to accept early job offers and forego later offers that are potentially better paid. Moreover, household leverage may have influence on the workers' ability to make optimal job search decisions, similar to its influence on financial decisions (Gathergood et al., 2019; Martinez-Marquina and Shi, 2021). Due to this influence, workers may neglect some of the options instead of exercising them, if household leverage directs workers' attention towards debt repayment. Hence, workers with reduced leverage may have an advantage over detecting the job options that may require directed job search. Unlike the first mechanism, these alternative mechanisms predict that displaced workers with high leverage will match with low-paying jobs.

Figure 4 provides some stylized evidence from Norway and plots household leverage

[30] Deteriorated credit scores after a default can make it harder to regain access to credit (Dobbie et al., 2020; Gross et al., 2020) and worsen labor market prospects (Bos et al., 2018; Dobbie and Song, 2015).Diamond et al. (2020) documents the non-pecuniary costs of foreclosures.

and starting wages of all displaced workers. This figure illustrates that household leverage is negatively correlated with starting wages of displaced workers. Albeit suggestive, due to confounding factors, this correlation does not answer the empirical question that these opposing effects of household leverage on displaced workers' starting wages raise.

We use the exogenous change in household leverage caused by the macroprudential policy with detailed labor market data to answer this empirical question. Figure 6 depicts the dynamic effect of household leverage on displaced workers' starting wages. This figure plots γ_k from Equation 3, where the dependent variable is a worker's wage growth between the job she is displaced from and the next job she finds. During the years before the LTV restriction, the wage growth of the treated and control groups follow parallel trends. This allows us to ascribe the change in the treatment group's wage growth after the restriction to the change in household leverage.[31] Indeed Figure 6 shows that treated workers have higher wage growth after being displaced, indicating that leverage can be detrimental to displaced workers' wage prospects.

Table 4 complements the implications of the Figure 6 with robust statistical evidence. Here, we present the results of the difference-in-differences model in Equation 2, where wage growth between job switches is the dependent variable. In Column (1), without 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. A concern may be that treated displaced workers have different education levels and that education can influence both labor market outcomes and household leverage. If so, failing to control for education will create a bias in the coefficient of interest. To mitigate this concern, we include education fixed effects in Column (3). Another concern may be related to the construction of our sample. As explained in Section 3.2, we restrict ourselves to workers who lost their jobs in mass layoffs. Such layoffs could reasonably occur due to location or industry specific shocks. If these

[31] Note that in Section 2, we document that there are no important macroeconomic or labor market related changes when the LTV restriction has introduced.

shocks also affect labor market prospects, ignoring location and industry characteristics can also generate a bias in our regressions. We therefore further saturate the model with location and industry fixed effects in Columns (4) and (5).

Ideally, one would like to compare 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. We therefore saturate the model with $Location \times Industry$ fixed effects on Column (6). In this tight specification, $d(\widehat{LTV} > 0.85)_h \times Post_t$ has a positive and statistically significant coefficient. The magnitude of the coefficient implies that treated displaced workers have a 45 percent higher wage growth rate after the policy implementation. Since the mean wage growth rate for the treated workers is -7.4 percentage points, the 45 percent higher growth implies that thanks to lower leverage, treated workers achieve a relative gain in wages of 3.3 percentage points.

One concern about the causal interpretation of our results is endogenous selection. Households who can buy a house before the policy may not be able to do so after the LTV restriction due to the down payment that the restriction requires. Therefore, the treated households before the restriction can be different than the treated households after the restriction in terms of their ability to come up with enough savings for the down payment. If this is the case, then the observed difference in the starting wages before and after the restriction can be partially driven by the difference between the treated groups generated by the restriction.

This endogenous selection of the households is expected to be strongest around the policy implementation. After the announcement of the restriction, the households who think that they cannot pay for the down payment would try to purchase a house before the implementation. Also, households with insufficient savings for the down payment but cannot purchase before the implementation have to accumulate enough savings, which can delay their purchase for some time. These two effects indicate that one way to tackle the selection problem is excluding a time period right before and right after the policy from the sample. As explained in Section 3, this is exactly what we do. By removing the 6 months before the first LTV restriction implementation, we effectively exclude the households who can time their purchase from the sample. Also, removing 18 months after the first implementation gives an opportunity for the affected households to accumulate enough savings for the down payment. Thanks to this "doughnut design", we expect to see that the endogenous selection is minimal in our setting.

We document that this is indeed the case in two ways. First, we check whether the LTV restriction alters the characteristics of the treated households in our sample.[32] To this end, we use log changes in income, wage, business income, transfers, unemployment benefits, and education level one period before the layoff as the dependent variable for difference-in-differences model in Equation 2. Confirming the effectiveness of our empirical design, the restriction does not have statistically or economically significant effects on these characteristics as shown in Table 5. In addition, incentives that property taxes create can be important for this ineffectiveness. In Norway, households enjoy lower tax rates for their primary houses comparing to the wealth tax.[33] Therefore, due to the tax advantages, households have incentives to increase the size of their real estate purchase as much as possible. This implies that when a restriction is introduced, the households still prefer purchasing a house but with a lower price, which allows the characteristics of the home buyers stay the same.[34] Moreover, the first two columns of Table 5 mitigates another concern. One argument can be that when workers observe that down payment requirements increase, they might start to look for ways to increase their earnings. This may generate a momentum that can help them in the job searching process once they are displaced. In this line of argument, our result that low-leverage job seekers find better-paid jobs could partially reflect a momentum effect. Finding that the restriction does not have an influence on previous income or wage growths

[32] Bernstein and Koudijs (2021) use a similar strategy for mortgage amortization policy in the Netherlands.
[33] For primary houses, the tax value is 25% of the housing value with a tax rate of 0.7%.
[34] We find that the LTV restriction decreases the house prices (Table A2).

mitigates this concern effectively.

Second, we homogenize the treated households across the both periods in terms of their ability to pay for the down payment. As explained before, the main reason for the endogenous selection is the changes in the treated households' ability to pay for the down payment. Thus, refining the treated groups in terms of this ability mitigates the concerns regarding the selection. First, we calculate the down payment for each home purchase using the policy threshold. Then, we remove the households who do not have enough deposits for the down payment from the pre-treatment period. Therefore, all households in this refined sample have enough savings for the down payment. Table 6 documents that the estimated coefficients in the refined sample are similar to the ones in Table 4, which indicates that our results does not suffer from selection problems.

In Table 7, we provide several robustness checks for the impact of household leverage on the starting wages. First, to show that our results do not hinge on the sample period, we change the sample starting period in the first two columns. Columns (1) and (2) reports the results where we start the sample one year earlier or later. Doing so does not change our results. Second, we remove all people who receive cash transfers greater than NOK 100,000 or have business income between 2000 and 2017, since their job search behavior may be different.[35] Columns (3) and (4) document that removing such workers does not affect our results. One concern might be that treated workers can react differently to macroeconomic conditions. If a change in the macroeconomic conditions occurs around the time the LTV restriction is implemented, the coefficient of $d(\widehat{LTV} > 0.85)_h \times Post_t$ could pick up this differential response to these conditions. Although macroeconomic conditions were stable (see Section 2), we take one step further in Column (5) and interact inflation, unemployment rate, GDP growth, and the monetary policy rate with $d(\widehat{LTV} > 0.85)_h$. Doing so increases the positive impact of leverage on displaced workers' wage growth. In Table A13, we show that all interaction terms of $d(\widehat{LTV} > 0.85)_h$ with the four macro variables are insignificant,

[35] The cash transfers can be an inheritance or a gift by parents.

indicating that the wage growth of treated employees is not differentially affected by the macro conditions. Finally, we saturate the model with $d(\widehat{LTV} > 0.85)_h \times Education$ fixed effects to verify if education affects the treated employees differently than the control group. Column (6) shows that this does not change our results either.

The main selection criterion in the construction of the treated and control groups is the cut-off value of the LTV ratio. Before the introduction of the policy, these two groups have different LTV ratios. After adoption of the mortgage restriction, treated households have lower LTV ratios than they would have chosen in an unconstrained market. This suggests that treated households, in the post-treatment period, should be expected to have LTV ratios just below the policy threshold. If true, then observations from the treatment and control groups with LTVs just below the policy threshold would make a better comparison, since they are more similar in terms of the main selection criterion. In our baseline regressions, the lower bound for the LTV ratio is 50 percent. If this value is reasonable, then narrowing the sample selection criteria from 50 percent towards to policy threshold (i.e. 85 percent) should not affect the estimated treatment effect. We demonstrate that this is the case. Figure 7 plots the coefficient of $d(\widehat{LTV} > 0.85)_h \times Post$ from Equation 2, where we include year, education, location, and industry fixed effects. The y-axis shows the coefficient of $d(\widehat{LTV} > 0.85)_h \times Post_t$ and the bars reflect the confidence 95 percent bands. Moving rightward along the x-axis, each step raises the sample's lower bound for the LTV ratio by 5 percent. Since the coefficient on $d(\widehat{LTV} > 0.85)_h \times Post_t$ remains virtually unchanged, we can alleviate any concerns that observed wage growth differences between the treated and control workers are a result of inherent differences due to the selection criterion.

Figure 6 clearly depicts that the treated and control groups have parallel trends in the outcome variable before the treatment. However, fundamental differences between the treated and control job seekers could be driving our results. We tackle this concern with a placebo test. First, we remove the observations that occurred after the LTV ratio restriction. Then, we create a dummy variable, $Placebo_t$ that takes the value of one for the two periods before the restriction and zero for the earlier periods. Moreover, we also remove the households whose LTV ratios above the threshold from the placebo post sample. This helps us to mimic exactly sample construction of the main sample.[36] After these sample adjustments, we interact $Placebo_t$ with $d(\widehat{LTV} > 0.85)_h$ as if there exists a shock at the beginning of the placebo period. If the results are driven by the differences between the treated and control groups, $d(\widehat{LTV} > 0.85)_h \times Placebo_t$ should have a significant coefficient. Table 8 shows this is not the case. Analogous to Table 4, we run regressions without controls and then add year, education, location, industry, and location×industry fixed effects consecutively. In none of the models $d(\widehat{LTV} > 0.85)_h \times Placebo_t$ has a significant and/or economically sizeable coefficients, allaying any concerns about the parallel trends assumption.

5.3 Through what mechanism does leverage affect wages?

After establishing that displaced workers with low leverage have higher wage growth, we turn to investigating through what mechanism leverage affects these workers' starting salaries. To better understand this, we start by inspecting job search behavior. First, we look at the extent to which the time that displaced workers spend unemployed depends on their leverage. Next, we investigate displaced workers' debt utilization after the displacement. Then, we analyze whether household leverage has an impact on employer and occupation characteristics. Finally, we provide several heterogeneity tests that support the mechanism that we reveal.

High leverage increases the probability of default. Following a negative income shock, such as a job loss, workers with higher leverage may find it harder to avoid the default. Hence, they may be willing to accept early job offers to avoid the default. To test this hypothesis that leverage shortens the unemployment spells of high-leverage workers, we use the employee-employer register and calculate the unemployment spells. Then, we enlist

[36] In Section A3 we use a simulation exercise and show that this removal does not create a bias in our estimations.

the difference-in-differences model in Equation 2, now with the log of displaced workers' unemployment spells, measured in days, as the dependent variable. First two columns of Table 9 provides the results. Column (1) indicates that job seekers with lower leverage have 60 percent longer unemployment spells, an increase of 79 days. In Column (2), we saturate the model with year, education, location, and industry fixed effects to control for time effects, individual characteristics, and firms' labor demand. These fixed effects do not change our results qualitatively and in fact marginally increases the size of the measured effect.

One channel through which household leverage affects the job search behavior could be its influence on debt usage during the unemployment spell. Literature has documented that access to credit during the unemployment spell affects the job search behavior and labor market outcomes (Herkenhoff et al., 2016; Herkenhoff, 2019). If leverage before the job displacement affects the debt utilization during the unemployment spell, then this ex-post debt utilization can be important for the findings we document. 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 use this variable as the dependent variable. These columns indicate that the LTV ratio restriction does not affect the ex-post debt utilization as $d(\widehat{LTV} > 0.85)_h \times Post_t$ has insignificant coefficients in both columns.

