The price of leverage: what LTV constraints tell about job search and wages

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December 2022

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Abstract

Does household leverage matter for workers' job search, matching in the labor market, and wages? Theoretically, household leverage can have opposing effects on the labor market through debt-overhang and liquidity constraint channels. To test which channel dominates empirically, we exploit the introduction of a loan-to-value ratio restriction in Norway that exogenously reduces household leverage. We study home-buyers who lose their job after purchasing and find that a reduction in leverage raises wages after unemployment. Lower leverage enables workers to search longer, find jobs in higher-paying firms, and switch to new occupations and industries. The positive effect on wages is persistent and more pronounced for workers who are more likely to benefit from less-constrained job search, such as young people. Our results show that policies that limit household leverage, aiming to enhance financial stability, can improve labor market outcomes. Our findings also contribute to the debate on the costs and benefits of policies constrain household borrowing.

JEL classification: E21, G21, G51, J21.

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

Kabaş: g.kabas@tilburguniversity.edu. Roszbach: kasper.roszbach@norges-bank.no. This paper previously circulated under the title "Household Leverage and Labor Market Outcomes: Evidence from a Macroprudential Mortgage Restriction". The authors would like to thank Knut Are Aastveit, Konrad Adler, Yavuz Arslan, Marlon Azinovic, Scott Baker, Christoph Basten, Tobias Berg, Katharina Bergant, Asaf Bernstein, Bruno Biais, Neil Bhutta, Marco Ceccarelli, Piotr Danisewicz, Anthony DeFusco, Ahmet Degerli, Sebastian Doerr, Tim Eisert, Andrew Ellul, Işıl Erel, Egemen Eren, Andreas Fuster, Marc Gabarro, Thomas Geelen, Ella Getz Wold, Paul Goldsmith-Pinkham, Itay Goldstein, Knut Hakon Grini, Ragnar Juelsrud, Sasha Indarte, Ankit Kalda, Karolin Kirschenmann, Andreas Kostol, Yueran Ma, David Matsa, Fergal McCann, Charles Nathanson, Dirk Niepelt, Steven Ongena, Michaela Pagel, Pascal Paul, José-Luis Peydró, Ricardo Reis, Francesc Rodriguez Tous, Ahmet Ali Taskin, Neeltje van Horen, Joachim Voth, Uwe Walz, Toni Whited, Jiri Woschitz, Jérémy Zuchuat, as well as participants at the CBID Central Banker's Forum, EFiC Conference in Banking and Corporate Finance, IBEFA Young Economist Seminar Series, IBEFA-ASSA Meetings, PhD Conference On Real Estate and Housing at OSU, Swiss Society for Financial Market Research Conference, Swiss Winter Conference on Financial Intermediation, Western Economic Association International Conference, Young Swiss Economists Meeting, Bayes Business School, BI Norwegian Business School, Cleveland Fed, Danmarks Nationalbank, LUISS Guido Carli, Nova SBE, Norges Bank, Tilburg University, University of Groningen and University of Zurich for their helpful conversations and comments. Kabaş gratefully acknowledges financial support from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme ERC ADG 2016 (No. 740272: lending). This paper should not be reported as representing the views of Norges Bank. The views expressed are those of the authors and do not necessarily reflect those of Norges Bank.

1 Introduction

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

Theoretically, household leverage can affect the wages of displaced workers through multiple channels.[3] First, household leverage may increase the wages through a debt-overhang channel. In this channel, by directing a larger share of wages to debt-related payments, higher household leverage reduces workers' willingness to work. This leads workers to require higher wages, generating a positive effect on wages (Donaldson et al., 2019). Second, higher household leverage may reduce wages through a liquidity constraint channel. Liquidity constraints may be more binding for workers with high leverage since their debt-related payments are larger. This, in turn, can change these workers' job search behavior. For

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

[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 certain negative side effects that these policies may bring about. Galati and Moessner (2013) and Claessens (2015) provide a thorough discussion on macroprudential policies.

[3] Throughout the paper, we focus on the starting wages of displaced workers in their new jobs and refer to them as starting wages or simply wages.

instance, to avoid costly defaults, workers with high leverage may be willing to accept an earlier job offer, instead of waiting for a later offer with possibly a higher wage (Herkenhoff, 2019; Ji, 2021). The presence of opposing channels makes the effect of a borrowing restriction on job search and subsequent wages ultimately an empirical question.

We therefore exploit the introduction of an LTV restriction in Norway that creates exogenous variation in household leverage to enable such a test. Our main finding is that a reduction in household leverage improves the wages of displaced workers in their new jobs. Specifically, we show that a decline in a worker's debt-to-income (DTI) ratio by 25 percent leads to a relative increase in starting wages by 3.3 percentage points.[4] We explain our main result by documenting that the policy-induced reduction in leverage affects workers' job search behavior in three ways. First, following the mandated reduction in their leverage, workers prolong their unemployment duration by approximately 2.5 months. Second, workers with lower leverage are relatively more likely to switch to other occupations and industries, implying their job search reach is broader. Third, workers with lower leverage find jobs in firms that pay a higher wage premium. Furthermore, we show that the improvement in wages does not diminish over time. Overall, our results imply that household leverage creates constraints on job search behavior, and that a policy restricting leverage relaxes these constraints and thereby enables displaced workers to attain higher wages in their new jobs.

The Financial Supervisory Authority of Norway implemented the LTV ratio restriction we analyze in 2012 to curb the rise in house prices and household indebtedness. By introducing a cap on LTV ratios at 85 percent, this policy created exogenous variation in household leverage, measured as the debt-to-income (DTI) ratio, since affected workers would have obtained a larger mortgage in the absence of this restriction. We investigate the consequences of this LTV restriction on labor market outcomes with an empirical strategy that has two parts.

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

The first part relates to the fact that the LTV restriction is applied to all new homebuyers.[5] Due to this feature, there is no variable distinguishing the affected workers, who take smaller mortgages as a result of the restriction, from the unaffected workers who obtain the same mortgage as without the restriction. To make this distinction, we use the characteristics and LTV ratio decisions of homebuyers before the introduction of the restriction. Because homebuyers could take mortgages with LTV ratios either above or below the cap before introduction of the restriction, these observations enable us to correctly classify homebuyers before the restriction into treatment and control groups correctly within this period. We use these correctly classified homebuyers and their characteristics to classify our entire regression sample into treatment and control groups using a random forest (RF) algorithm, a machine learning method (Abadie, 2005).[6,7] The RF algorithm matches workers in the regression sample to the homebuyers before the restriction using a rich set of individual characteristics. Therefore, a worker in the regression sample is classified as treated if homebuyers with similar characteristics have initial LTV ratios above 85 percent before the restriction. Similarly, control workers are those who are matched to homebuyers with initial LTV ratios below 85 percent.

Second, workers may endogenously alter the initiation of their job search as a reaction to the restriction. For instance high-skilled workers may become more inclined to switch jobs to increase their earnings, skewing the skill distribution of job switchers. An analysis that does not properly control for this endogenous reaction may therefore produce biased estimates. We address such concerns by using only displaced workers, who lost their job in a mass layoff, in our regression models. For these workers, job search is not triggered by their individual characteristics, such as skills. Furthermore, to avoid the accumulation of unobserved home equity prior to the layoff, we restrict our sample to those displaced workers

[5] A small number of mortgages are exempted; see Section 4.1

[6] Van Bekkum et al. (2019); Aastveit et al. (2020) have a similar strategy in an LTV ratio restriction setting and use a linear probability model to predict the treatment status.

[7] We train and validate the RF model with all first-time homebuyers before the policy, excluding the homebuyers who meet the criteria to be in the regression sample. Then, we use this trained RF algorithm to classify the regression sample. Section 4.1 explains the implementation of RF prediction model in detail.

who bought a house within 12 months before their displacement.

Implementing our empirical strategy to study the effect of the LTV restriction on labor market outcomes entails a steep data requirement, which we tackle with the help of several administrative population registers available in Norway. First, we obtain debt, income, and other balance sheet data from the official tax filings for the entire adult population of Norway. We merge these tax data with information 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 the details of the employment relationship. Finally, we complement this with home purchases collected by the Norwegian Mapping Authority. The combined data set includes information about individuals' assets and liabilities, wages, unemployment duration, job choice, and other individual characteristics such as education and immigration status. We combine this data set with our empirical strategy to conduct a difference-in-differences analysis, comparing those displaced workers who had recently bought a house and are likely affected by the LTV restriction (treatment group) to those unaffected by the restriction (control group). We obtain three main results.

First, upon introduction of the restriction, the treated workers experience a drop in their household leverage. More specifically, the restriction reduces these treated workers' DTI ratios by 25 percent. This decline in the DTI ratio is realized through a reduction in the mortgage size and a downward adjustment in the price of acquired homes. The LTV restriction was thus highly effective in constraining borrowing by households and therefore provides 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 show that affected workers with reduced leverage realize higher wage growth between the job from which they are displaced and the next job they find. In particular, we find that a 25 percent decline in workers' DTI ratio leads to a 3.3 percentage point smaller decline in wages compared to the 7.4 percentage point average reduction displaced workers experience. The improvement in starting wages is robust to controlling for individual, location, and industry-specific characteristics, refining the worker sample by removing workers with a possibly different attachment to the labor market, narrowing down the sample by gradually excluding observations below the restriction threshold, and controlling for macroeconomic conditions.

We show that the effect of the restriction on wages is not driven by endogenous selection into the housing market. Because regulatory borrowing restrictions can affect house purchase decisions directly, i.e., not only through borrowing capacity, workers that cannot afford the down payment can decide to buy a house before the implementation of the LTV restriction. Alternatively, they can delay their purchase until after our sample period when they have saved for a down payment. The potential for an LTV constraint to exert such an influence on house purchase decisions could change the characteristics of the treatment group and bias our results. We address such a concern in two ways. First, we show that the LTV restriction does not change the observable characteristics of the treated workers. Second, in a robustness test, we remove all workers who would not have been able to make a 15 percent down payment out of their deposits in the pre-restriction period. [8]. Estimates for this restricted sample are very close to our main estimates, indicating that our results are not confounded by this selection effect.

Third, we document that the improvement in starting wages stems from a mitigation of the job search constraints higher leverage creates. This influences starting wages positively through several channels. Through a reduction of their leverage, workers are able to extend their job search by 2.5 months, which suggests that lower debt reduces the pressure on displaced workers to quickly find or accept a new job. Moreover, we establish that the

[8]Because the main reason for endogenous selection is the ability to afford the down payment, this restricted sample does not suffer from the selection effect.

reduction in leverage enables displaced workers to find job matches with firms that pay higher wage premiums (Abowd et al., 1999). This improved matching explains 20 percent of the gain in starting wages. A reduction in leverage also enables a broadening of the scope of 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. Changes in geographical labor mobility or investment in additional education are not drivers of our results.

We provide support for this mechanism by documenting how heterogeneity across the sample affects the positive effect of reducing household leverage. If the LTV restriction improves wages by relaxing the constraints that leverage puts on job search, we may find greater gains for sub-samples who have more potential to benefit from a better job search. We find that workers younger than the median age, having a shorter job tenure with the previous employer or with higher education, drive the improvement in wages. This is consistent with the notion that it is easier for younger workers or workers with higher education to invest in the human capital required for a different occupation or industry. Longer job tenure with the same firm also tends to make human capital more firm-specific and limit the value of better job search. Further heterogeneity tests indicate that the improvement in wages is particularly larger for female and low-income workers. These findings imply that a reduction in household leverage and related improvements in job search may be important for disadvantaged workers in the labor market.

Finally, we find that the positive effect on wages is persistent. Four years after their displacement, workers with reduced leverage are able to maintain a significant wage advantage. More specifically, these workers have a 4.7 percentage point higher wages at the end of the four-year post displacement period that we observe. These same workers also enjoyed lower wage volatility during the four years after their displacement, indicating that the rise in wages is not attributed to these workers taking jobs with greater hour volatility or a greater risk for discontinuation.

In sum, our paper documents the constraints that household leverage creates in the job search of displaced workers, and a mitigation of these constraints improves these workers' starting wages. Our results thus imply that macroprudential policies aiming to limit household leverage can have positive side effects on the labor market. We show that macroprudential policies provide better prospects in the labor market, thereby contributing to the debate on the trade-offs involved in constraining household borrowing. In addition, our results provide new insights on how household leverage interacts with the real economy through labor markets. These labor market implications of household leverage are potentially important for policymakers as high household leverage has been a common characteristic of recent recessions and household debt levels continue to be elevated in many countries.

The findings in our paper speak to at least four strands of the literature. First, our paper adds to the literature showing that household debt and credit access affect 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 the 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 need to cut their spending (Eggertsson and Krugman, 2012; Mian et al., 2013; Guerrieri and Lorenzoni, 2017), which in turn puts pressure on the aggregate demand and increases unemployment (Mian and Sufi, 2014; Mian et al., 2017). Our findings complement these analyses by documenting the direct effect of household leverage on the labor markets. In addition to the indirect demand channel, household leverage affects workers' job search and labor market outcomes by introducing constraints that reduce the matching quality in the labor market.

Our second contribution is to the discussion on the effectiveness of macroprudential policies. Many countries implemented policies to limit household leverage after the Global Financial Crisis, sparking a debate about their impact on the economy. On the one hand, these policies can curb 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 et al., 2019; Araujo et al., 2019; Peydró et al., 2020).[9] On the other hand, these policies limit the access to credit markets, which can generate adverse unintended side effects, for example in the access to housing or precautiuonary saving. (Acharya et al., 2019; Aastveit et al., 2020; Tzur-Ilan, 2020). We investigate the influence of an LTV ratio restriction, one of the most common macroprudential tools, on the labor markets and document a positive, unintended, effect of an LTV restriction on labor markets. In closely related work Pizzinelli (2018) develops a life-cycle model with LTV and LTI restrictions to study second earners' labor supply. While Pizzinelli (2018) finds no effect of an LTV restriction on female employment, we empirically document that, by reducing household leverage, an LTV restriction increases displaced workers' starting wages.

Third, our paper is related to the papers studying the determinants of the labor supply and job search. While unemployment insurance and severance pay have been the main focus of these papers, after the Global Financial Crisis, recent literature has been studying how household balance sheets interact with labor supply and job search. One finding of this literature is that negative home equity following a decline in house prices impairs labor supply by limiting labor mobility (Bernstein and Struyven, 2017; Brown and Matsa, 2019; Gopalan et al., 2020; Bernstein, 2020). Also, similar to the liquidity effect of unemployment insurance (Chetty, 2008), access to credit via credit cards or loans backed by home equity enables workers to have better job search (Herkenhoff, 2019; He and le Maire, 2020; Kumar and Liang, 2018). In addition, interest payments can influence labor supply decisions through a consumption commitment channel (Chetty and Szeidl, 2007; Zator, 2019).[10] We contribute

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

[10]See also Mulligan (2009, 2010); Li et al. (2020); Maggio et al. (2019); Fos et al. (2019) and Cespedes et al. (2020). Rothstein and Rouse (2011) find that student debt affects students' academic decisions, causes graduates to choose higher-salary jobs at the cost of taking fewer lower-paid "public interest" jobs. Sharing negative information about households' past credit market behavior has also been shown to reduce employment and mobility (Bos et al., 2018).

to this growing literature in two ways. First, to our knowledge, we provide the first causal effect of a household leverage restriction policy on job search behavior and wages.[11] While the debt overhang channel (Donaldson et al., 2019) and the positive effect of access to credit and interest payments on earnings suggest that household leverage might have a positive effect on wages[12], we find a negative effect driven by liquidity constraints that household leverage generates (Ji, 2021). Second, we use only displaced workers who lost their jobs due to mass layoffs instead of the whole population or all home buyers. Unemployed workers and their conditions are crucial input for the policy decisions since they influence important macroeconomics indicators, such as unemployment rate and consumption. Therefore, documenting how a household leverage restriction affects the labor market outcomes of such workers can enable policymakers to make better policy designs. Furthermore, displaced workers allow us to cleanly estimate changes in wages and job search as their job search is not triggered by their individual characteristics.

Fourth, our paper adds to the research investigating 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).[13] We contribute to this literature by demonstrating that policy-induced reductions in household leverage can mitigate the loss of income following a 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,

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

[12] Note that access to credit and interest payments are positively correlated with household leverage. Thus, the positive effect of access to credit and interest payments on earnings implies that household leverage might have a positive effect on earnings as well.

[13] 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. 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).

Section 4 explains the empirical strategy, Section 5 presents the impact of LTV constraint on household finances and labor market outcomes, and Section 6 concludes.

2 Institutional background

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

Housing market Norway's housing market can be characterized by its high homeownership ratio. Since the Second World War, the Norwegian Government has supported homeownership through several policies, such as tax breaks for homeowners. Due to these policies, the homeownership rate in Norway has been stable at slightly above 80 percent since the beginning of the 1990s, making Norway's homeownership rate one of the highest among advanced countries.[14] This high rate is coupled with full-recourse mortgages, most of which have floating rates. The default rate on these mortgages is low, which can be explained by the high costs attached to a default. In addition to non-pecuniary costs, such as involuntary relocation, a default creates an additional financial burden on defaulters in two ways. First, banks apply fees for delayed payments, which can be substantially higher than mortgage rates. Second, the seized real estate is usually sold in the market with a discount that can be up to 20 percent.[15]

Labor market regulation The labor market in Norway is governed by the Working Environment Act and the Labor Market Act, both of 2005.[16,17] 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

[14] For comparison, the same rate for Sweden and Denmark are about 60 percent.
[15] Buyers apply this discount due to legal process that such a purchase entails.
[16] Act relating to the working environment, working hours, and employment protection.
[17] Lov om arbeidsmarkedstjenester.

special status for collective redundancies− situations where notice of dismissal is given to at least 10 employees within a 30-day period. In such situations, the employer does not have to provide personal-specific reasons to the workers. The notification period for job termination depends on the worker's job tenure and age. The Act states that a period of one month's notice shall be applicable to both workers and employers. For the workers who have been employed in the firm for at least five consecutive years, two months of period is applied.[18]

Unemployment benefit coverage in Norway approximately equals the OECD average of 60 percent. Specifically, displaced workers can receive 62.4 percent of their previous income up to six times the National Insurance Scheme's basic amount, which was NOK 75,641 (USD 12,712) in 2010. The duration of the unemployment insurance is up to 2 years, depending on the workers' previous earnings.[19] Most importantly for our purposes, neither the coverage ratio nor the duration has changed during our sample period. In addition, only a small number of workers in our sample have longer unemployment duration than 2 years and, as explained in Section 5.2, removing such workers does not change our results, alleviating concerns regarding the influence of unemployment insurance on our results.

Norwegian economy and macroprudential policy framework Norway's economy has displayed stable economic growth, with both inflation and average unemployment below four percent during the past 30 years. For instance, during the Global Financial Crisis (GFC), its GDP fell by only 1.7 percent.[20] Reflecting this stability, house prices have nearly tripled since 2000. Norwegian households' debt to GDP ratio has simultaneously increased from 50 percent to 105 percent (Figure 1). Due to these steep rises in house prices and household indebtedness and possible spillover effects on financial stability, the Norwegian policymakers

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[18]This period increases gradually up to six months with the worker's age. For the majority of the workers in our sample, one month is the relevant duration.

[19]Workers need to earn at least 1.5 times the basic amount over the previous 12 months or on average more than three times the basic amount over the past 36 months to be eligible for unemployment benefits. To be entitled to the maximum of 104 weeks of unemployment benefits a person had to earn 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.

[20]Figure A1 illustrates the macroeconomics conditions in Norway.

have implemented macroprudential policies, which we explain in Section 4.1 in detail. The main legal basis for these policies is the Financial Institutions Act (Lov om Finansforetak og Finanskonsern, henceforth FIA). According to this act, the financial stability instruments are shared among the Ministry of Finance (MoF), the Finanstilsynet (Financial Supervisory Authority−FSA) and Norges Bank (Central Bank of Norway). While the FSA advises the MoF on desirable regulations under the FIA, decisions on new regulations are made by the Ministry.

3 Data and sample construction

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

3.1 Data sets

We obtain the labor market data for our study from the official employer-employee register (Aa registeret) administered by Norwegian Labour and Welfare Administration (NAV). All employers and contractors are obliged by law to report their employees and details on the employment relationship. In this register, we can track for which employer a worker works, what occupation she held, what wages were paid, the job start and termination dates, as well as the geographic location of the workplace. We complement this labor market information with administrative data from the population register and official tax records. The population register includes background variables such as gender, age, parent identifiers, marriage status, residential municipality, immigration status, and education. The tax records enable us to isolate labor income and business income, capital gains, interest expenses, government transfers, debt, bank deposits, and total wealth. The last data set is collected by the Norwegian Mapping Authority and contains information on all real estate and housing transactions, including both the buyers' identifier, the transaction value, and a location identifier.

3.2 Sample construction

Our main sample period starts in 2006 and ends in 2013.[21] 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 workers who satisfy two criteria: (a) job loss due to a mass layoff, and (b) bought a house before the job loss. To avoid a bias that an unobserved ability to build 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 with 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.

[21]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 performed by NAV. The 2014 reporting change generates noise in the data because of large numbers of "intra-group" job changes.

3.3 Variable construction

In this project, we use variables on 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 payments at the household level. When considering labor market outcomes and job search behavior, we switch to the individual (worker) level. This part of the paper introduces the main variables we use and provides 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 by using a credit type identifier. Therefore, we 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 to produce 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 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 the household's total debt by the household's total income before the 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 the symmetric growth rate to allow for labor market exit and limit the role of outliers.[22] In line with the job displacement literature, the average wage growth for displaced workers is negative in our sample. Our data set enables us to 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 an unemployment spell of 132 days in our sample.

We use the Norwegian Standard Classification of Education at the 3-digit level to measure education. Our education variable captures both the level and the broad field of education. The level indicates if a person has compulsory, intermediate, or higher education. The broad field refers to a general classification of academic content. There are 142 unique education levels in our sample.[23] We use Statistics Norway's seven-digit occupational information to capture changes in the profession, which 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.[24] The remaining digits break down each main occupational category into further subgroups.

Of the workers in our sample, 15 percent reside in Oslo, close to the city's population weighting 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.