After documenting that the household leverage before the displacement is important for having longer unemployment spells, now we ask whether lower leverage helps displaced workers find better employers? To address this question, we follow Abowd et al. (1999) (AKM) and estimate the firm wage premium, i.e., the wages that firms pay after controlling for employee characteristics, for all firms in our sample. To this end, we regress the log of wages on employer, employee, and year fixed effects as well as employee characteristics.[37] Then, we use the estimated firm fixed effects as firm wage premia. To understand whether having lower leverage helps displaced workers find a better match, we take the difference

[37] We remove the firms with fewer than 5 movers to reduce the labor mobility bias. In the AKM sample we also discard job seekers from our estimation sample.

between wage premiums of new and old employers of workers, $\Delta Firm\ Wage\ Premium$, and use it as the dependent variable in our difference-in-differences setting. Last two columns of Table 9 establish that treated workers experience a statistically significant increase in their new employers' wage premiums. Even though the effect is not significant without any fixed effects, once we include year, education, location, and industry fixed effects, the coefficient becomes highly statistically significant. 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.

A reduction in household leverage caused by the LTV restriction might alter the scope of job search. To appreciate the impact that leverage has, we analyze three measures of job search scope: occupation, industry, and location. Specifically, we use our difference-in-differences model to test whether workers with low leverage are more likely to make a change along any of these three dimensions. The results in Table 10 indicate that lower leverage induces workers to broaden their job search along some margins. In Table 10, the odd-numbered columns do not include any fixed effects and the even-numbered columns include year, education, location, and industry fixed effects. In the first two columns, the dependent variable is a dummy, which is 1 if a worker has a different occupation in her new firm. Switching to other occupations and/or industries can be helpful for displaced workers to reduce the scarring effect of job displacements (Ruhm, 1991; Stevens, 1997; Arulampalam, 2001). 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 lower leverage displaced workers also have a 15 percent higher probability of finding their new jobs in other industries than before their displacement.Geographical labor mobility, an important determinant of labor supply, has been shown to be adversely affected when a household has negative home equity. We find that leverage, when isolated from a household's home equity position, does not affect displaced workers' geographic labor mobility. This complements the findings of Bernstein (2020) and Gopalan et al. (2020).

Together, our findings provide a clear picture of the mechanism through which a reduction in leverage increases workers' starting wages. Thanks to lower leverage, displaced workers can afford to stay unemployed for longer duration. Also, they are able to find jobs in firms with higher wage premium. Moreover, they can broaden their job search reach by switching to other occupations and/or other industries. Intuitively, we expect this mechanism to be stronger and the observed effect on starting wages larger for job seekers who are in a position to benefit more from improved matching. To verify this, we again run wage growth regressions for displaced workers while splitting up our sample using three different criteria. First, we assign households to either a lower or higher age half. Younger people are expected to exploit the opportunity of a better job search more, since they can use the longer spells to make investments to their skills and it is easier for them to switch to other occupations and industries. The first two columns of Table 11 confirm that the increase in wage growth is around 70 percent for workers younger than 33, substantially higher than in the full sample. For the older job seekers the effect is insignificant. Next, we split up job seekers by the duration of their 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 development 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 better job search. Columns (3) and (4) support this intuition. The effect of having low leverage is stronger for workers with a below median tenure. Finally, we break down our sample into workers whose highest educational attainment is upper secondary school or lower, and those who have undergraduate degree. Higher education can facilitate the process of switching to other occupations and industries. Moreover, Eriksson and Rooth (2014) have documented for Sweden that longer unemployment spells diminish employers' return rates to job applications for medium and low skill jobs, suggestive of a negative correlation between unemployment spells and starting wages for workers with low education. In line with Eriksson and Rooth (2014) and our intuition, we report that the rise in wage growth is larger for higher educated workers.

Discussions about LTV restriction policies point out that they affect the households with lower income more strongly, since paying for the down payment is more demanding for such households (Van Bekkum et al., 2019). Due to the down payments that the LTV restrictions introduce, low-income households can change their consumption patterns, which may not be optimal from a welfare point of view. However, for workers from low-income households, a reduction in leverage can generate higher improvements in their starting wages since a reduction in deb-related payments creates a relatively larger cash release. To this end, we divide our sample into three groups with respect to the income levels. The first three columns of Table 12 document that the improvement in starting wages is significantly stronger for workers from low-income households. This finding indicates that even though an LTV ratio restriction can affect the low-income household negatively during the home-purchasing process, it allows them to improve their wages.

One robust finding on wages is that female workers earn less than their male counterparts. In addition to the other factors, differences in risk-aversion or in salary demands can contribute to the gender pay gap (Blau and Kahn, 2017; Roussille, 2020; Cortés et al., 2021). These findings imply that the impact of a reduction in leverage on starting wages can be affected by the worker's gender. For instance, a worker with higher risk-aversion can reduce her job search duration by accepting an earlier job offer, even though she may receive an offer with a higher salary if she was on the labor market. Or, due to higher leverage a worker can reduce her ask salary to increase the chances of getting a job offer. To test how worker's gender differentiates the impact of a reduction in leverage on starting wages, we split our sample into two with respect to worker's gender. The last two columns of Table 12 clearly show that the positive effect of the reduction in leverage on starting wages is stronger for female workers, which complements the findings of (Roussille, 2020; Cortés et al., 2021). Overall, our results suggest that a reduction in leverage particularly improves the starting wages of disadvantaged workers.

5.4 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 13. 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 saturating the model with year, education, location, industry, and location×industry fixed effects. Together, these findings establish that the increase in wage growth is robust and not short-lived.

Finally, we consider the treated workers' wage volatility. Section 5.3 established that lower leverage facilitated employees switching to other occupations and industries. Shifting to other occupations and/or other industries may increase wage volatility, for example due to a lack of appropriate experience in these new occupations or industries. If, however, matching *quality* improves thanks to lower leverage, then we would expect to observe that lower-leverage job seekers have lower wage volatility after the LTV 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 Equation 2. Table 14 makes clear that lower-leverage households have significantly lower wage volatility. In addition to the increase in wage growth, lower leverage thus further improves labor market outcomes by making the wages more stable. One interpretation of this finding can be that thanks to the lower leverage, workers have a better match in the labor market, which reduces their wage volatility.

6 Discussion

Household leverage is an extensively studied driver of the economy. Spurred by the effects of the 2008-2009 financial crisis, academics and policymakers have since attempted to design and evaluate policies to mitigate the undesirable consequences of household leverage for the economy. Labor market outcomes and job search are both key variables for the economy as well. Empirical evidence on the relationship between households' leverage and Labor market outcomes and job search is limited, however.

Difficulties in obtaining high-quality register data and the fact that decisions on jobs and debt are taken simultaneously are explanations for our incomplete understanding of the interrelationship between labor market outcomes and debt. Our research strategy attempts to overcome these challenges and make an advance in the identification of household leverage's importance for labor market outcomes. We exploit the introduction of a macroprudential policy in Norway that exogenously reduces households' leverage through a maximum loan-to-value ratio restriction for home purchases. Using data for the full adult Norwegian population and information on mass layoffs, the policy change permits us to causally identify the impact of household leverage on labor market outcomes.

Our work is closely related to the literature that studies the determinants of labor supply and job search. We show that limiting household leverage generates persistent and positive effects on displaced workers' wages. We make several specific contributions. First, we confirm that an LTV ratio restriction is highly effective in constraining borrowing by households. Second, we causally identify the effect of leverage on starting wages of displaced workers and find that displaced workers have a 3.3 percentage points higher wage following a 25 percent decline in their debt-to-income ratio. Third, displaced workers, following a reduction in debt engage in longer job search and find jobs at firms that pay higher wages, switches into new occupations and different industry also become more likely. We argue this is made possible when lower debt service reduces the pressure on displaced workers to quickly accept job offers. Firm-specific wage premia explain 20 percent of the gain in starting wages. Moreover, younger, higher educated workers with shorter job tenure experience stronger effects on their wages. Finally, the identified effects are not short-lived and rather appear to be reinforced over time.

Our results also relate to studies of the effects of credit access and household leverage on the economy, and inform the debate on the effectiveness of macroprudential policies. Research has mainly focused on the effectiveness of macroprudential policies in restraining credit and housing markets. These policies are often most binding for young households without equity and can lead to postponement of households' home purchasing (Fuster and Zafar, 2016; Van Bekkum et al., 2019) or reductions in housing market access (Tzur-Ilan, 2020). Our results indicate that macroprudential policies also bring about economically significant and sustained benefits for low income, highly indebted, young, and female workers. This suggests that policymakers should internalize a broader range of beneficial effects when designing macroprudential policies.

The findings in this paper potentially have bearing on other policy choices that affect what leverage levels are efficient in an economy. When household leverage influences the career and income path of workers, then policies to alter debt may have knock-on effects on wealth, health, family conditions, offspring or even political choice. The existence and size of such downstream effects would in turn affect the social value of household leverage-reducing policies.

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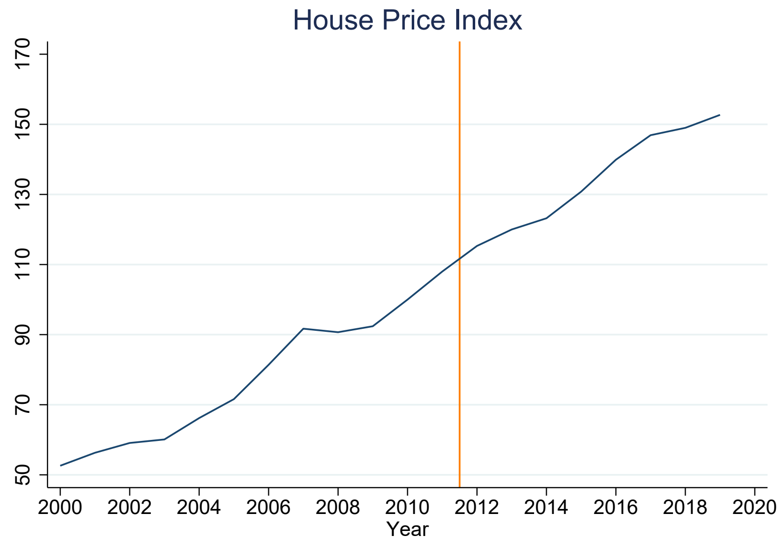
Figure 1: **Household debt and house prices**

This figure shows the house price index (Figure 1a) and household credit to GDP ratio (Figure 1b) in Norway between 2000 and 2019. The orange line indicates the implementation date of LTV ratio restriction.

(a)



(b)

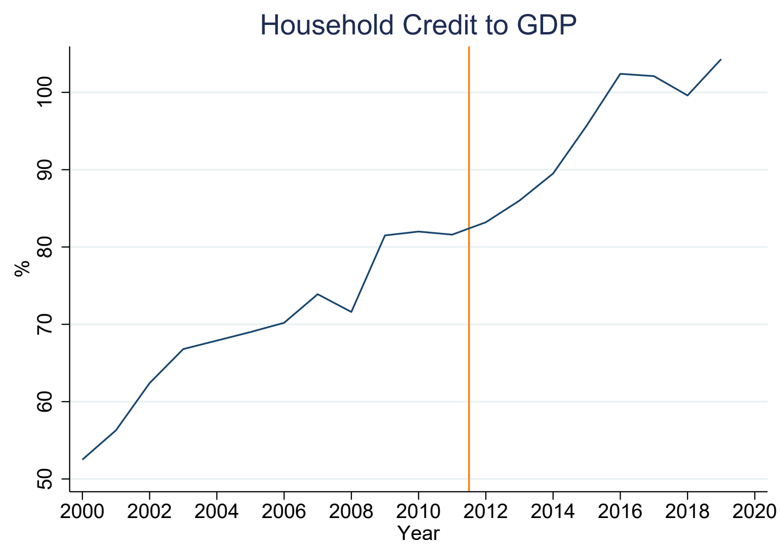


Figure 2: **Receiver Operating Characteristic curve**

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.