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

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

[23]The levels are primary, lower secondary, upper secondary, post-secondary, the first stage of higher education, and second stage of higher education. The broad fields are humanities and arts, teacher training and pedagogy, social sciences and law, business administration, natural sciences, health, primary industries, and transport and communications. As an example, with 3-digit detail, we can differentiate whether a person with social sciences and law background studies in sociology or psychology.

[24]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 Empirical strategy

Our objective is to estimate the effect of the LTV restriction on job search and subsequent wages. Thanks to the exogenous nature of the LTV restriction, this estimation enables us to identify the causal effect of household leverage on labor market outcomes. To understand this, we can consider a regression model where labor market outcomes are naively regressed on household leverage. Since leverage and labor choices are likely to be made jointly, this model will potentially yield biased estimates for leverage. For instance, a worker might have a low wage if her skills are also low. At the same time, her low wage could lead her to have higher leverage to sustain consumption, creating a spurious negative correlation between leverage and wages. We address this endogeneity challenge by exploiting the legally imposed LTV restriction as a source of exogenous variation in household leverage.

4.1 Using macroprudential policy as an experiment

Due to the steep rise in house prices and household debt levels, the Finanstilsynet (FSA) issued "Guidelines for prudent lending standards for new residential mortgage loans" to be effective by fall 2010. This initial version of the guidelines established a maximum permissible LTV ratio of 90 percent. Shortly after, the FSA issued an update on the guidelines in December 2011 that will be effective in January 2012. The reason for the update is that the initial 2010 guidelines proved to be ambiguous about the precise implementation period, and the FSA reported low compliance with the policy.[25] In this update, the FSA reduced the LTV threshold to 85 percent and specified that mortgages granted on the same property by other lenders shall also be included in the LTV ratio.[26] Thus, the stricter updated guidelines make sure the compliance by covering loans with collateral claims on a particular property

[25]The FSA explained the motivation for the update as "the proportion of residential mortgages with a high loan-to-value ratio is on the increase, and a round of inspections of mortgage lending practice at a selection of banks shows that credit assessments need to improve (Finanstilsynet, 2011)."

[26]Also, interest-only mortgages and collateralized lines of credit were restricted by an LTV ratio of 70 percent.

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 is that it covers the whole population of new homebuyers, meaning that all LTV ratios are below 85 percent after the restriction.[27] However, this feature does not mean that the restriction treats every homebuyer. Before the restriction, 35 percent of the homebuyers obtain mortgages with initial LTV ratios lower than 85 percent. This implies that approximately one-third of the workers in the post-treatment period would have LTV ratios below 85 percent even though the LTV restriction was not implemented. Such workers who endogenously prefer to have low LTV ratios are natural candidates for the control group. Yet, we do not have a variable that enables us to separate these workers from the treated ones.

The common solution the literature applies to similar cases where the treatment status is missing is proxying the treatment status with one variable that is positively correlated with the actual treatment status.[28] Facing a similar problem, recent papers on the effects of LTV restrictions follow Abadie (2005) and use linear probability prediction models to construct the treatment and control 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 workers into treated and control groups.

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

[27]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 are thus untreated, we remove these few observations from our estimation sample. The placebo test in Section 5.2 shows that this removal does not create a bias in the wage growth regressions.

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

first advantage is that by using many variables instead of a single variable, RF improves the accuracy of the treatment classification. This advantage is expected since a rich set of variables has more information compared to a single variable. Athey and Imbens (2019); Calvi et al. (2021) consider this advantage and notice how beneficial ML methods can be for cases similar to ours. The second advantage is that, unlike linear probability models, RF does not impose any functional form on the classification. Therefore, RF is capable of capturing the true data generating process more flexibly. Third, similar to other ML methods, RF is designed to maximize out-of-sample forecast power. This is crucial for our purpose as using many variables in the classification model can generate an overfitting problem. By focusing on out-of-sample instead of in-sample, RF alleviates the concerns regarding overfitting and provides more robust classification performance for the post-treatment period.

We use RF to classify the households as treated and control units in three steps.[29] In the first step, we construct the training and validation samples. In these samples, we use homebuyers from the period between 2002 and 2010. Moreover, we exclude the workers in our sample as having these workers in these samples might increase overfitting. We use several population registers to collect a rich set of household-level data, which include household-level income, wage, deposits, DTI, business income, education, age, location, and immigration status. Moreover, we add parents' deposits, debt, wealth, education, and immigration status to the data.[30] To incorporate the influence of the macroeconomic conditions and house prices on the LTV ratio decisions, we include GDP growth, inflation, unemployment, monetary policy rate, and regional and national house prices. We label the homebuyers as treated if their initial LTV ratios are above 85 percent and the others as control. In the second step, we use these correctly classified homebuyers with the variables above to train and validate the RF model.[31] In the last step, we classify all workers in the regression sample into treated and control groups using the trained RF model.

[29]Details about the application can be found in section A2.

[30]All of the balance sheet items are lagged by one period.

[31]Pruning and how the parameters are chosen are explained in section A2.

Thanks to the data availability, the classification power of the RF is high. The standard way to assess the performance of a binary classifier is by plotting its Receiver Operating Characteristics (ROC) curve and calculating the Area Under the Curve (AUC). A ROC curve shows the true positive rate and false positive rate for different probability thresholds to classify an observation to be treated, and the AUC, which summarizes the information on the ROC curve, is the measure of the area under the ROC curve (Bradley, 1997). The range of values of AUC is between 0.5 and 1, and higher values indicate a more successful classifier. For instance, a perfect classifier that classifies each observation correctly would have an AUC of 1.[32] In the literature, AUC values larger than 0.9 are considered as excellent (Hosmer Jr et al., 2013). In our case, the AUC is 0.88, which means that RF performs very well. Another way to evaluate the performance is by looking at the success rate of RF for the pretreatment workers. We can compare these workers' true treatment status and how RF classifies them. We see that RF correctly classifies 82 percent of these workers, which documents the model's classification power.

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 essential 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 related to the likelihood of being affected by the LTV ratio restriction. Table 2 lays out these differences between the treated and control groups. For instance, workers 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.

[32]Specifically, AUC shows the probability that a randomly chosen treated observation will have a higher estimated treatment probability than a randomly chosen control observation.

One concern about the classification model could be that the LTV restriction can affect house prices and this may reduce the model's accuracy after the restriction. 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 encapsulated by before-policy house prices, the classification model should be able to incorporate the effect of the LTV restriction on the house prices.

4.2 Empirical specification

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

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

where y_{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 worker 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 workers 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 high out-of-sample predictive power of our RF model provides additional reassurance about the risk of misclassification. Moreover, as depicted in Figure A3, the majority of the misclassified workers in the pretreatment period are clustered around the threshold. The impact of the LTV restriction on household leverage is smaller for the workers whose LTV ratios are closer to the threshold since the restriction reduces the leverage with a smaller amount. Therefore, having the majority of the misclassified workers clustered around the threshold suggests that the magnitude of the attenuation bias is limited.

The second implication is about the differences between the treatment and control groups. As we use observable differences among workers to assign them to treatment and control groups, it is natural to see that these groups have different characteristics. Due to these differences, the treatment and control groups may have different labor market prospects. Since we use a difference-in-differences estimation, we control for the influence of these different characteristics by taking differences among treated workers and control workers. These different characteristics can pose a threat to the causal interpretation only if their influence on labor market outcomes changes at the same time as the LTV restriction. Therefore, our causal interpretation rests on the assumption that the effect of these different characteristics on labor market outcomes should not change at the same time as the LTV restriction. The graphs in Section 5 illustrate robust parallel trends between the treatment and control groups before the restriction. As these parallel trends indicate that the influence of different characteristics does not change over time in the pretreatment period, they also provide support for the assumption.

5 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 LTV 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 DTI 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 4 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 4. 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 on smaller mortgages, which they use to buy cheaper houses. We find that they, after introduction of the policy, take on mortgages that are on average NOK 603,000 smaller to pay for homes that are NOK 503,000 cheaper. According to our estimations, the 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 principal repayment 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 from 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 pressure on displaced workers because they need to service their debt (Ji, 2021). The reason is that defaulting on loans has been shown to be associated with substantial costs such as deteriorated credit scores or worsened labor market prospects.[33] Or, workers may not be willing to make adjustments to their housing consumption due to consumption commitments(Chetty and Szeidl, 2007). They may therefore decide to accept earlier 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 in 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.

We use the exogenous change in household leverage caused by the macroprudential policy

[33]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; Maggio et al., 2019).Diamond et al. (2020) documents the non-pecuniary costs of foreclosures.

with detailed labor market data to answer this empirical question. Figure 5 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, wage growth for 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.[34] Indeed Figure 5 shows that treated workers experience 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 5 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 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-

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

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 experience 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.

Selection One concern about the causal interpretation of our results is endogenous selection into homeownership. Households that 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. As explained in Section 2, Norway has one of the highest homeownership rate among the advanced countries. This cultural tendency suggests that the selection could be limited if the policy does not influence transition into homeownership significantly. We plot the homeownership transition rate in Figure 6, which depicts a lack of effect of the LTV restriction on transition into homeownership. Even though this figure implies that the selection may not be a major problem for our findings, we further investigate the role of the selection for our results.

This endogenous selection of the households is expected to be strongest around the policy implementation. After the announcement of the restriction, the households that think that they cannot afford 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 six months before the first LTV restriction implementation, we effectively exclude the households that 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.[35] 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 when wealth tax is assessed.[36] 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.[37] 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

[35]Bernstein and Koudijs (2021) use a similar strategy for mortgage amortization policy in the Netherlands.

[36]For primary houses, the tax value is 25 percent of the housing value with a tax rate of 0.7 percent.

[37]We find that the LTV restriction decreases the house prices (Table A2).

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 mitigates this concern effectively.

Second, we homogenize the treated households across the both periods in terms of their ability to afford the down payment. As explained before, the main reason for the endogenous selection is the changes in the treated households' ability to afford 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 that 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.

External validity Another concern about our findings is their external validity. The external validity can be limited since we use data from Norway, which is known with its generous labor market policies, and employ a specific sample. We now investigate how generalizable our results are, starting by our sample selection criteria.

As explained in Section 3.2, we use displaced workers who bought their homes up to 12 months before losing their jobs. This sample enables us to observe job search behavior clean from individual characteristics and to prevent the individual saving propensity to influence the effect of policy on household leverage, helping us establishing causality. Even though this sample is highly relevant for policy discussions[38], it does not represent the general population. In Table 7, we relax our sample criteria and assess the relationship between leverage and wages in wider samples.[39] In Column (1), we use the whole population and find that leverage

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

[39]In the first three columns of Table 7, we use ln(debt) instead of DTI as the main independent variable.

is negatively associated to wage growth within the same individual, which is in line with our results. In Column (2), we use unemployed workers, who could be unemployed voluntarily or involuntarily. The coefficient is again negative, but increases in magnitude. This increase may reflect the fact that the wages are sticky due to wage contracts. Therefore, including employed workers in the sample can mask the effect of leverage on wages. In Column (3), we use only displaced workers. The magnitude of the negative coefficient is smaller compared to the one in Column (2). This decline in magnitude illustrates the importance of using workers who lose their jobs involuntarily since it indicates that selection into unemployment generates an upward bias. In the last column, we turn to displaced workers who bought their homes before losing their jobs. Instead of 12 months, this column uses workers who bought their homes up to 4 years. Note that, in this sample, the leverage is not exogenous because workers may pay their mortgage in different speeds, making the leverage at the job loss dependent on individual savings rate. The difference in savings rate can create a bias if the rate is correlated with factors that influence labor market outcomes. Therefore, the change in coefficient size is informative about the magnitude of bias that individual savings rate may generate. Indeed, we estimate a larger coefficient in this wider sample, which indicates the importance of using recent homebuyers for establishing causality.

Next, we consider the labor market policies in Norway. Section 2 points out that the unemployment insurance generosity in Norway is mainly driven by its duration, which is 2 years, as the replacement rate is close to OECD median. The long unemployment insurance duration may reduce our results' external validity, if the workers exhaust unemployment insurance fully. We assess this possibility in Table 8. In Columns (1) and (2), we remove the workers whose unemployment spell is longer than 500 days and 2 years respectively. In line with the fact that the average unemployment spell in our sample is shorter than 5 months, removing such workers do not affect our results. The second labor market policy we consider is the notification period for job termination. If the worker's tenure in the firm is longer

The reason is that income appears both in wage growth and DTI, which creates a mechanical correlation. Instead of using income as denominator in DTI, we create wage bins and include these bins as fixed effects.

than 5 years, the notification period increases to 2 months from 1 month, which can allow the worker to adjust her job search before job contract ends. In Column (3) of Table 8, we show that this does not create a threat for our results since removing workers whose tenure is longer than 5 years does not change our findings.

Another concern regarding the external validity of our results is related with comparability of the workers in our sample. For instance, displaced workers may have different characteristics than other workers, such as different education, or productivity (Caggese et al., 2019). Thus, results that are obtained from displaced workers may not be applicable to other workers. We alleviate this concern in Table 9. In Table 9, we use all workers that are employed in our sample firms for the sample period. We regress different worker characteristics on a dummy variable that takes the value of 1 if the worker is in our main regression sample. If the workers in our sample are statistically different than other workers in the same firm, this dummy variable should have statistically significant coefficient. To this extent, we use age, education, marital status, gender, immigration status, and wage growth before mass layoff as worker characteristics. Column (1) in Table 9 depicts that workers in our sample are, on average, younger than other workers in the same firm. This is expected as workers in our sample are first-time homebuyers. Due to this finding, we include age fixed effects, in addition to firm and year fixed effects, in the remaining columns. Table 9 demonstrates that workers in our sample are not statistically different than other workers in terms of education, marital status, gender, immigration status, and wage growth. This suggests that workers in our sample are comparable to other workers in the same firm after controlling for age. Overall, our findings fail to suggest a threat for external validity of our results.

Additional robustness checks In Table 10, we provide several robustness checks for the impact of household leverage on 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.[40] 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 A6, we show that all interaction terms of $d(\widehat{LTV} > 0.85)_h$ with the four macro variables are insignificant, indicating that wage growth for 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)

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

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 5 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.[41] 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 11 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.

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

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 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 12 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 12 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 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.[42] 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 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. The last two columns of Table 12 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-

[42] 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.

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 13 indicate that lower leverage induces workers to broaden their job search along some margins. In Table 13, 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 a 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 14 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 an 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 affording 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 15 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 15 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 16. 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 17 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 LTV ratio restriction for home purchases. Using data for the entire adult population of Norway 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 a 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, more highly 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, 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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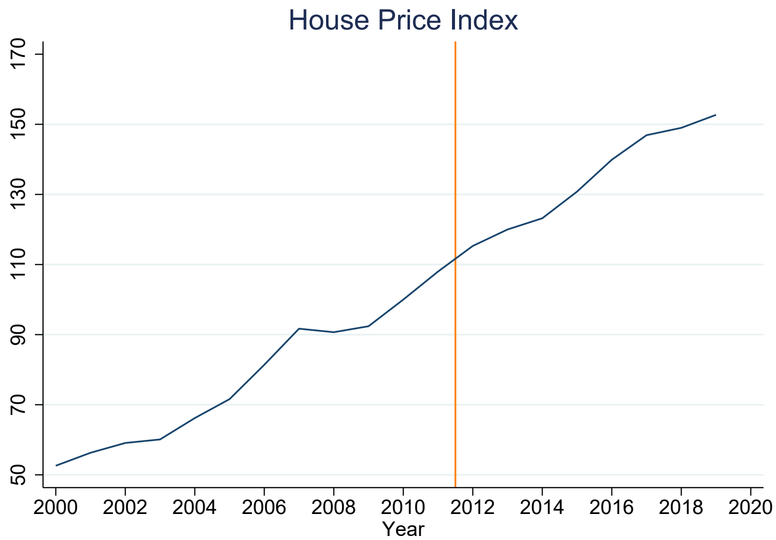
Figure 1: **Household debt and house prices**

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)

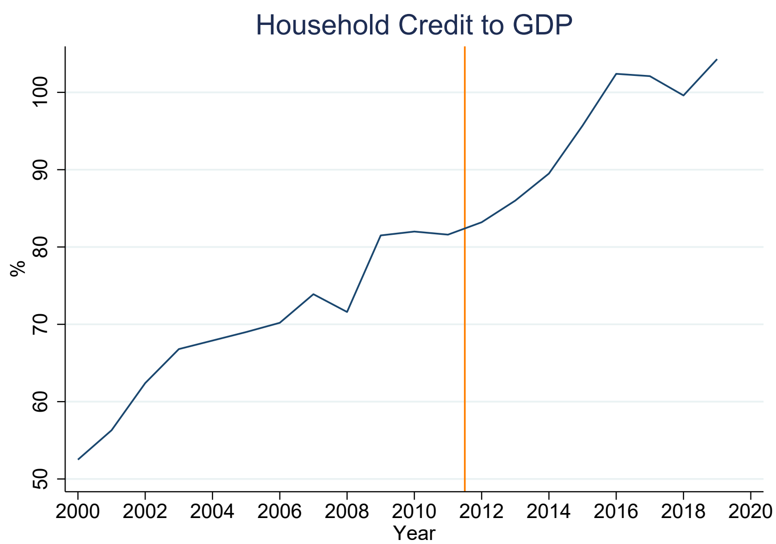


Figure 2: **Receiver Operating Characteristic curve**

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.

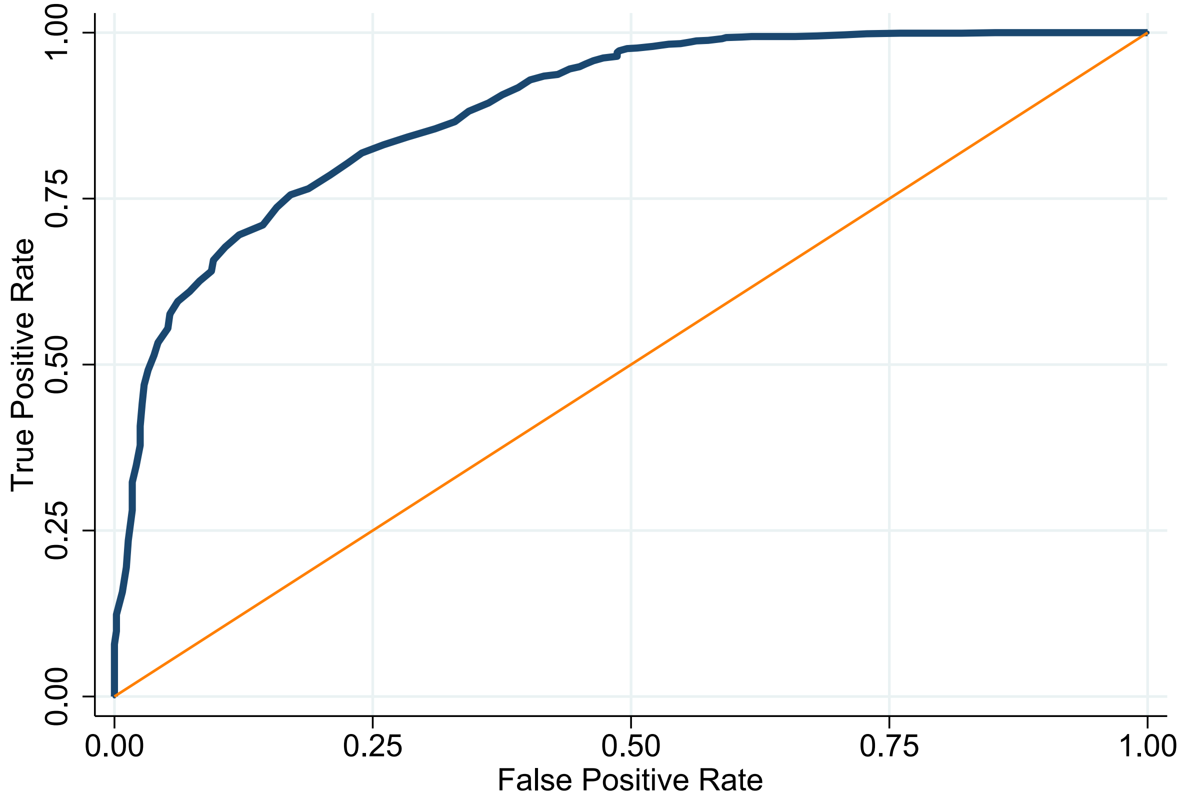


Figure 3: **Variable importance**

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.