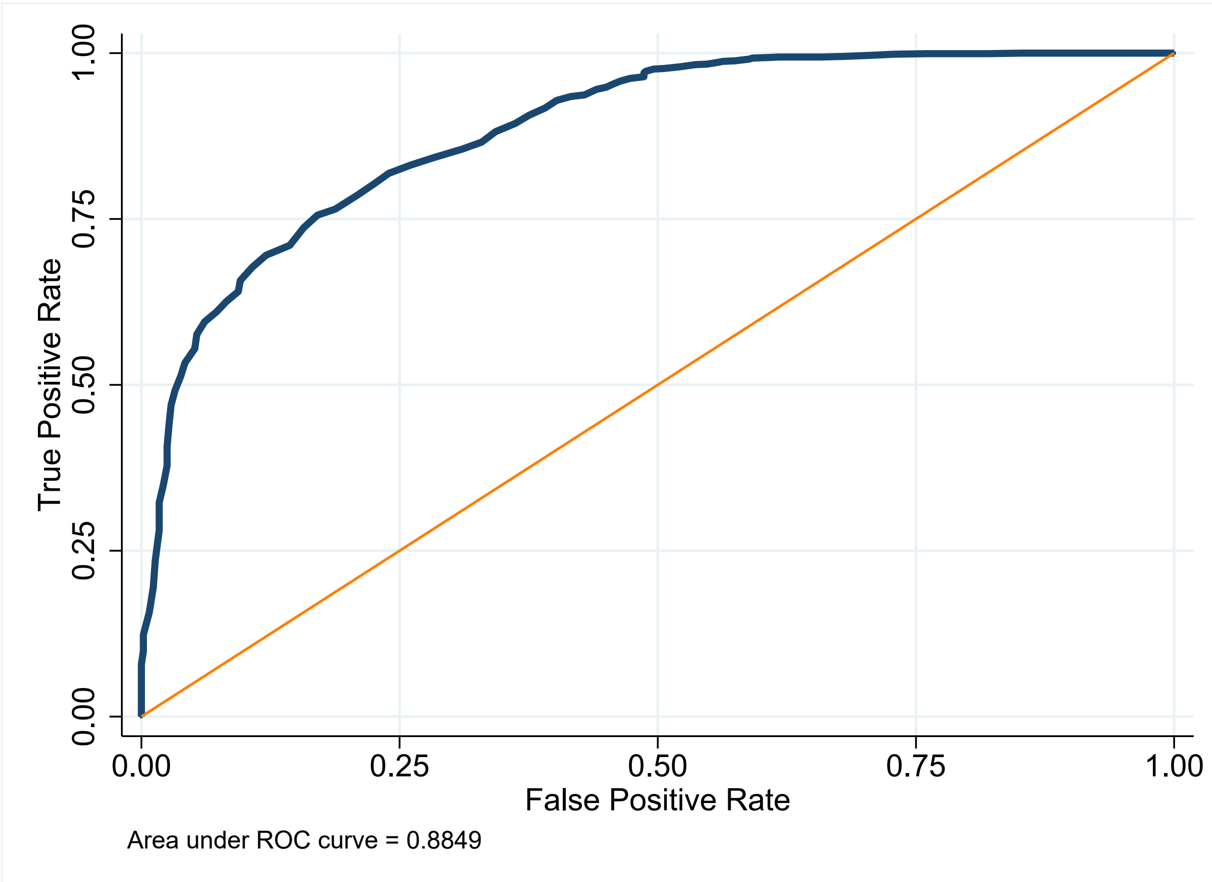


Figure 3: **Variable importance**

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.

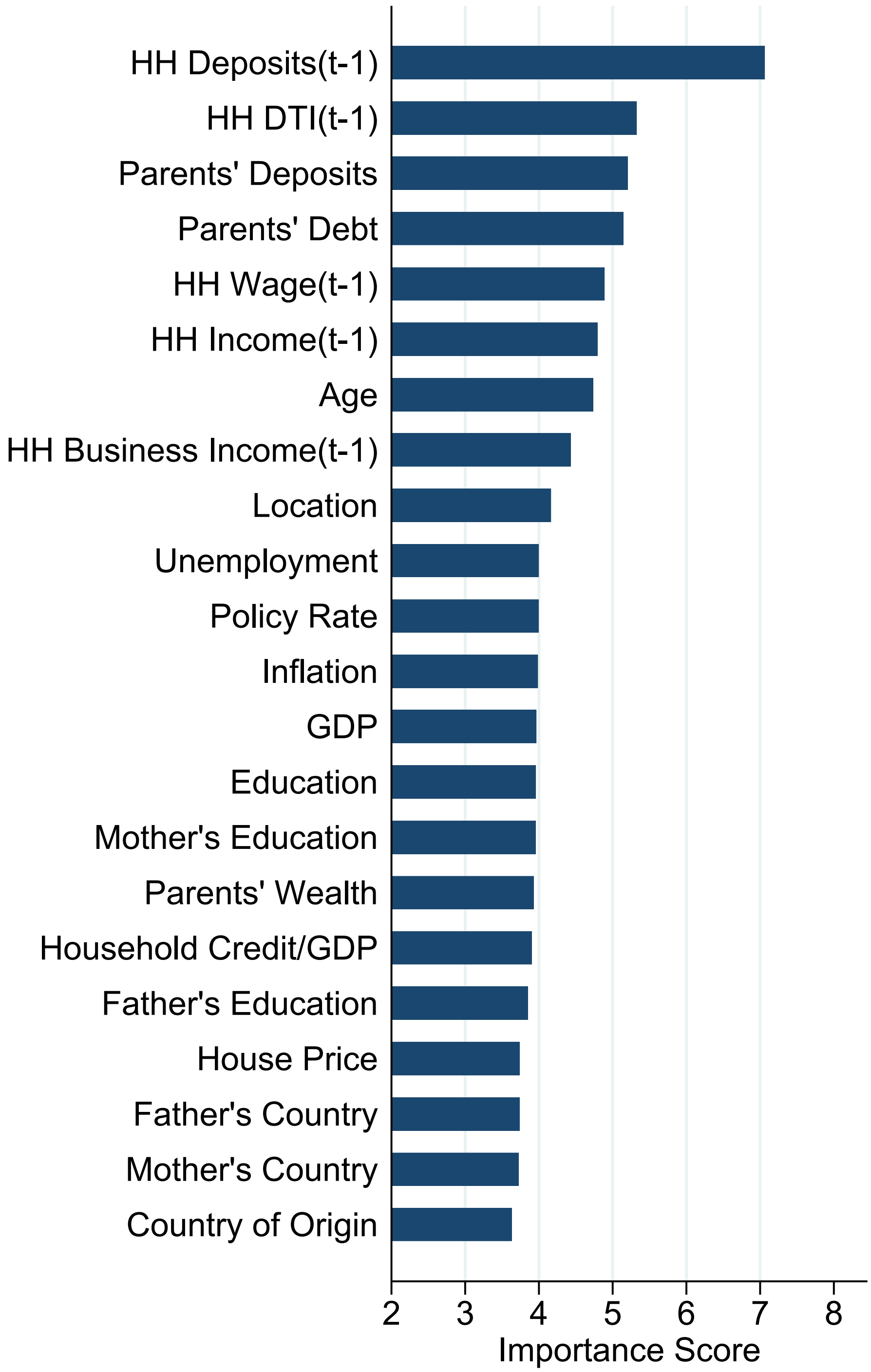


Figure 4: **Household debt and labor market outcomes**

This figure shows that debt-to-income (DTI) ratio is negatively correlated with starting wages of displaced workers. The sample consists of workers who lost their jobs due to mass layoffs.

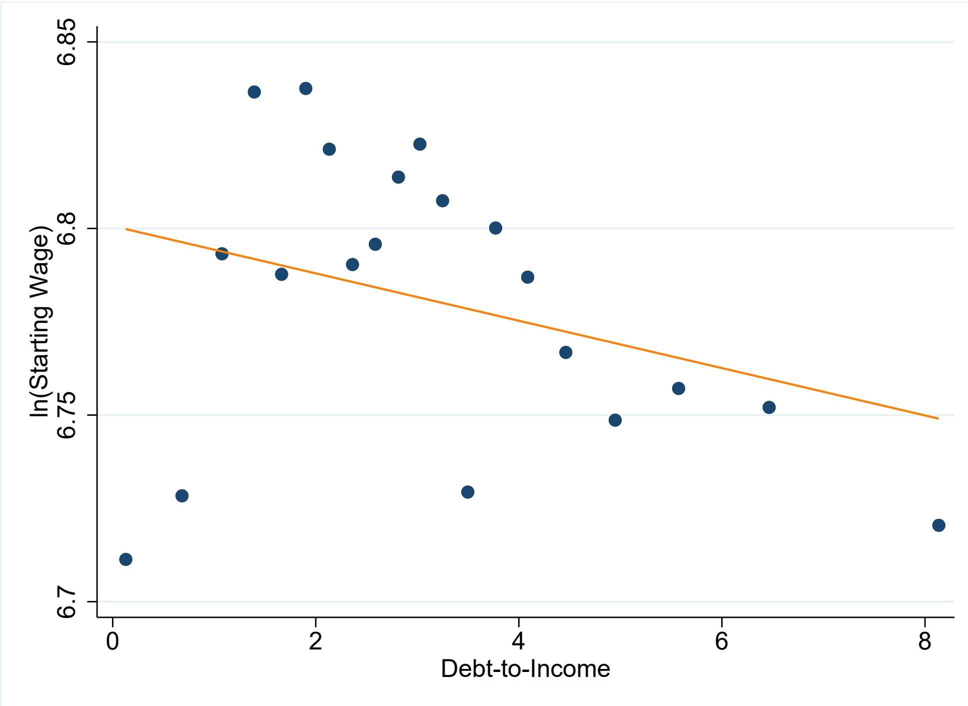


Figure 5: **Dynamic impact of macroprudential policy on DTI ratio**

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 βs 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.

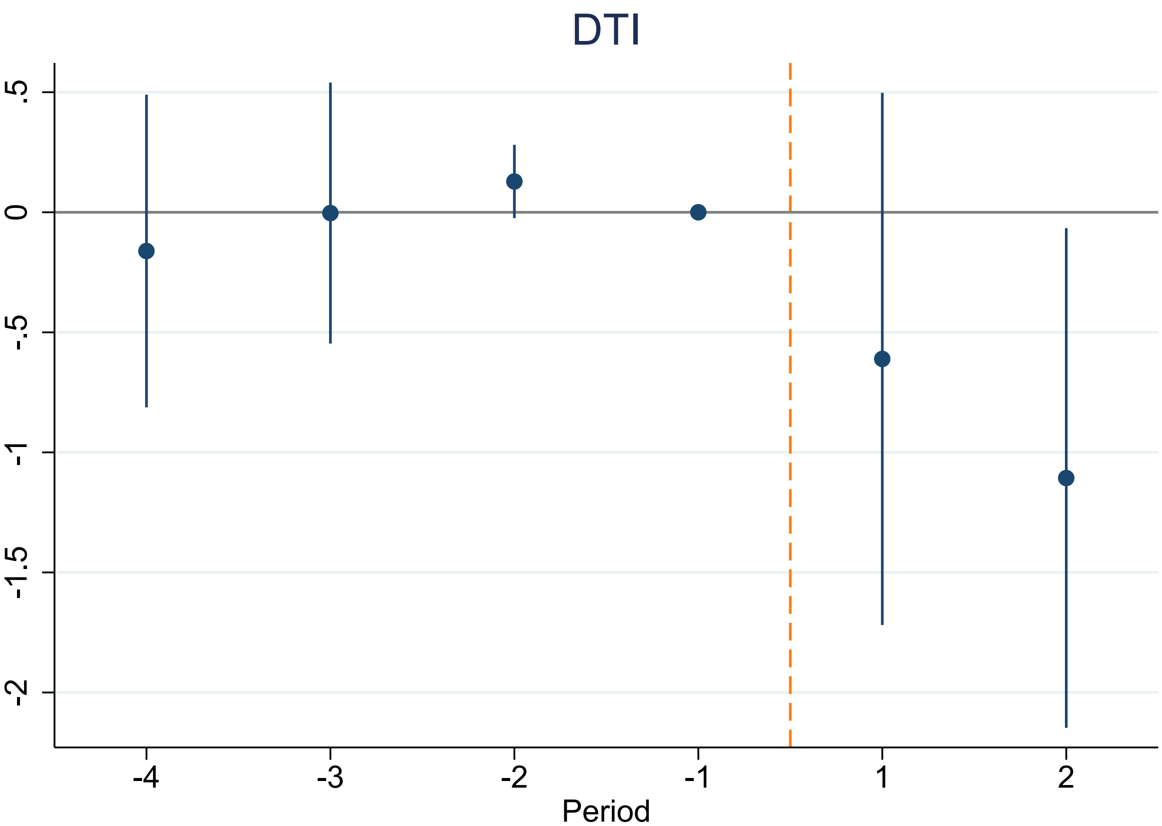


Figure 6: **Dynamic impact of policy on wage growth**

This figure shows the dynamic effect of the LTV policy on the wage growth of 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 βs 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.

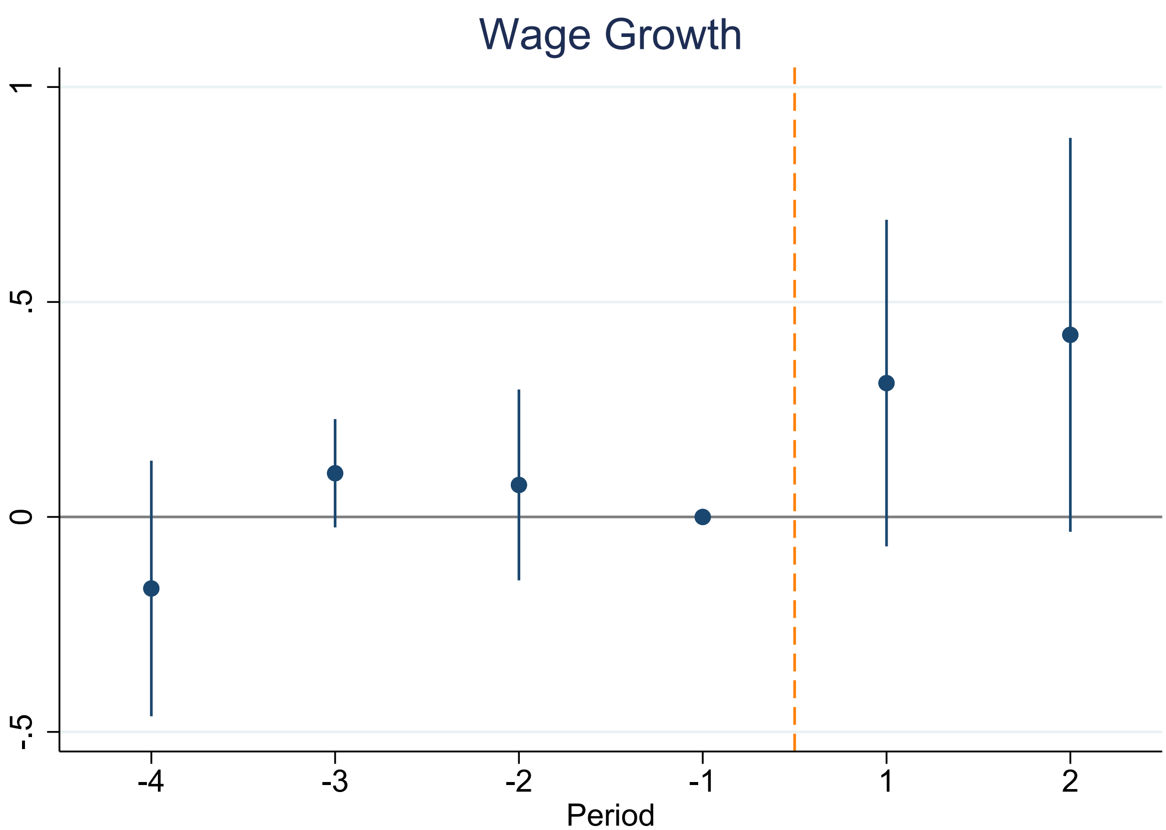


Figure 7: **Different thresholds for LTV lower bound**

This figure provides a robustness check for the effect of LTV policy on the wage growth of 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. $d(\hat{LTV} > 0.85)$ 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.

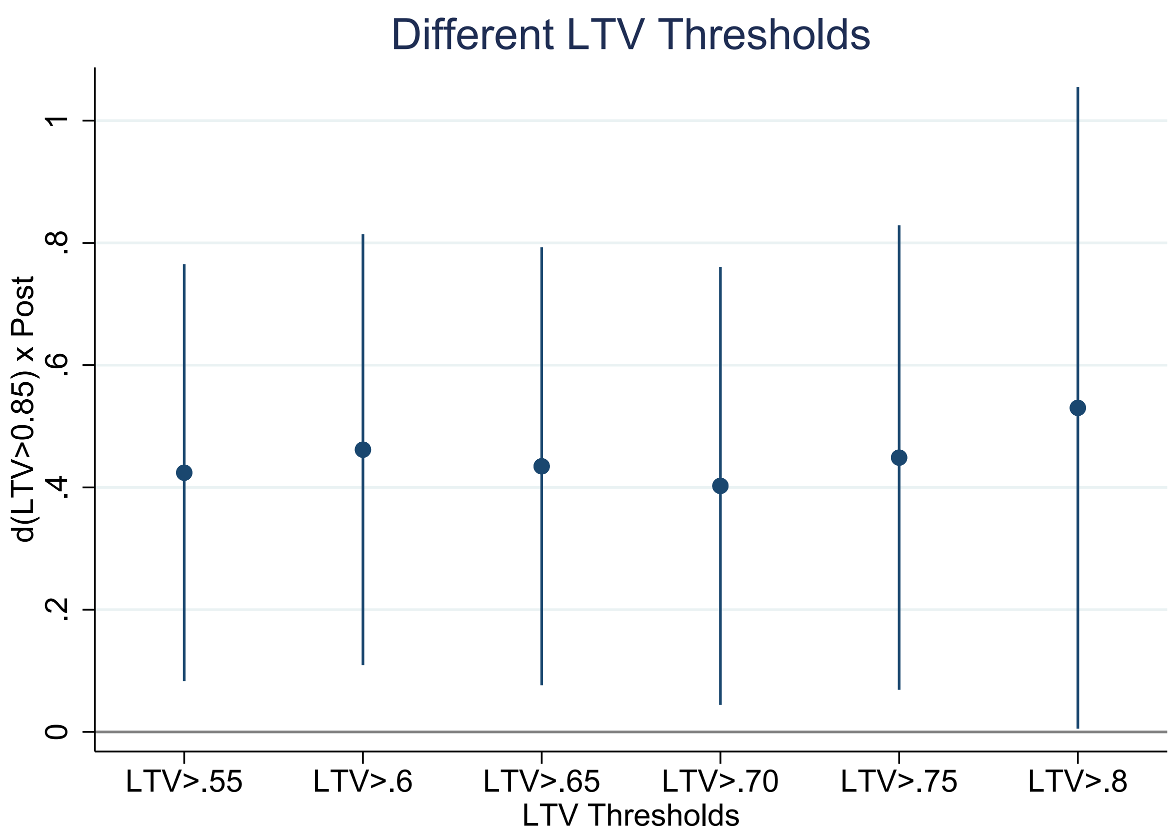


Table 1: **Summary statistics**

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
Δ ln(Ex $-$ Post Debt)	0.09	0.98	-0.05	0.00	0.07
Δ ln(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				

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

This table compares the variables used in the prediction model for the treated and control groups. $d(L\hat{T}V < 0.85)$ indicates that the household is predicted to be control and $d(L\hat{T}V \geq 0.85)$ 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($\widehat{LTV}$ ≥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
$\text{Debt-to-Income}_{t-1}$	2.58	1.54	1.04	4.20
Deposits_{t-1}	869.19	156.09	713.10	28.61
$\text{Business Inc.}_{t-1}$	54.81	22.91	31.90	2.05
$\text{Parents' Debt}_{t-1}$	1898.84	1987.59	-88.75	-0.46
$\text{Parents' Dep.}_{t-1}$	1458.99	600.92	858.06	10.18
$\text{Parents' Wealth}_{t-1}$	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			

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 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. $d(L\hat{T}V > 0.85)$ 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. 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) × Post	-1.094***	-1.058***	-1.138***	-1.108***	-1.148***	-1.017**
	(0.372)	(0.348)	(0.394)	(0.358)	(0.353)	(0.401)
d($\widehat{LTV}$>0.85)	0.895***	0.858***	1.192***	1.206***	1.188***	1.193***
	(0.284)	(0.256)	(0.304)	(0.268)	(0.234)	(0.250)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × 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					

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

This table documents the effect of the LTV ratio policy on the wage growth of 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 ($d(L\hat{T}V > 0.85)$). *Post* equals to 1 for the years 2012 and 2013 and equals to 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) × Post	0.335**	0.343**	0.482***	0.495***	0.449**	0.390*
	(0.154)	(0.153)	(0.161)	(0.158)	(0.160)	(0.187)
d($\widehat{LTV}$>0.85)	-0.102***	-0.109***	-0.129***	-0.125***	-0.123***	-0.120***
	(0.010)	(0.027)	(0.033)	(0.036)	(0.031)	(0.028)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × 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					

Table 5: **Impact of the policy on characteristics of the treated group**

This table documents that the LTV ratio policy does not change the characteristics of the treated group. 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. All dependent variables are lagged by one period (i.e. one period before the layoff). $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 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.

Previous	Inc.	Wage	Buss. Inc.	Trans.	Unemp. Ben.	Educ.
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	0.042	0.061	0.183	-0.311	-0.043	0.031
	(0.191)	(0.195)	(0.141)	(0.426)	(0.243)	(0.071)
d($\widehat{LTV}$>0.85)	0.064	0.060	-0.050	0.022	0.105**	0.004
	(0.055)	(0.055)	(0.055)	(0.085)	(0.047)	(0.019)
Fixed Effects:						
Year FE	✓	✓	✓	✓	✓	✓
Education FE	✓	✓	✓	✓	✓	
Location FE	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓
Obs.	1,833	1,833	1,833	1,833	1,833	1,876
R^2	0.110	0.109	0.080	0.120	0.093	0.083
Mean(Dependent Var.)	0.361	0.369	0.092	0.333	0.050	0.777

Table 6: **Removing treated households who cannot pay for the down payment before the policy**

This table documents that removing the households who are not able to pay for the down payment does not affect the impact of the LTV ratio policy on the wage growth of 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 households who 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. $d(L\hat{T}V > 0.85)$ 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. 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) × Post	0.289*	0.291*	0.444**	0.443**	0.373*	0.265
	(0.156)	(0.150)	(0.168)	(0.184)	(0.213)	(0.250)
d($\widehat{LTV}$>0.85)	-0.055*	-0.057*	-0.073*	-0.057	-0.056	-0.036
	(0.031)	(0.032)	(0.040)	(0.042)	(0.048)	(0.061)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	941	941	927	927	927	927
R^2	0.014	0.028	0.142	0.161	0.181	0.294
Mean(Wage Growth)	-0.074					