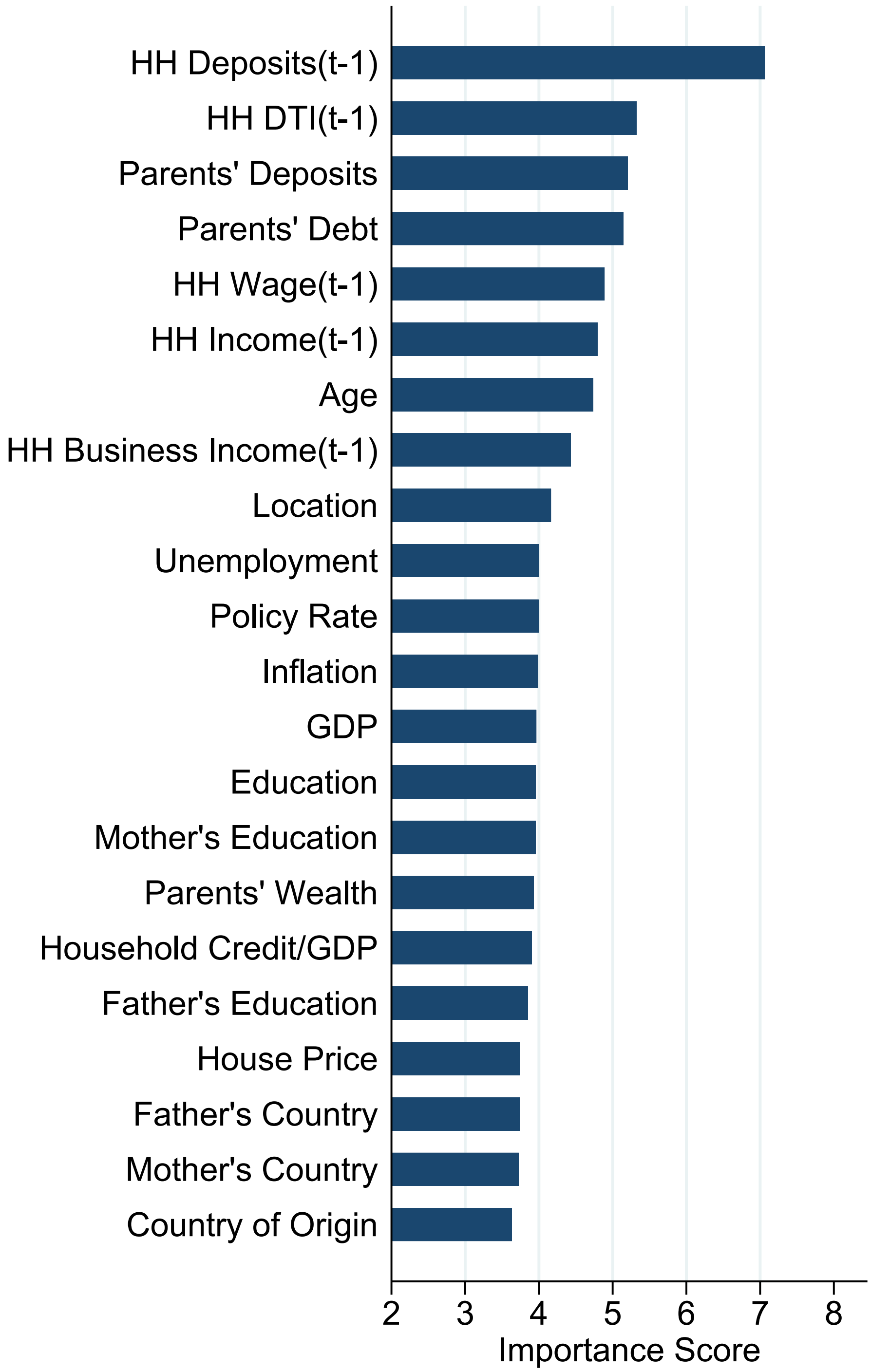


Figure 4: **Dynamic impact of macroprudential policy on DTI ratio**

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.

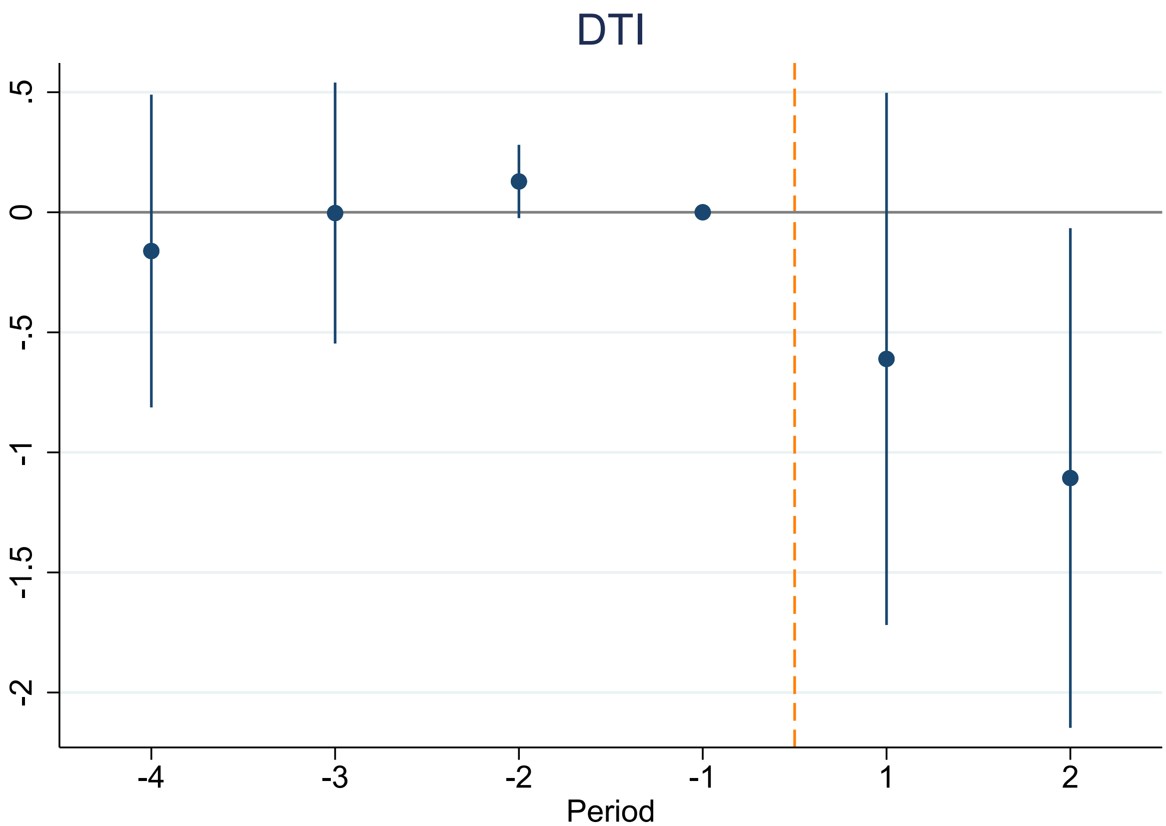


Figure 5: **Dynamic impact of policy on wage growth**

This figure shows the dynamic effect of the LTV policy on wage growth for displaced workers. The sample is individual level data between 2006 and 2013, where leverage is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. Figure shows the 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.

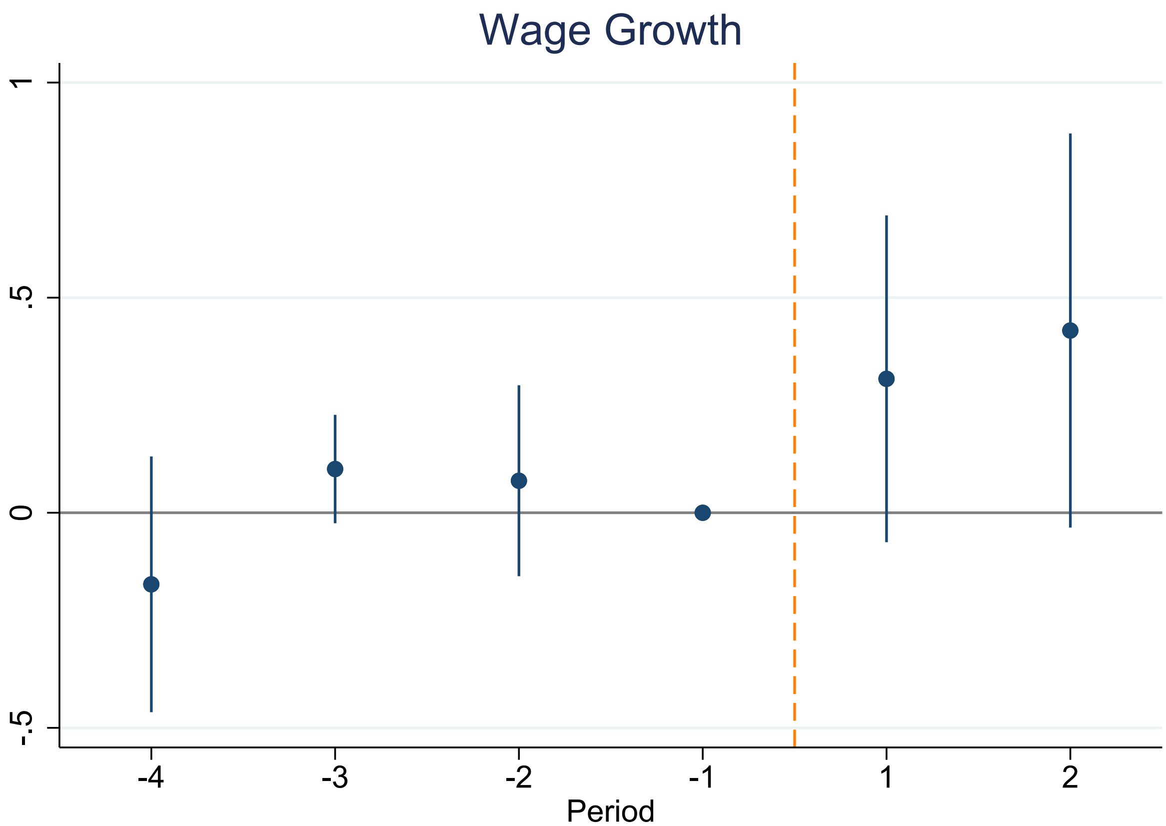


Figure 6: **Transition into Homeownership Rate**

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

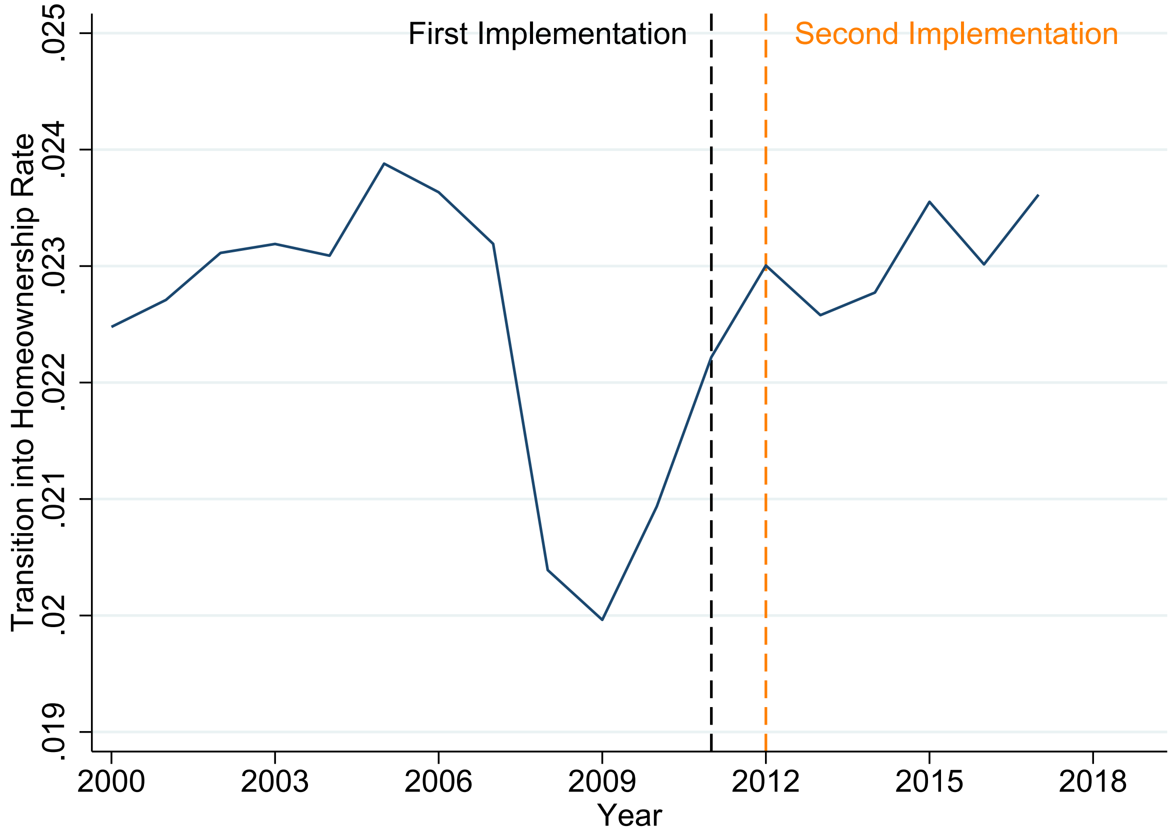


Figure 7: **Different thresholds for the LTV lower bound**

This figure provides a robustness check for the effect of LTV policy on wage growth for displaced workers. The sample is individual level data between 2006 and 2013, where leverage is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. $d(L\hat{T}V > 0.85)$ 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. 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.