Table 7: **Robustness checks for wage growth**

This table documents the effect of the LTV ratio policy on the wage growth of displaced workers. 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 $d(L\hat{T}V > 0.85)$ with four main macro variables: inflation, unemployment, GDP growth, and monetary policy rate. Column (6) interacts $d(L\hat{T}V > 0.85)$ with education levels. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 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)
	2005	2007	No Transf.	No Bus. Inc.	Macro	Education
d($\widehat{LTV}$>0.85) × Post	0.426**	0.449**	0.409**	0.430**	0.983***	0.423*
	(0.183)	(0.186)	(0.180)	(0.183)	(0.329)	(0.205)
d($\widehat{LTV}$>0.85)	-0.108**	-0.096***	-0.088**	-0.126***	-5.076	0.703***
	(0.040)	(0.033)	(0.038)	(0.037)	(3.510)	(0.184)
Fixed Effects:						
Year FE	✓	✓	✓	✓	✓	✓
Education FE	✓	✓	✓	✓	✓	✓
Location FE	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓
Treated × Macro Var.					✓	
Treated × Education FE						✓
Obs.	2,016	1,614	1,649	1,737	1,833	1,833
R^2	0.124	0.124	0.138	0.122	0.124	0.171
Mean(Wage Growth)	-0.074					

Table 8: **Placebo test for wage growth**

This table provides a placebo test for the effect of the LTV ratio restriction on the wage growth of displaced workers. Each column uses worker level data between 2006 and 2010. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. The households that are predicted to be treated and obtain mortgages with LTV ratios higher than threshold are removed. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. $d(\hat{LTV} > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Placebo* equals to 1 for the years 2009 and 2010 and equals to 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) × Placebo	0.014	0.017	-0.015	-0.033	-0.039	-0.152
	(0.111)	(0.106)	(0.128)	(0.136)	(0.131)	(0.168)
Placebo	0.016	-0.000	0.041	0.034	0.027	0.045
	(0.072)	(0.067)	(0.077)	(0.092)	(0.117)	(0.137)
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,050	1,050	1,029	1,029	1,029	1,029
R^2	0.000	0.002	0.099	0.114	0.169	0.259
Mean(Wage Growth)	-0.074					

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 $ln(Unemployment\ Spell)$ as the dependent variable. Columns (3) and (4) use $\Delta ln(Ex - Post\ Debt)$ (i.e. log change in household level debt after the year of displacement) as the dependent variable. Columns (5) and (6) use $\Delta Firm\ Wage\ Premium$ (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). $d(L\hat{T}V > 0.85)$ 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. 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)		Δ ln(Ex-Post Debt)		Δ ln(Firm Wage Pre.)	
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	0.608***	0.567*	-0.067	-0.114	0.004	0.058**
	(0.205)	(0.281)	(0.244)	(0.313)	(0.023)	(0.027)
d($\widehat{LTV}$>0.85)	0.019	0.017	-0.023	-0.063	0.029***	0.009
	(0.091)	(0.110)	(0.024)	(0.057)	(0.007)	(0.008)
Fixed Effects:						
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	

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

This table documents the effect of the LTV ratio policy on the job search broadness 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. $d(L\hat{T}V > 0.85)$ 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. 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) × 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)
Fixed Effects:						
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	

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

This table documents the heterogeneous effect of the LTV ratio policy on the wage growth 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 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 33. 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. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 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	
	(1)	(2)	(3)	(4)	(5)	(6)
	Low	High	Low	High	Low	High
d($\widehat{LTV}$>0.85) × Post	0.700***	0.126	0.609**	0.433	0.101	0.402**
	(0.210)	(0.277)	(0.227)	(0.423)	(0.260)	(0.173)
d($\widehat{LTV}$>0.85)	-0.195**	-0.024	-0.160**	-0.054	-0.161***	-0.026
	(0.069)	(0.049)	(0.072)	(0.040)	(0.036)	(0.030)
Fixed Effects:						
Year FE	✓	✓	✓	✓	✓	✓
Education FE	✓	✓	✓	✓		
Location FE	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓
Obs.	1,044	789	866	967	419	882
R^2	0.170	0.219	0.159	0.195	0.096	0.062
Mean(Wage Growth)	-0.074					

Table 12: **Heterogeneous effects of policy on wage growth**

This table documents that the effect of LTV ratio policy on the wage growth of displaced workers is stronger for workers with low income and female 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)-(3) split the sample in terms of worker income levels. Column (1) uses workers whose income lower than the sample's 25^{th} percentile. Column (2) uses workers whose income are between sample's 25^{th} and 50^{th} percentile. Column (3) uses workers whose income are higher than sample's 75^{th} percentile. Columns (4) and (5) splits the sample with respect to workers' gender. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 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	Income			Gender	
	(1)	(2)	(3)	(4)	(5)
	Low	Medium	High	Male	Female
d($\widehat{LTV}$>0.85) × Post	0.833*	0.268	0.193	0.233	0.735*
	(0.475)	(0.264)	(0.244)	(0.152)	(0.384)
d($\widehat{LTV}$>0.85)	-0.209***	-0.102*	-0.044	-0.119*	-0.122*
	(0.061)	(0.052)	(0.058)	(0.059)	(0.064)
Fixed Effects:					
Year FE	✓	✓	✓	✓	✓
Education FE	✓	✓	✓	✓	✓
Location FE	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓
Obs.	432	911	490	1,022	811
R^2	0.312	0.176	0.261	0.156	0.228
Mean(Wage Growth)	-0.074				

Table 13: **Impact of policy on 4-year wage growth**

This table documents the effect of the LTV ratio policy on the wage growth of displaced workers. 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 after displacement for 4 years. $d(\hat{LTV} > 0.85)$ 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. 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) × Post	0.257***	0.259***	0.246**	0.220*	0.182**	0.201*
	(0.061)	(0.066)	(0.113)	(0.116)	(0.080)	(0.106)
d($\widehat{LTV}$>0.85)	0.003	0.002	-0.005	-0.008	-0.006	-0.012
	(0.036)	(0.037)	(0.036)	(0.043)	(0.031)	(0.033)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,856	1,856	1,815	1,815	1,815	1,815
R^2	0.010	0.012	0.092	0.104	0.115	0.189
Mean(Wage Growth)	0.182					

Table 14: **Impact of policy on households' wage volatility**

This table documents the effect of the LTV ratio policy on the displaced workers' wage volatility. 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 the wage volatility of workers. Wage volatility is calculated by taking standard deviation of annual wages for four years after the job displacement. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 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 Volatility					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	-26.274***	-26.846***	-32.215**	-28.707*	-24.719*	-30.496**
	(5.917)	(7.609)	(15.242)	(15.901)	(12.988)	(13.655)
d($\widehat{LTV}$>0.85)	1.033	1.294	4.282	5.332	5.183*	4.138
	(3.270)	(3.301)	(3.211)	(3.697)	(2.635)	(2.951)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,869	1,869	1,828	1,828	1,828	1,828
R^2	0.008	0.009	0.154	0.165	0.178	0.222
Mean(Wage Volatility)	82.757					

Appendix

A1 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 A5 shows γ_k from Equation 3 where we use the LTV ratio as 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 A5. 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 on average equal LTV ratios.

The treatment effect we find is larger than what other studies, like Van Bekkum 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 exception 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 A6. 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 Bekkum et al., 2019; Aastveit et al., 2020) that LTV constraints reduce mortgage size (Figure A6a) and the cost of homes treated households buy (Figure A6b). A tighter borrowing constraint does not reduce treated households' liquidity by draining deposits Figure A6c. Table A2 indicates that treated household take mortgages that are NOK 603,000 smaller mortgages to pay for homes that are NOK 503,000 cheaper.[38] In line with the lack of decline in deposits, we find that the LTV restriction has a similar negative impact on household leverage when it is calculated as (Total Debt - Deposits)/Income (Table A4).

Finally, we look into the policy's influence on households' cash flow. With smaller mortgages, we expect interest payments to decrease mechanically. A reduction in risk may also cause banks to charge a lower risk premium (Elul et al., 2010; Fuster and Willen, 2017; Ganong and Noel, 2020; Gupta and Hansman, 2020). Figure A7 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 amortizations, 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.

A2 Random Forest algorithm

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

As explained in Section 3, we use several population registers. Merging these registers, we obtain the following variables: income, wage, deposits, debt, unemployment benefits, business income, age, education, location, immigration status. Our date set allows us to observe the parents identifiers. Thus, we include parents' income, wealth, deposits, debt, education, and immigration status.

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

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 the household's main earner's information. 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 the 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 true positive rate (proportion of treated units that are correctly identified) on the y-axis, and 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 true positive rate than false positive rate as we lower the probability threshold. In other words, closer a ROC curve 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.[39] 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.

A3 Placebo test

As explained in Section 4, we remove the households who 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, thus they should 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 who are not affected by the restriction. However,

[39] We plot the variable importance that uses built in function in Figure A8.

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 the starting wages.

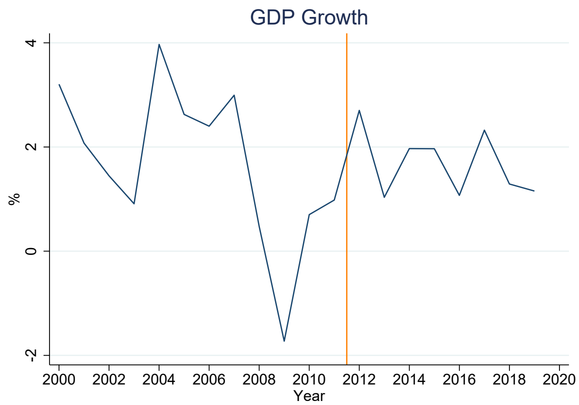
To mitigate these concerns, we adopt a conservative strategy in our placebo test reported in Table 8. Namely, we remove the households who obtain mortgages with LTV ratio above the threshold from the placebo post period. Doing this effectively removes the households who would not be affected by the policy 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 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 A9 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.

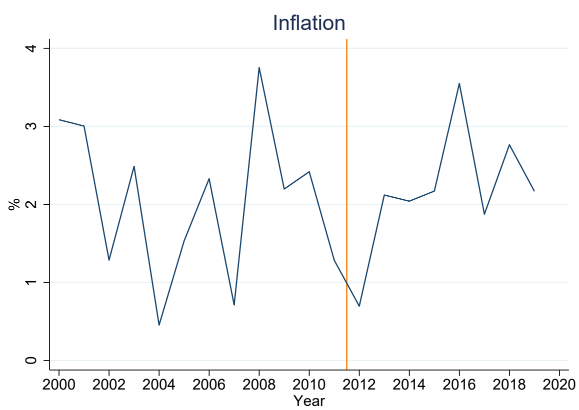
Figure A1: **Macroeconomic conditions**

This figure shows the macroeconomic conditions in Norway between 2000 and 2020. Figure A1a plots GDP growth, Figure A1b plots unemployment rate, Figure A1c plots inflation, and Figure A1d plots monetary policy rate. The orange line indicates the date of the LTV ratio restriction.

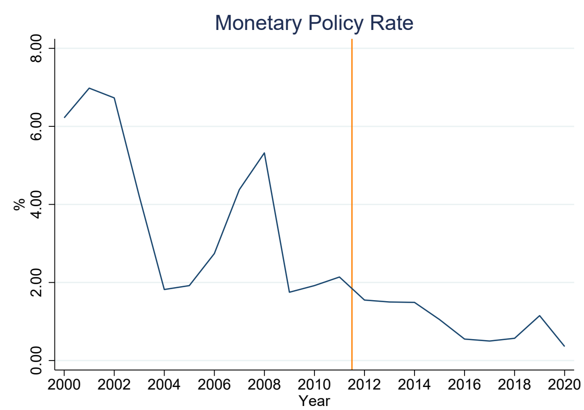


(a)

(b)



(c)



(d)

Figure A2: **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.