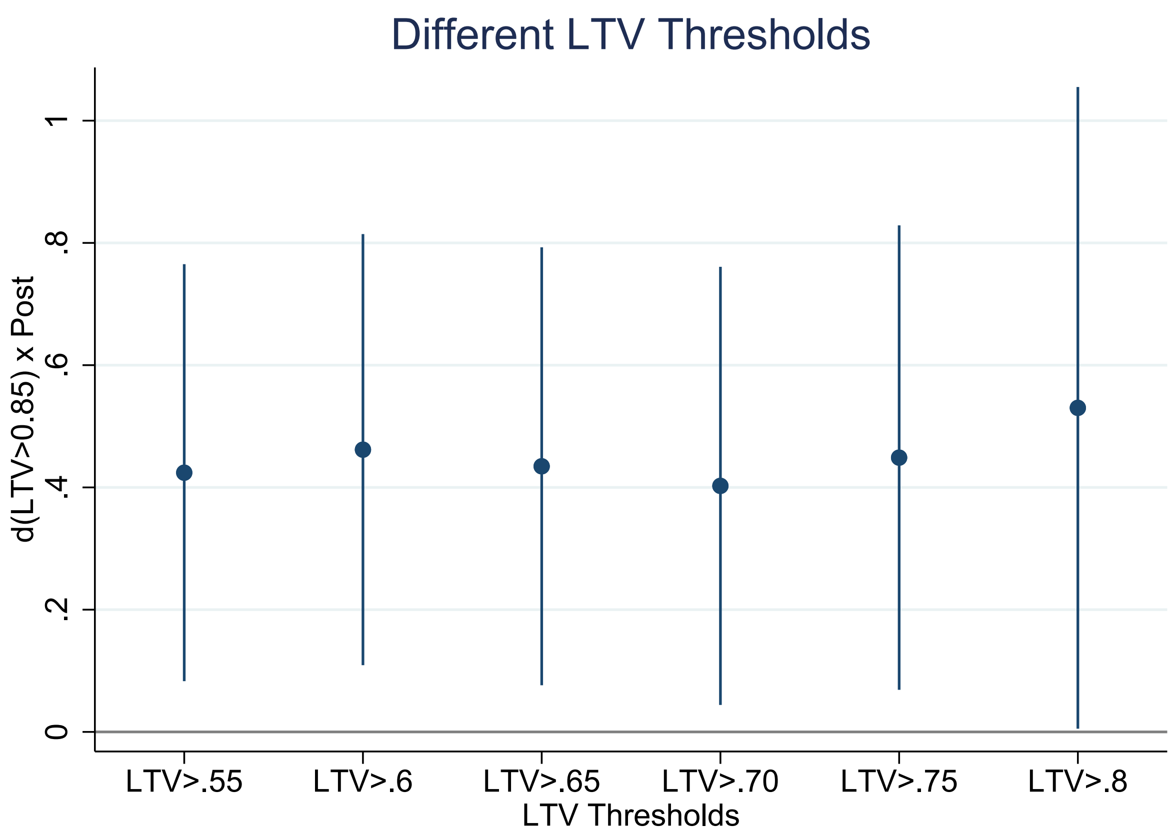


Table 1: **Summary statistics**

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

	$\frac{Debt}{Income}$					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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 wage growth for displaced workers. Each column uses individual level data between 2006 and 2013, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. *Treated* takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value ($d(L\hat{T}V > 0.85)$). *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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). $d(\hat{LTV} > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

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 that cannot afford the down payment before the policy**

This table documents that removing the households that cannot afford the down payment does not affect the impact of the LTV ratio policy on wage growth for displaced workers. Each column uses individual level data between 2006 and 2013, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The households that purchase a house before the policy, obtain a mortgage with an LTV ratio higher than the threshold and do not have enough deposits for the hypothetical down payment are removed from the sample. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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: **External validity: Relaxing sample filters**

This table documents that using the negative relationship between wages and debt exists in wider samples as well. First three columns use individual level data between 2000 and 2017. Column (4) uses data between 2003 and 2013. Column (1) uses the whole population. Column (2) uses individuals who receive unemployment benefits. Column (4) uses individuals who lost their jobs due to mass layoffs. Column (4) uses individuals who lost their jobs due to mass layoffs and bought their houses up to 4 years before being laid off. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

Wage Growth	Full	Unemployed	Displaced	≤4y
	(1)	(2)	(3)	(4)
$\ln(\text{debt})_{t-1}$	-0.026***	-0.052***	-0.019***	
	(0.0001)	(0.0003)	(0.001)	
$d(\widehat{LTV}>0.85) \times$ Post				0.415***
				(0.228)
$d(\widehat{LTV}>0.85)$				-0.125***
				(0.0178)
Fixed Effects:				
Individual FE	✓			
Wage bins FE	✓	✓	✓	
Year FE	✓	✓	✓	✓
Obs.	33,421,099	1,880,454	148,875	8,361
R^2	0.360	0.376	0.116	0.015

Table 8: **External validity: Impact of policy and labor market policies**

This table documents the effect of the LTV ratio policy on wage growth for displaced workers. Each column uses individual level data between 2006 and 2013, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. Column (1) restrict the sample by removing workers whose unemployment spell is longer than 500 days. Column (2) restricts the sample by removing workers whose unemployment spell is longer than 2 years. Column (3) restricts the sample by removing workers whose job tenure at their previous employer is longer than 5 years. *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 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

Wage Growth	Spell		Tenure
	(1)	(2)	(3)
	< 500days	<2 years	<5 years
d($\widehat{LTV}$>0.85) × Post	0.539***	0.494**	0.362*
	(0.172)	(0.178)	(0.189)
d($\widehat{LTV}$>0.85)	-0.129***	-0.131***	-0.152***
	(0.0374)	(0.0374)	(0.0509)
Fixed Effects:			
Year FE	✓	✓	✓
Education FE	✓	✓	✓
Location FE	✓	✓	✓
Industry FE	✓	✓	✓
Obs.	1,700	1,756	1,453
R^2	0.132	0.124	0.133
Mean(Wage Growth)	-0.074		

Table 9: **Comparing workers-in-sample to other workers**

This table compares the workers used in the regression sample to other workers in their previous firms. Each column uses individual level data between 2006 and 2013, where observations between first and second policy implementation are excluded. Independent variable, $Workers-in-sample$, is a dummy variable that takes the value of 1 for workers who are in the regression sample. Column (1) uses workers' age as the dependent variable. Column (2) uses a dummy variable that takes the value of 1 for workers who have higher education as the dependent variable. Column (3) uses a dummy variable that takes the value of 1 for workers who are married as the dependent variable. Column (4) uses a dummy variable that takes the value of 1 for workers who are female as the dependent variable. Column (5) uses a dummy variable that takes the value of 1 for workers who are immigrants as the dependent variable. Column (6) uses lagged wage growth as the dependent variable. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are two-way clustered at firm and year level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Age	Education	Married	Female	Immigrant	Wage Gr.
	(1)	(2)	(3)	(4)	(5)	(6)
Workers-in-sample	-4.759***	0.0231	0.0227	0.0125	0.023	0.0183
	(0.591)	(0.0144)	(0.0165)	(0.0194)	(0.008)	(0.0273)
Fixed Effects:						
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Age FE		✓	✓	✓	✓	✓
Obs.	200,411	200,411	200,411	200,411	200,411	200,411
R^2	0.247	0.186	0.265	0.139	0.104	0.106
Mean(Dependent Var.)	38.471	0.763	0.386	0.424	0.185	0.205

Table 10: **Robustness checks for wage growth**

This table documents the effect of the LTV ratio policy on wage growth for 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 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
	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 11: **Placebo test for wage growth**

This table provides a placebo test for the effect of the LTV ratio restriction on wage growth for 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. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Placebo* is equal to 1 for the years 2009 and 2010 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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 12: **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 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	ln(Unemp. Spell)		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 13: **Impact of policy on job search broadness**

This table documents the effect of the LTV ratio policy on the job search breadth of displaced workers. Each column uses worker level data between 2006 and 2013. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable in Columns (1) and (2) is a dummy variable, which takes the value of 1 if worker changes her occupation in her new employer. Dependent variable in Columns (3) and (4) is a dummy variable, which takes the value of 1 if worker changes the industry in her new employer. Dependent variable in Columns (5) and (6) is a dummy variable, which takes the value of 1 if worker changes her job location in her new employer. $d(\hat{LTV} > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Diff. Occupation		Diff. Industry		Diff. Job Location	
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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 14: **Heterogeneous effects of policy on wage growth**

This table documents the heterogeneous effect of the LTV ratio policy on wage growth for displaced workers. Each column uses worker level data between 2006 and 2013. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. Columns (1) and (2) split the sample in terms of worker age, where "Low" refers to workers younger than 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 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

Wage Growth	Age		Tenure		Education	
	(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 15: **Heterogeneous effects of policy on wage growth**

This table documents that the effect of LTV ratio policy on wage growth for 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 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

Wage Growth	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 16: **Impact of policy on 4-year wage growth**

This table documents the effect of the LTV ratio policy on wage growth for 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. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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 17: **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 the 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 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage 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 the dependent variable and provides visual evidence on the validity of the parallel trends assumptions and the effectiveness of the policy. Relative to the pre-policy baseline year of 2009, the LTV ratio of the treated and control groups evolves similarly in the pre-treatment period, supporting the parallel trends assumption. After implementation of the macroprudential policy, the LTV ratios of treated households' fall significantly relative to the control group. Table A1, presents the estimation results from the corresponding difference-in-differences model in Equation 2 and confirms the visual intuition from Figure 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 equal LTV ratios on average.

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 exemption quota and therefore aren't affected by the treatment. Second, our baseline sample selection we allowed for a wider LTV ratio distribution. Both effects work in the direction of increasing the relative decline in the LTV ratio of treated households.

Next we examine how the macroprudential policy achieves this debt-reducing effect. We therefore inspect how mortgage size, the price of purchased homes, and deposits change and again start by considering the year-by-year effects in Figure 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 on mortgages that are NOK 603,000 smaller mortgages to pay for homes that are NOK 503,000 cheaper.[43] In line with the lack of decline in deposits, we find that the LTV restriction has a similar negative impact on household leverage when it is calculated as (Total Debt - Deposits)/Income (Table A4).

Finally, we look into the policy's influence on households' cash flow. With smaller mortgages, we expect interest payments to decrease mechanically. A reduction in risk may also induce banks to charge a lower risk premium (Elul et al., 2010; Fuster and Willen, 2017; 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 principal repayments, we estimate that households' annual cash outflow improves by NOK 65,000. This is economically sizable and equivalent to about 10 percent of treated households' wages before displacement and 65 percent of the median households' deposits.

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, and immigration status. Our data set allows us to observe the parents

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

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

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

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

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

The $scikit - learn$ library has a built in *variable importance* feature, which calculates the importance by looking at the decrease in the mean impurity. However, this method can overstate the importance of categorical variables with higher cardinality.[44] 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 that are able to obtain mortgages with LTV ratios above the threshold after the LTV restriction. The reason is that these households do not experience a reduction in their leverage, and they should thus not experience a change in their starting wages. Even though this argument is reasonable, it might introduce bias into our estimations. We

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

are able to observe and remove these households that are not affected by the restriction. However, we cannot observe such households before the LTV restriction. Therefore, this removal can change the composition of the treated group before and after the restriction. In Table 5, we show that the characteristics of the treated group is not affected by the LTV restriction. However, there can still be concerns about how this removal affects our estimations for starting wages.

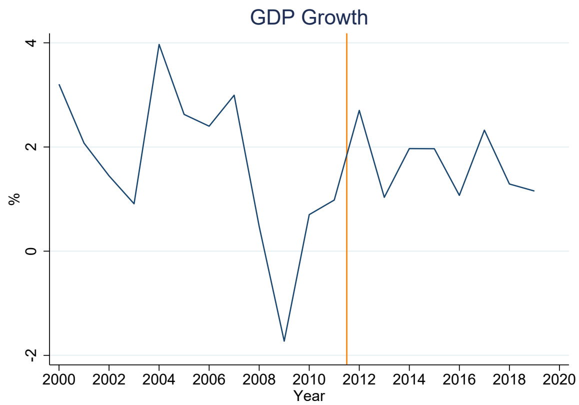
To mitigate these concerns, we adopt a conservative strategy in our placebo test reported in Table 11. That is, we remove the households that obtain mortgages with LTV ratio above the threshold from the placebo post period. Doing this effectively removes the households that would not be affected by the policy and obtain a mortgage with an LTV ratio above the threshold. However, the removed part also includes the households whose LTV ratio would be lowered below the threshold. Thus, there can be still concerns about how this removal affects the estimated coefficients.

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

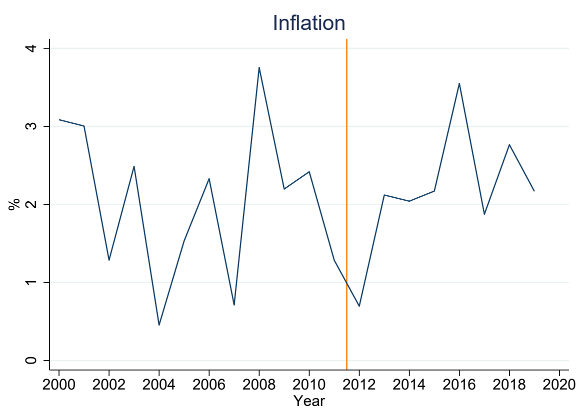
Figure A1: **Macroeconomic conditions**

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.

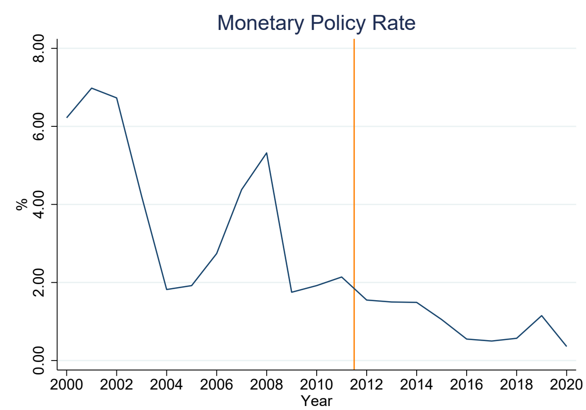


(a)

(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.

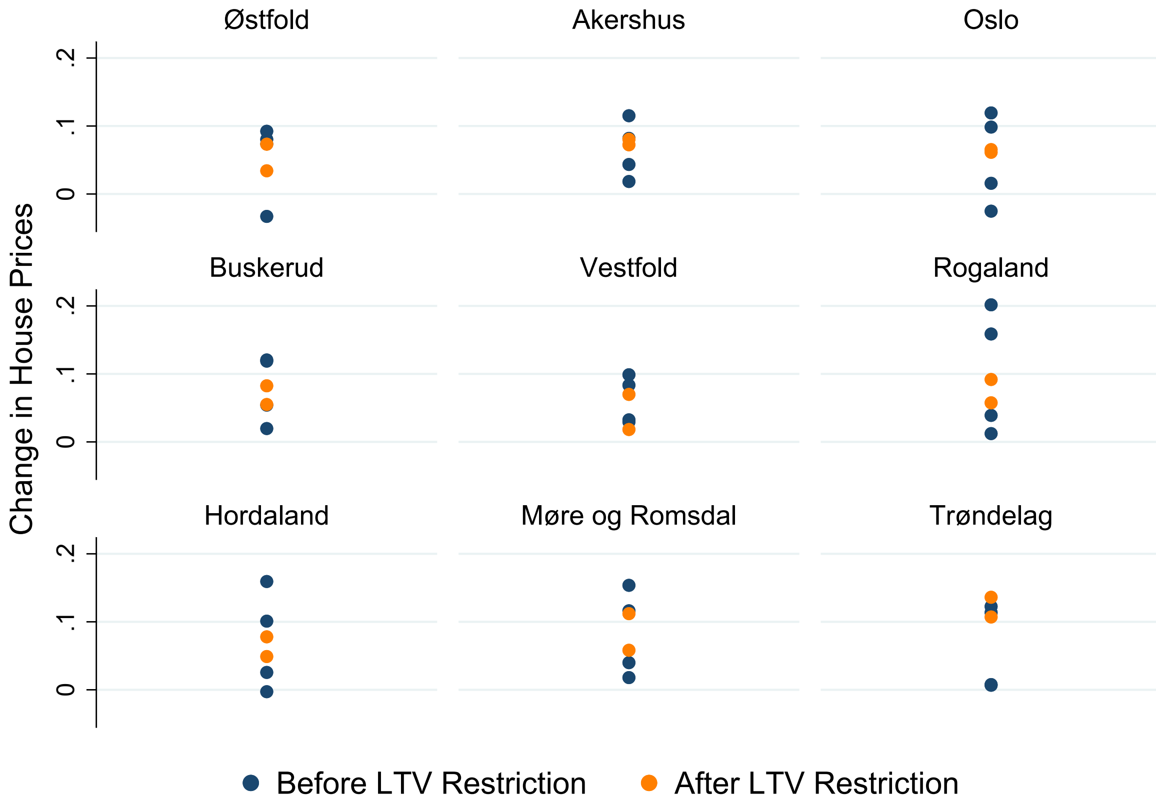


Figure A3: **Classification performance before the LTV ratio restriction**

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

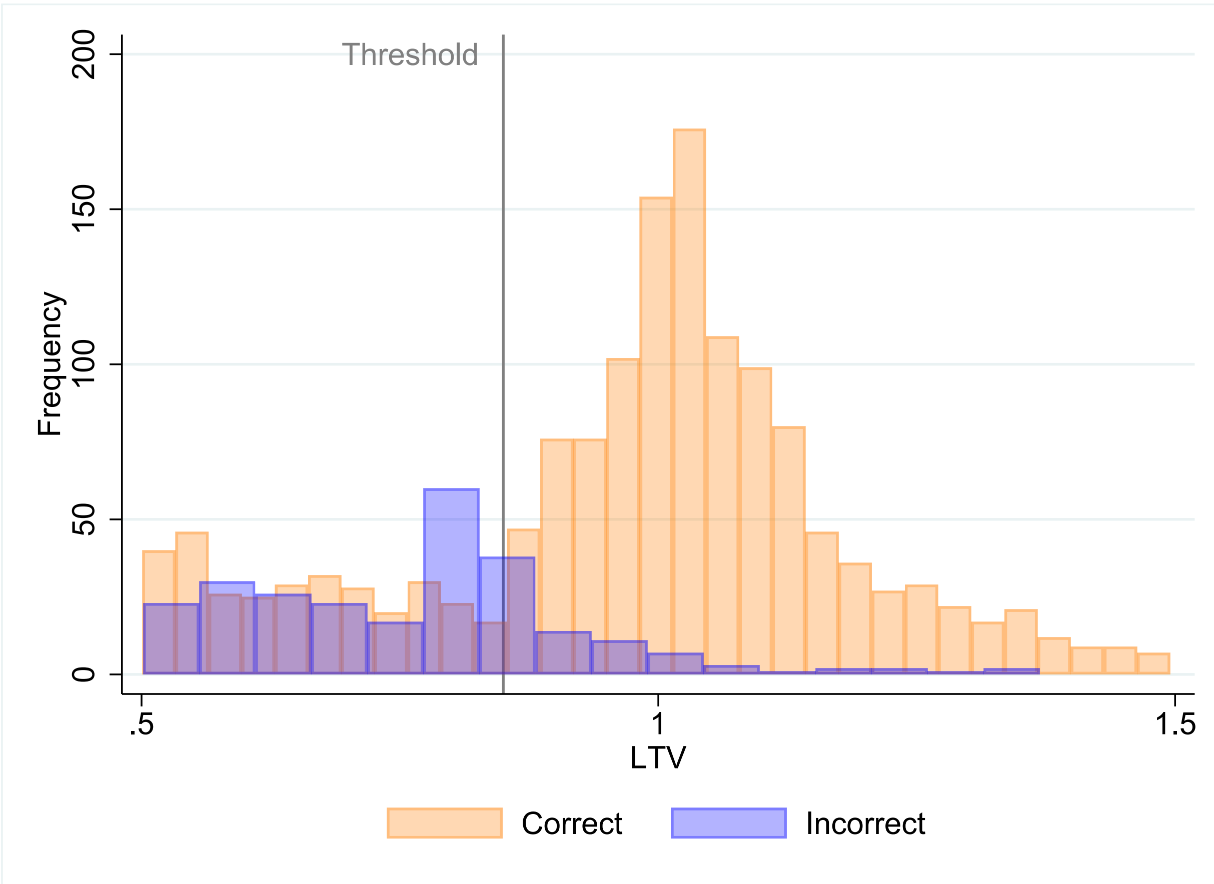


Figure A4: **Introduction of the macroprudential policy and house transaction volume**