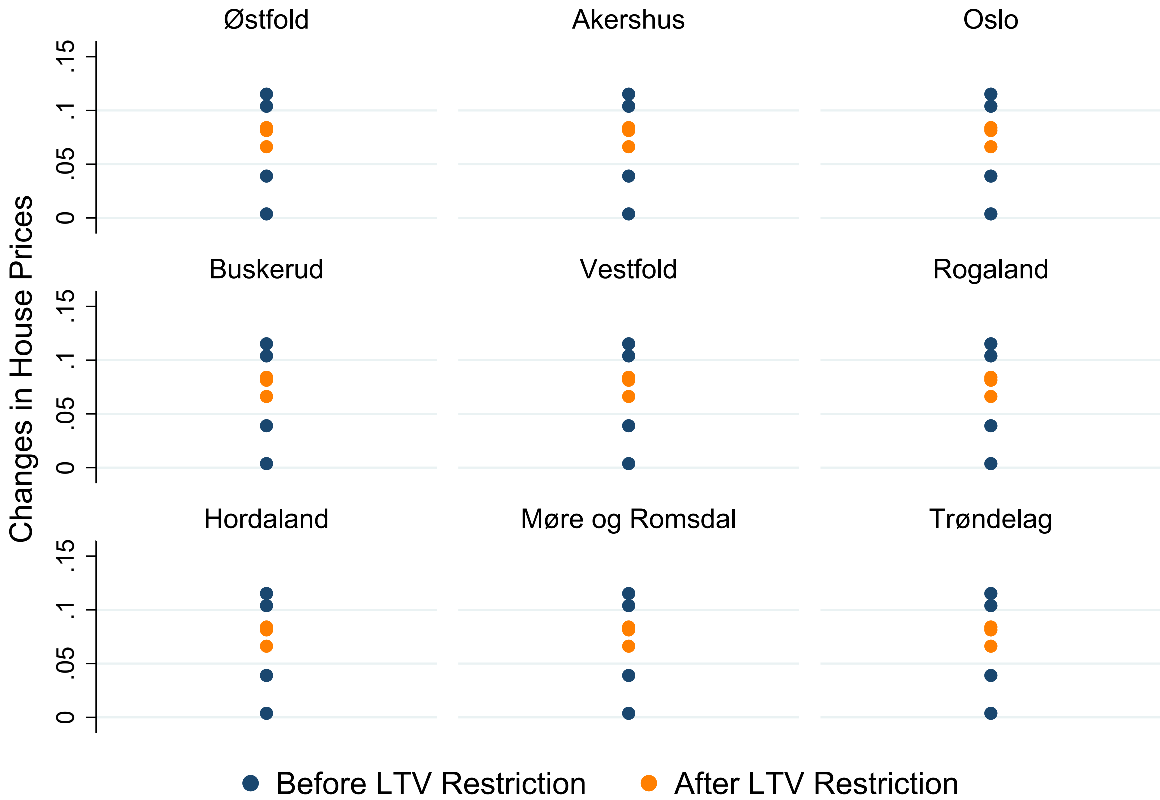


Figure A3: **Regional house prices**

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

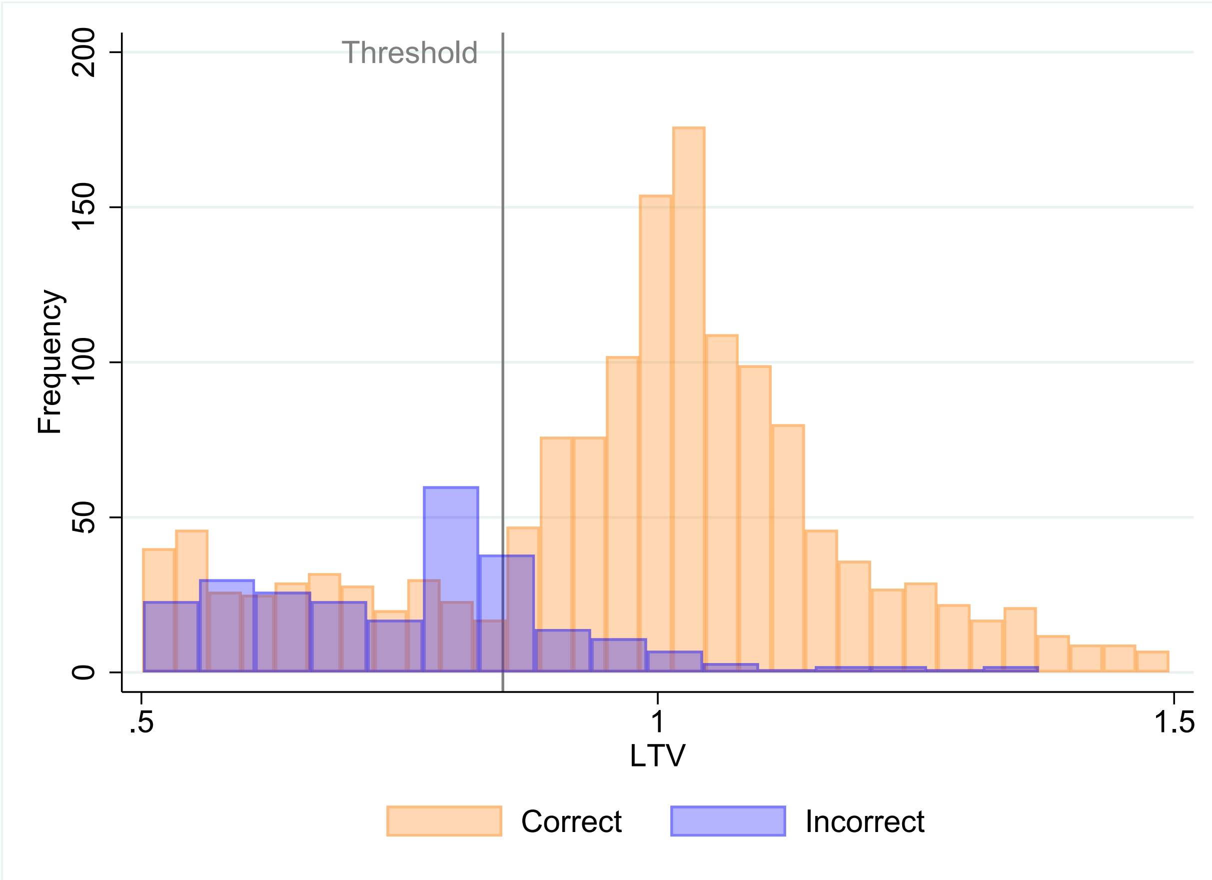


Figure A4: **Introduction of the macroprudential policy and house transaction volume**

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

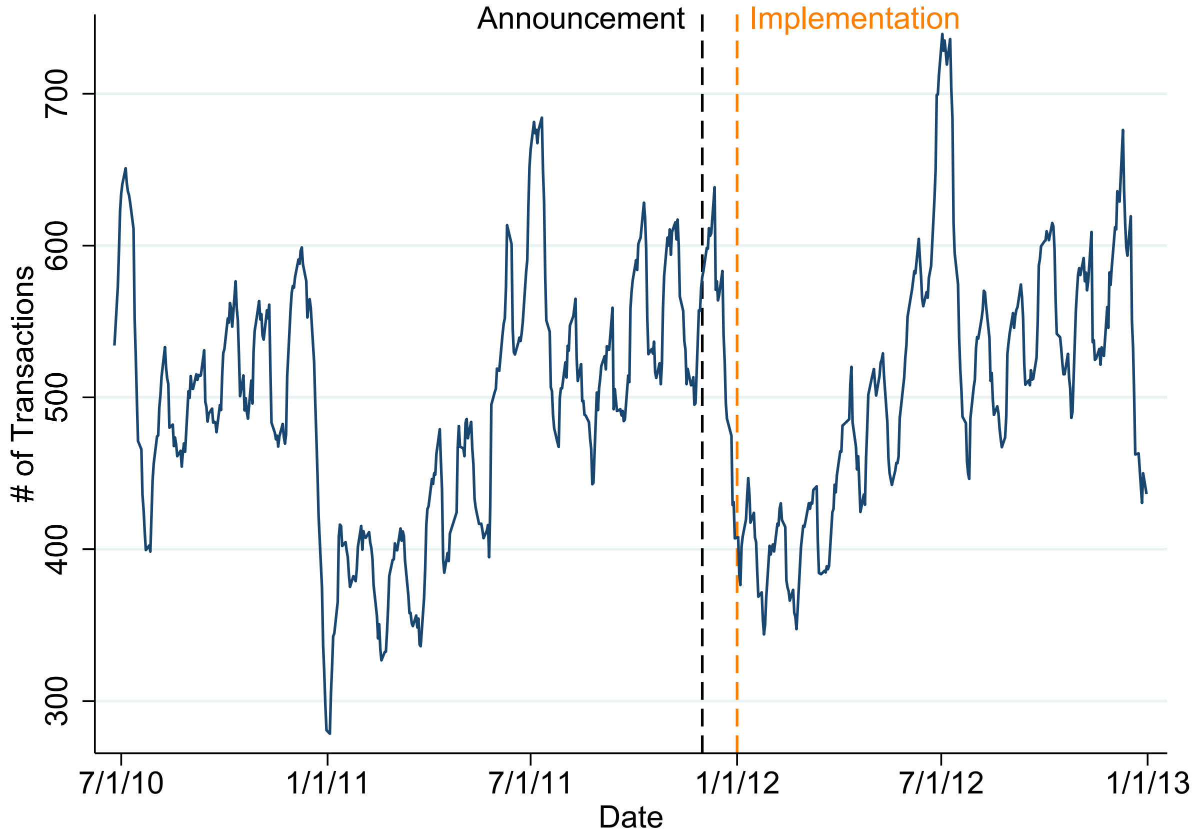


Figure A5: **Dynamic impact of LTV policy on LTV ratio**

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. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. Figure shows the βs 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.

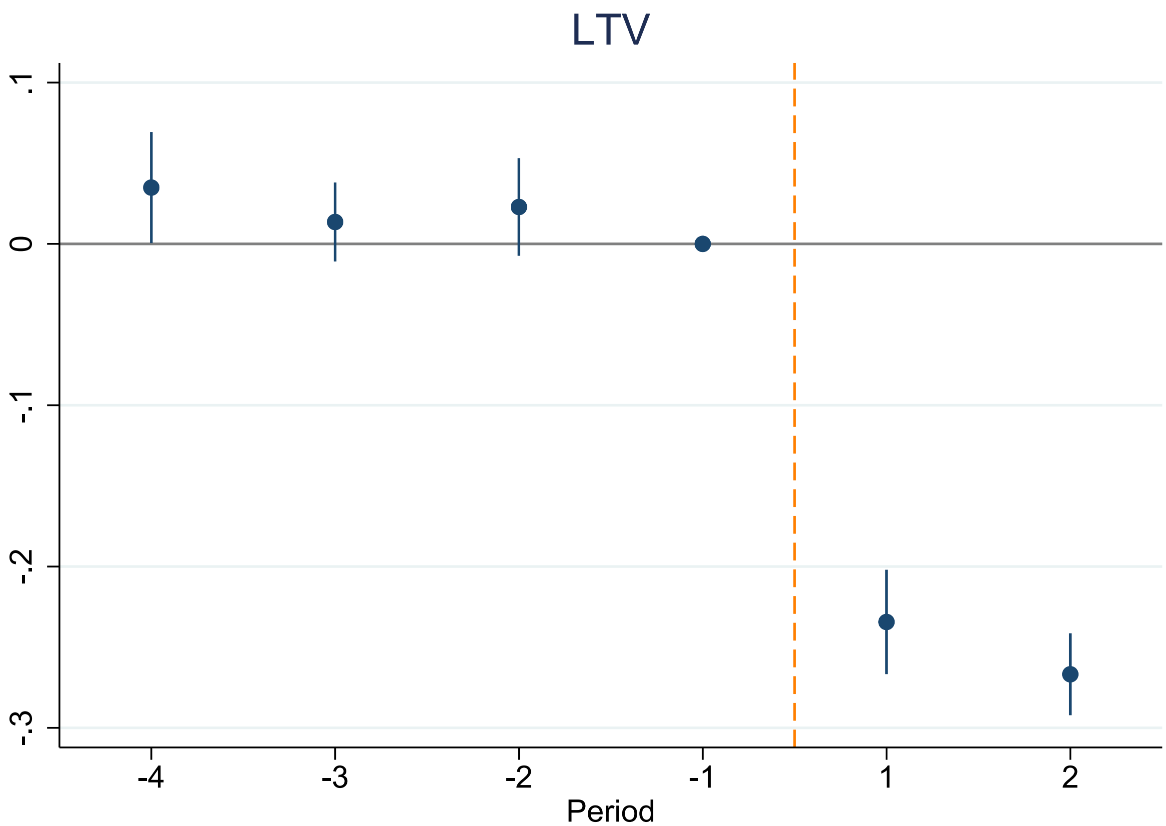
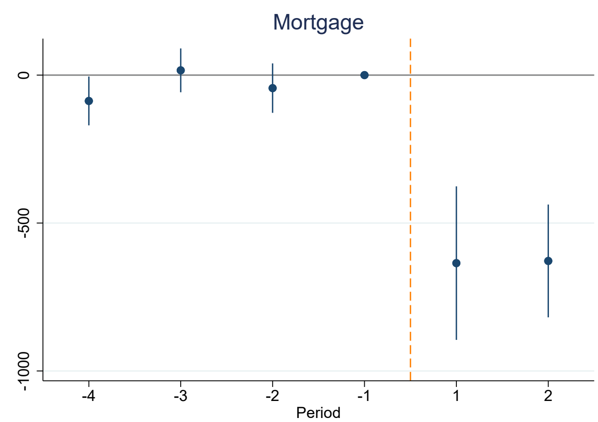


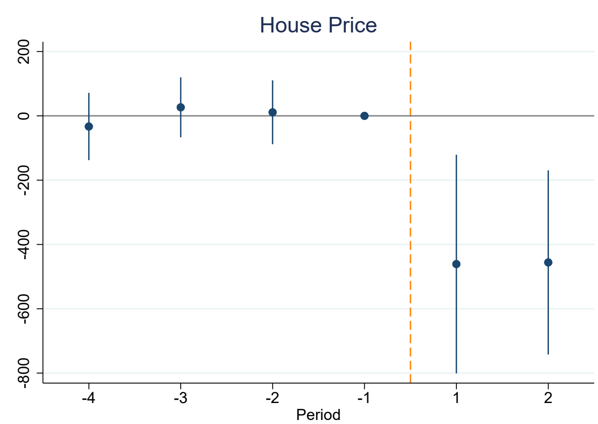
Figure A6: **Dynamic impact of macroprudential policy on mortgages, house prices, and deposits**

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. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. Figure shows the βs on the y-axis of the regression models, $y_{ht} = \sum_{k=-4}^{2} \gamma_k \; D_k \times d(L\hat{T}V > 0.85)_h + d(L\hat{T}V > 0.85)_h + \epsilon_{ht}$. Baseline event period is $k = -1$. Regression models 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.

(a)



(b)



(c)

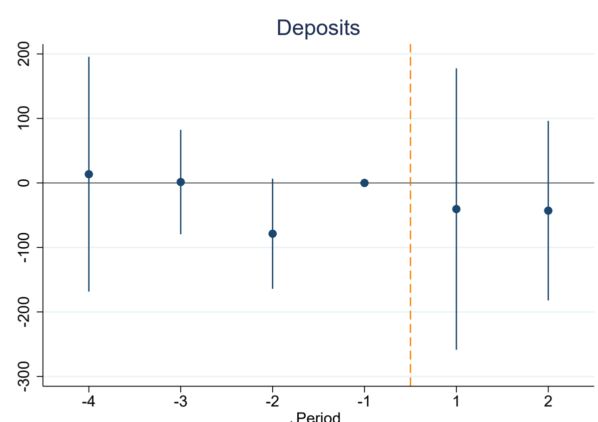


Figure A7: **Dynamic impact of macroprudential policy on interest expense**

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. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. Figure shows the βs on the y-axis of the regression model, $interest\ expense_{ht} = \sum_{k=-4}^{2} \gamma_k\ D_k \times d(L\hat{T}V > 0.85)_h + d(L\hat{T}V > 0.85)_h + \epsilon_{ht}$. 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.

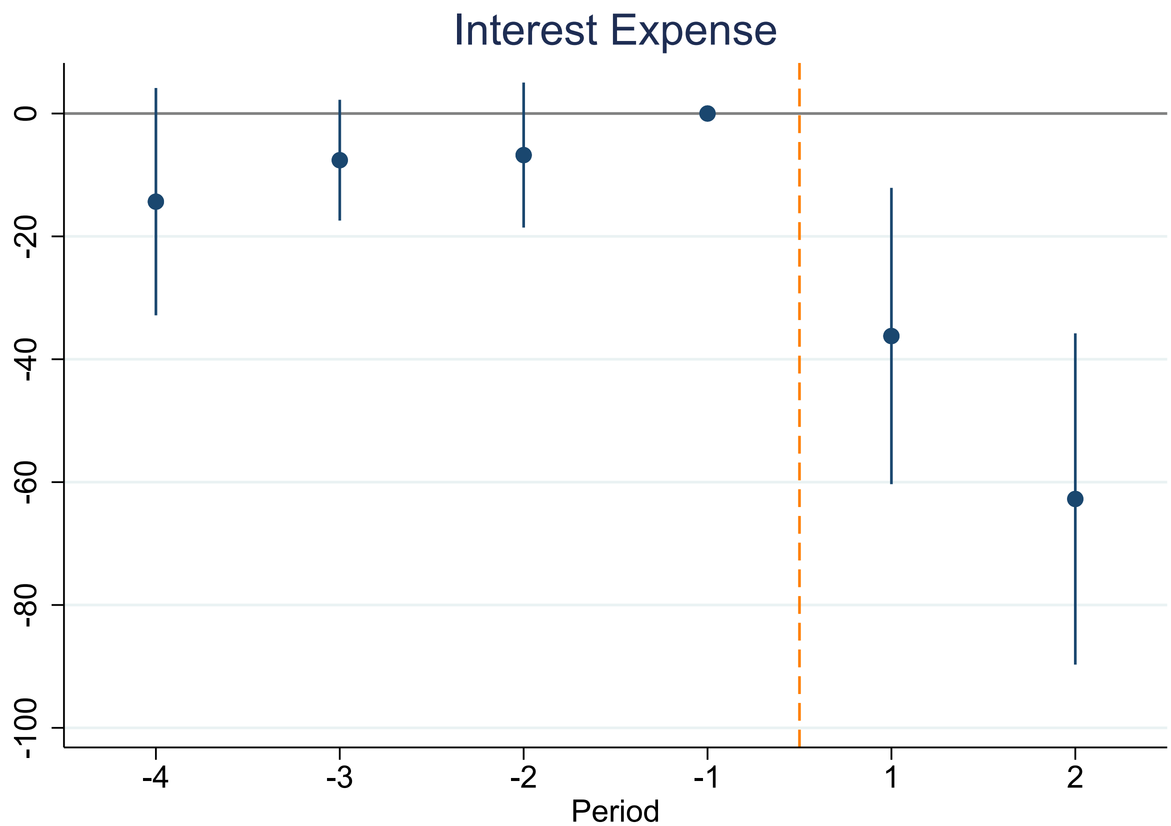


Figure A8: **Variable Importance**

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.

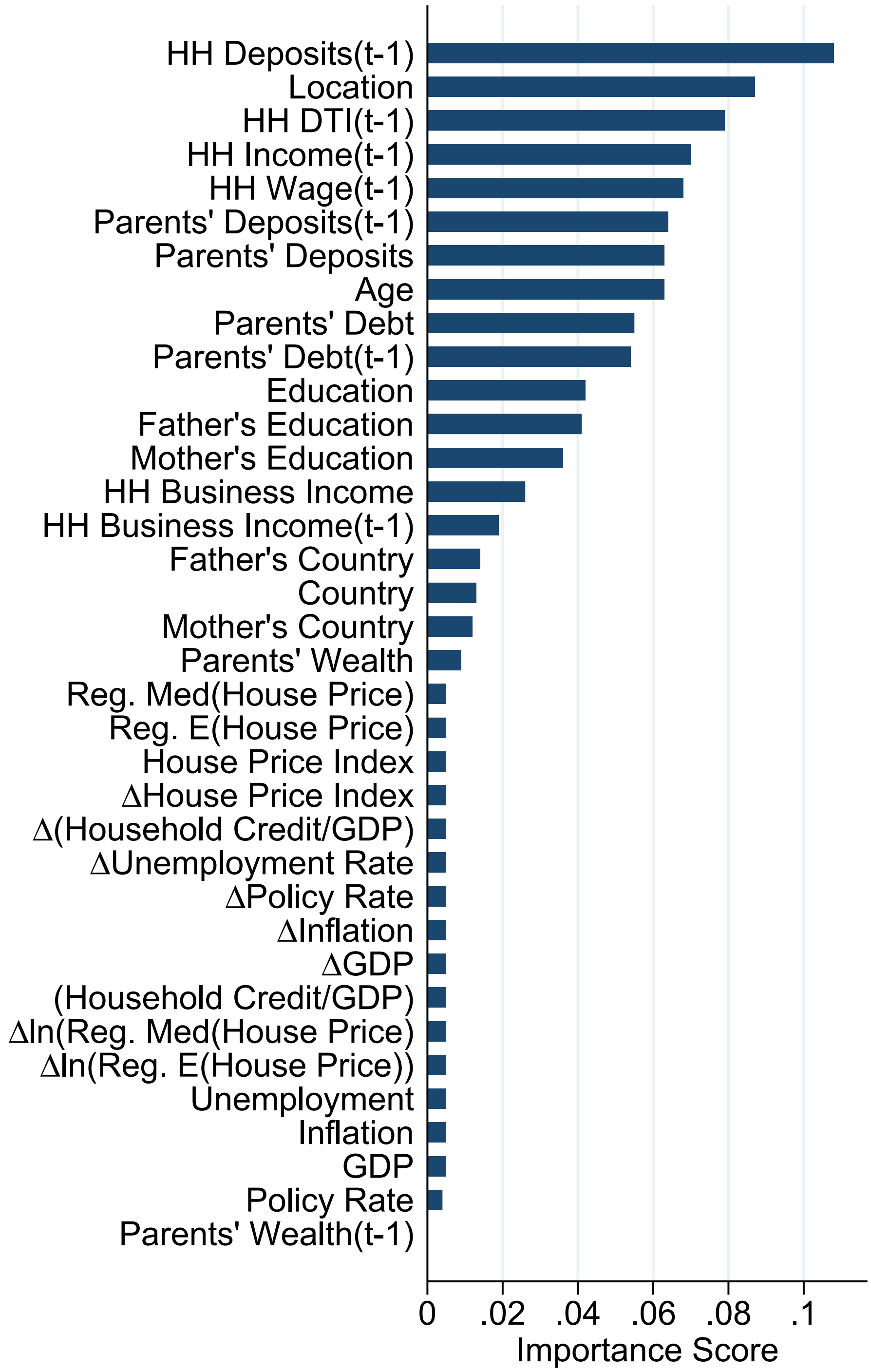


Figure A9: **Simulation exercise for placebo test**

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 4 estimated coefficients are significant in the saturated model.

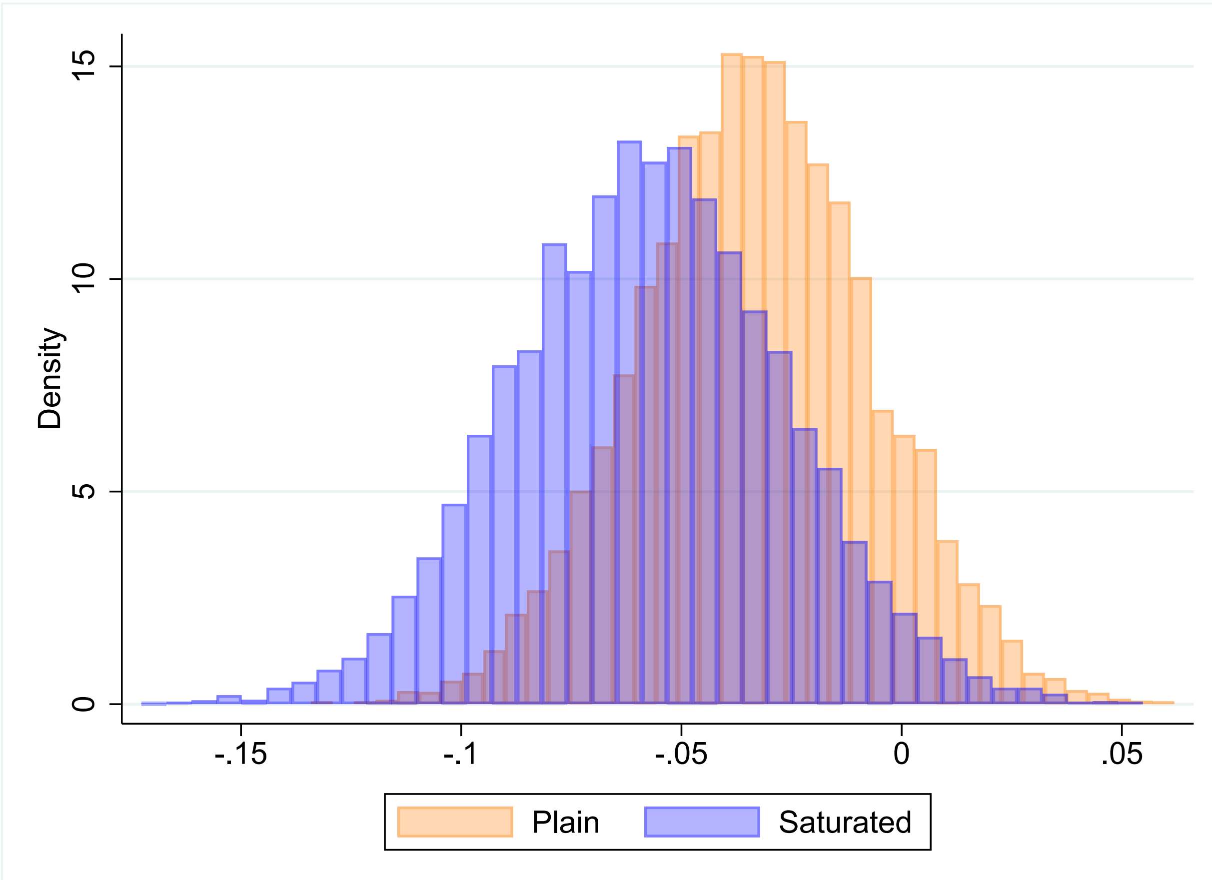


Table A1: **Impact of macroprudential policy on LTV ratio**

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.$d(L\hat{T}V > 0.85)$ 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. 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) × Post	-0.235***	-0.234***	-0.229***	-0.225***	-0.226***	-0.218***
	(0.021)	(0.021)	(0.021)	(0.017)	(0.018)	(0.030)
d($\widehat{LTV}$>0.85)	0.234***	0.233***	0.221***	0.216***	0.216***	0.212***
	(0.014)	(0.014)	(0.015)	(0.015)	(0.014)	(0.019)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × 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. $d(L\hat{T}V > 0.85)$ 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. 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) × Post	-603.153***	-667.540***	-436.306**	-503.119***	-69.821	-109.932
	(114.309)	(126.417)	(156.551)	(150.137)	(81.675)	(137.884)
d($\widehat{LTV}$>0.85)	-119.832*	90.282	-486.696***	-229.524**	-198.473***	-176.430***
	(65.223)	(61.379)	(93.149)	(81.908)	(12.966)	(45.433)
Fixed Effects:						
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 expenses**

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. $d(L\hat{T}V > 0.85)$ 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. 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) × Post	-45.875***	-44.626***	-41.265***	-36.504**	-31.523**	-37.456**
	(10.390)	(9.821)	(13.315)	(14.011)	(13.681)	(16.988)
d($\widehat{LTV}$>0.85)	-7.803**	-8.570***	-4.688	-2.726	-2.684	-0.780
	(2.769)	(2.173)	(3.609)	(4.285)	(4.278)	(5.007)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × 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 the Debt (net of deposits)-to-Income 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 households 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. $d(L\hat{T}V > 0.85)$ 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. 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) × Post	-1.035***	-1.023***	-0.986***	-0.788**	-0.934**	-0.796
	(0.323)	(0.339)	(0.320)	(0.337)	(0.380)	(0.480)
d($\widehat{LTV}$>0.85)	0.793***	0.778***	0.866***	0.890***	0.884***	0.883***
	(0.119)	(0.115)	(0.127)	(0.147)	(0.143)	(0.159)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × 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 who cannot pay for the down payment before the policy**

This table documents that removing the households who are not able to pay for the down payment does not affect the impact of the LTV ratio policy on the wage growth of 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 who 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. $d(L\hat{T}V > 0.85)$ 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. *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) × 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)
$ln(liq.)_{t-1}$	0.248	0.204	0.287*	0.278*	0.345**	0.124
	(0.163)	(0.161)	(0.158)	(0.151)	(0.152)	(0.144)
$ln(liq.)_{t-1} \times ln(liq.)_{t-1}$	-0.044	-0.037	-0.051*	-0.049*	-0.060**	-0.025
	(0.026)	(0.026)	(0.026)	(0.024)	(0.025)	(0.023)
$ln(liq.)_{t-1} \times ln(liq.)_{t-1} \times ln(liq.)_{t-1}$	0.002*	0.002	0.002**	0.002**	0.003**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × 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					

Table A6: **Impact of policy on wage growth**

This table documents that wage growth of treated and control groups do not react to macroeconomic variable 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. $d(L\hat{T}V > 0.85)$ 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. 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) × Post	0.744***	0.744***	1.030***	1.053***	0.983***	1.025*
	(0.154)	(0.154)	(0.325)	(0.284)	(0.329)	(0.555)
d($\widehat{LTV}$>0.85) × Inflation	-0.300**	-0.300**	-0.462	-0.476*	-0.478*	-0.589
	(0.142)	(0.142)	(0.272)	(0.249)	(0.269)	(0.522)
d($\widehat{LTV}$>0.85) × Unemployment	0.833	0.833	1.421	1.419	1.429	1.808
	(0.541)	(0.541)	(1.032)	(0.931)	(1.018)	(1.975)
d($\widehat{LTV}$>0.85) × GDP	-0.185**	-0.185**	-0.278*	-0.287*	-0.280*	-0.343
	(0.081)	(0.081)	(0.159)	(0.144)	(0.160)	(0.294)
d($\widehat{LTV}$>0.85) × Policy Rate	0.395*	0.395*	0.611	0.616*	0.610	0.754
	(0.193)	(0.193)	(0.378)	(0.335)	(0.372)	(0.692)
d($\widehat{LTV}$>0.85)	-3.074	-3.074	-5.102	-5.073	-5.076	-6.370
	(1.855)	(1.855)	(3.560)	(3.182)	(3.510)	(6.698)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × 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					

Table A7: **Impact of policy on unemployment spell**

This table documents the effect of the LTV ratio policy on the unemployment spell 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 is job seekers unemployment spell measured in days. $d(\hat{LTV} > 0.85)$ 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. 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(Unemployment Spell)					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	0.608***	0.584**	0.696**	0.734**	0.567*	0.632*
	(0.205)	(0.223)	(0.293)	(0.331)	(0.281)	(0.333)
d($\widehat{LTV}$>0.85)	0.019	0.038	0.044	0.027	0.017	-0.023
	(0.091)	(0.089)	(0.108)	(0.123)	(0.110)	(0.114)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R^2	0.006	0.015	0.133	0.143	0.160	0.231
Mean(ln(Unemployment Spell))	2.270					

Table A8: **Impact of policy on ex-post debt utilization**

This table documents the effect of the LTV ratio policy on the debt utilization of displaced workers during their unemployment spell. 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 log change in household level debt after the year of displacement. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 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.

	Δ ln(Ex − Post Debt)					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	-0.067	-0.059	-0.039	-0.081	-0.114	-0.209
	(0.244)	(0.253)	(0.272)	(0.304)	(0.313)	(0.336)
d($\widehat{LTV}$>0.85)	-0.023	-0.032	-0.059	-0.060	-0.063	-0.042
	(0.024)	(0.033)	(0.047)	(0.055)	(0.057)	(0.068)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R^2	0.000	0.008	0.081	0.092	0.096	0.153
Mean(Δ ln(Ex − Post Debt))	0.085					

Table A9: **Impact of policy on firm wage premium**

This table documents the effect of the LTV ratio policy on the displaced workers' employer's firm wage premium. 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 the difference of firm wage premiums between the old and new employer. Firm wage premium is estimated using AKM method (Abowd et al., 1999).$d(\hat{LTV} > 0.85)$ 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. 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.

	Δ ln(Firm Wage Premium)					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	0.004	0.010	0.062	0.082***	0.058**	0.081**
	(0.023)	(0.024)	(0.000)	(0.021)	(0.027)	(0.031)
d($\widehat{LTV}$>0.85)	0.029***	0.022**	0.012	0.008	0.009	0.010
	(0.007)	(0.009)	(0.000)	(0.009)	(0.008)	(0.009)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,672	1,672	1,637	1,637	1,637	1,637
R^2	0.002	0.042	0.193	0.228	0.386	0.472
Mean(Δ ln(Firm Wage Premium))	-0.286					

Table A10: **Impact of policy on job types**

This table documents that treated displaced workers switch to other occupations. 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 a dummy variable, which takes the value of 1 if worker changes her occupation in her new employer. $d(\hat{LTV} > 0.85)$ 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. 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.

	Different Occupation					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	0.202**	0.206**	0.281***	0.316***	0.293***	0.253**
	(0.088)	(0.093)	(0.091)	(0.094)	(0.097)	(0.118)
d($\widehat{LTV}$>0.85)	0.032	0.026	0.016	0.012	0.012	0.012
	(0.025)	(0.020)	(0.018)	(0.023)	(0.025)	(0.030)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R^2	0.009	0.028	0.137	0.153	0.183	0.261
Mean(Different Job)	0.764					

Table A11: **Impact of policy on industry switch**

This table documents that treated displaced workers switch to other industries. 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 a dummy variable which takes the value of 1 if the household stays in the same industry. Industry is measured at 2-digit NAICS code level. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 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.

	Different Industry					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	0.155*	0.166*	0.184*	0.211*	0.233**	0.178*
	(0.082)	(0.082)	(0.093)	(0.103)	(0.105)	(0.103)
d($\widehat{LTV}$>0.85)	0.038	0.026	0.021	0.023	0.020	0.015
	(0.024)	(0.021)	(0.020)	(0.023)	(0.023)	(0.022)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R^2	0.005	0.053	0.173	0.183	0.222	0.301
Mean(Different Industry)	0.650					

Table A12: **Impact of policy on labor mobility**

This table documents that treated displaced workers do not switch to other working places. 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 a dummy variable which takes the value of 1 if the household stays in the same working place measured at municipality level. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals to 1 for the years 2012 and 2013 and equals to 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.

	Different Job Location					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	0.066	0.063	0.026	0.052	0.024	0.046
	(0.132)	(0.133)	(0.137)	(0.151)	(0.157)	(0.170)
d($\widehat{LTV}$>0.85)	0.067	0.066	0.064	0.065	0.065	0.062
	(0.043)	(0.040)	(0.051)	(0.045)	(0.044)	(0.049)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R^2	0.005	0.016	0.097	0.136	0.142	0.214
Mean(Labor Mobility)	0.448					

Table A13: **Impact of policy on education**

This table documents that treated displaced workers do not increase their education levels. 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 a dummy variable which takes the value of 1 if the worker increases her education level. $d(\hat{LTV} > 0.85)$ 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. 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.

	Δ Education					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × Post	-0.019	-0.017	0.004	0.004	-0.007	-0.010
	(0.064)	(0.062)	(0.044)	(0.048)	(0.044)	(0.044)
d($\widehat{LTV}$>0.85)	0.014*	0.014*	-0.002	-0.002	-0.001	-0.000
	(0.007)	(0.008)	(0.010)	(0.011)	(0.011)	(0.012)
Fixed Effects:						
Year FE		✓	✓	✓	✓	✓
Education FE			✓	✓	✓	✓
Location FE				✓	✓	
Industry FE					✓	
Location × Industry FE						✓
Obs.	1,876	1,876	1,833	1,833	1,833	1,833
R^2	0.001	0.008	0.094	0.103	0.113	0.192
Mean(Δ Education)	0.061					