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

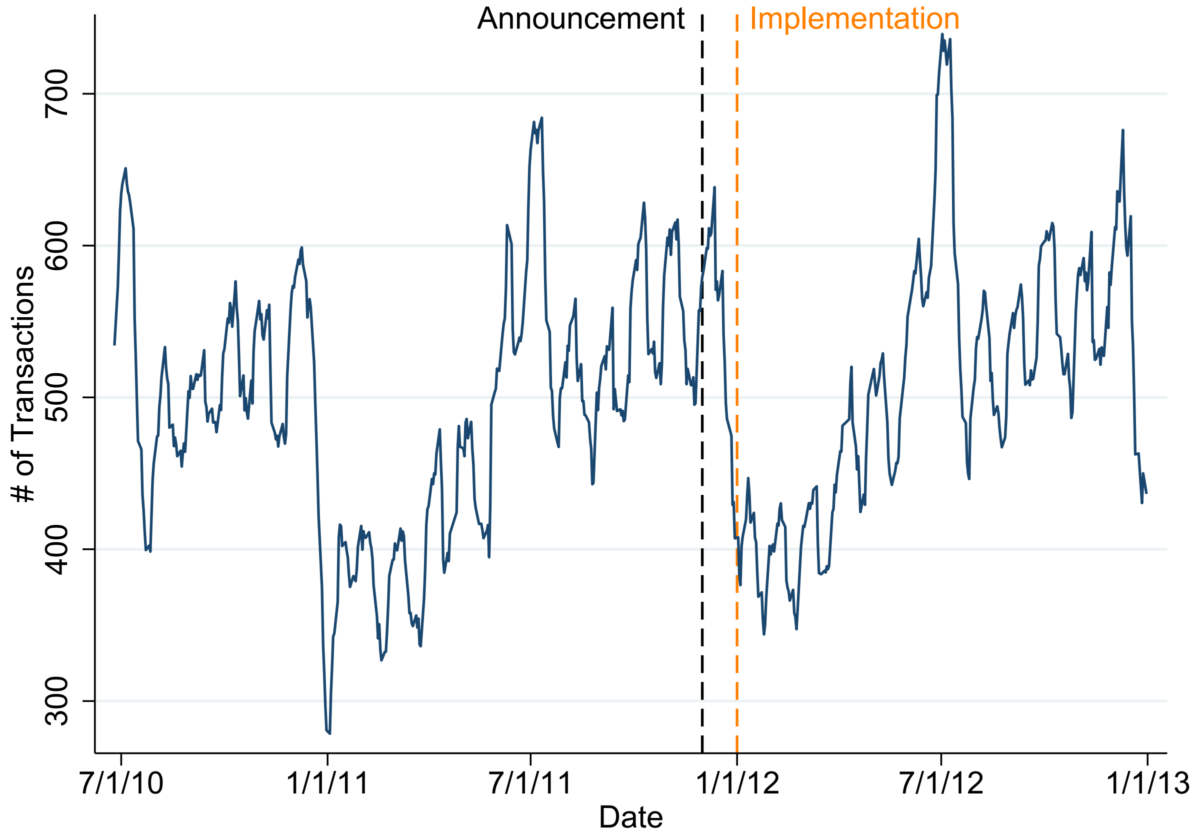


Figure A5: **Dynamic impact of LTV policy on LTV ratio**

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.

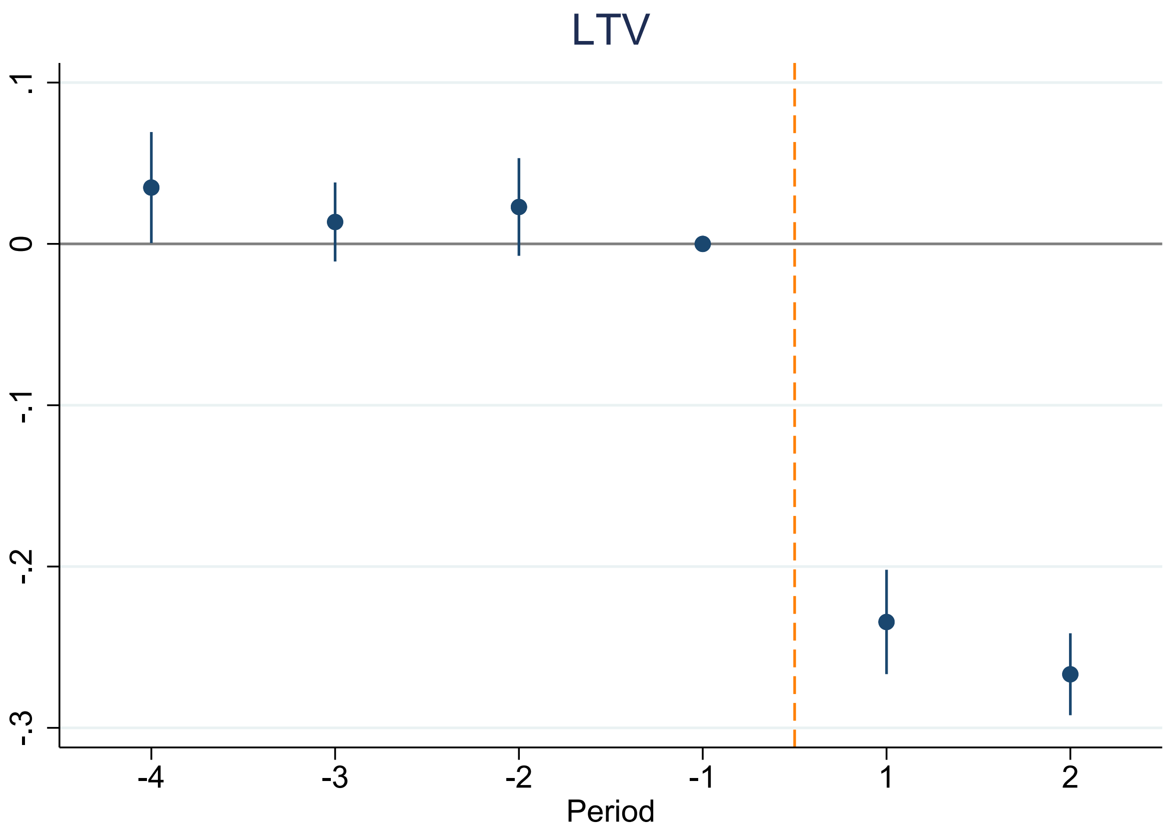
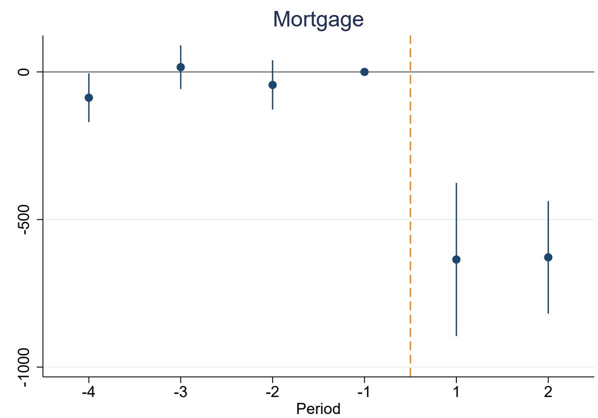


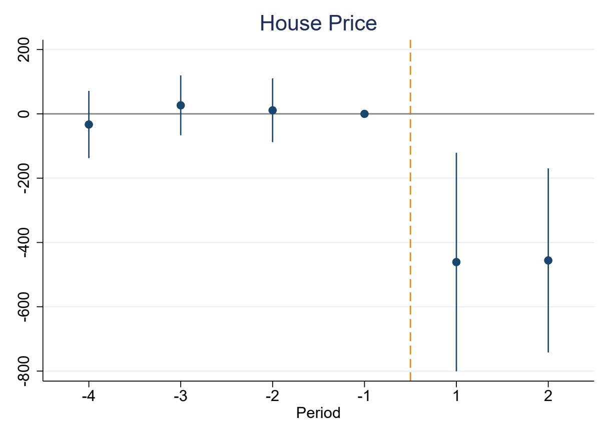
Figure A6: **Dynamic impact of macroprudential policy on mortgages, house prices, and deposits**

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)

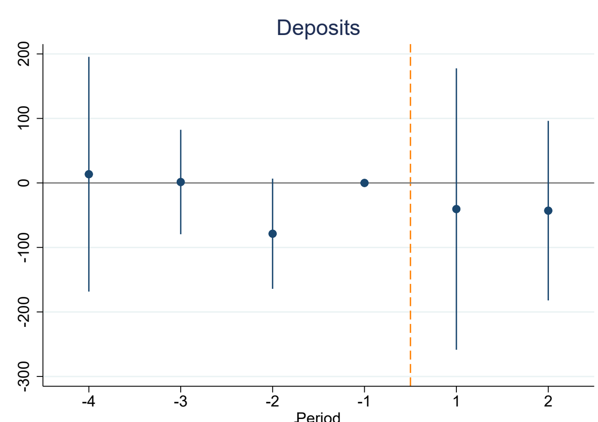


Figure A7: **Dynamic impact of macroprudential policy on interest expense**

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.

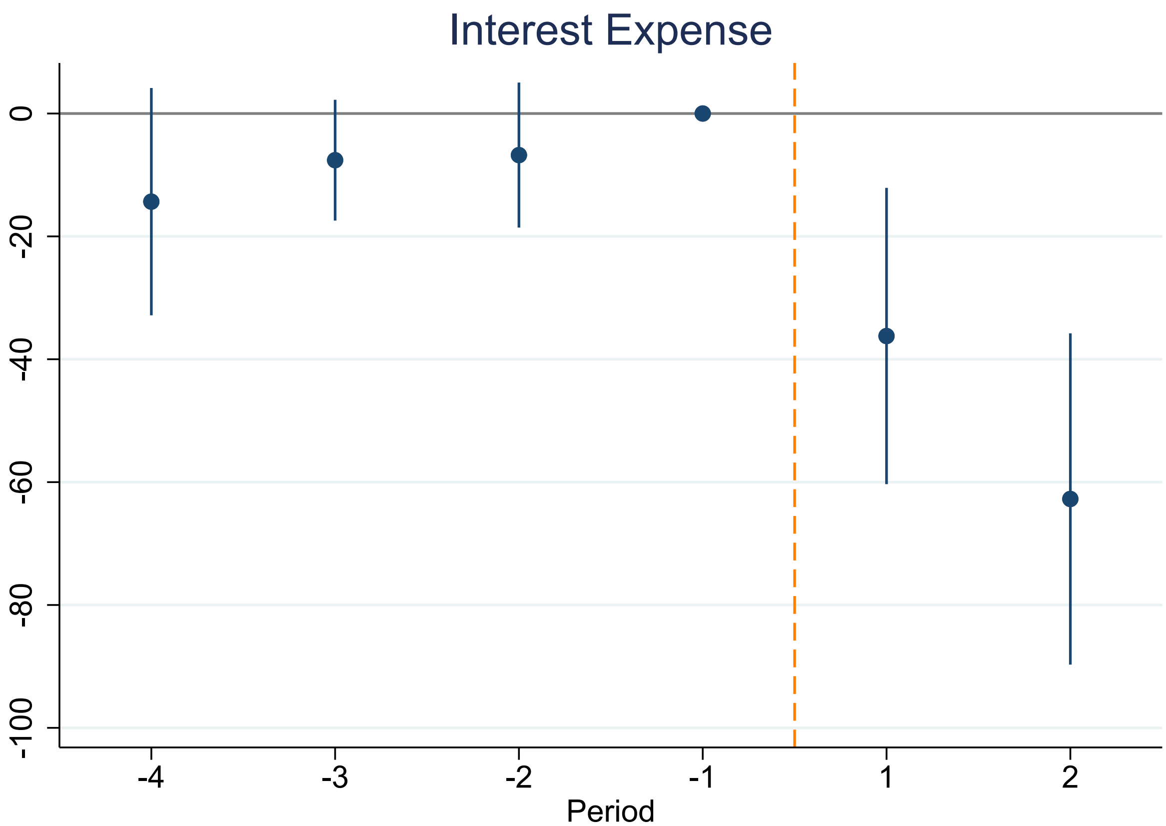


Figure A8: **Variable Importance**

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.

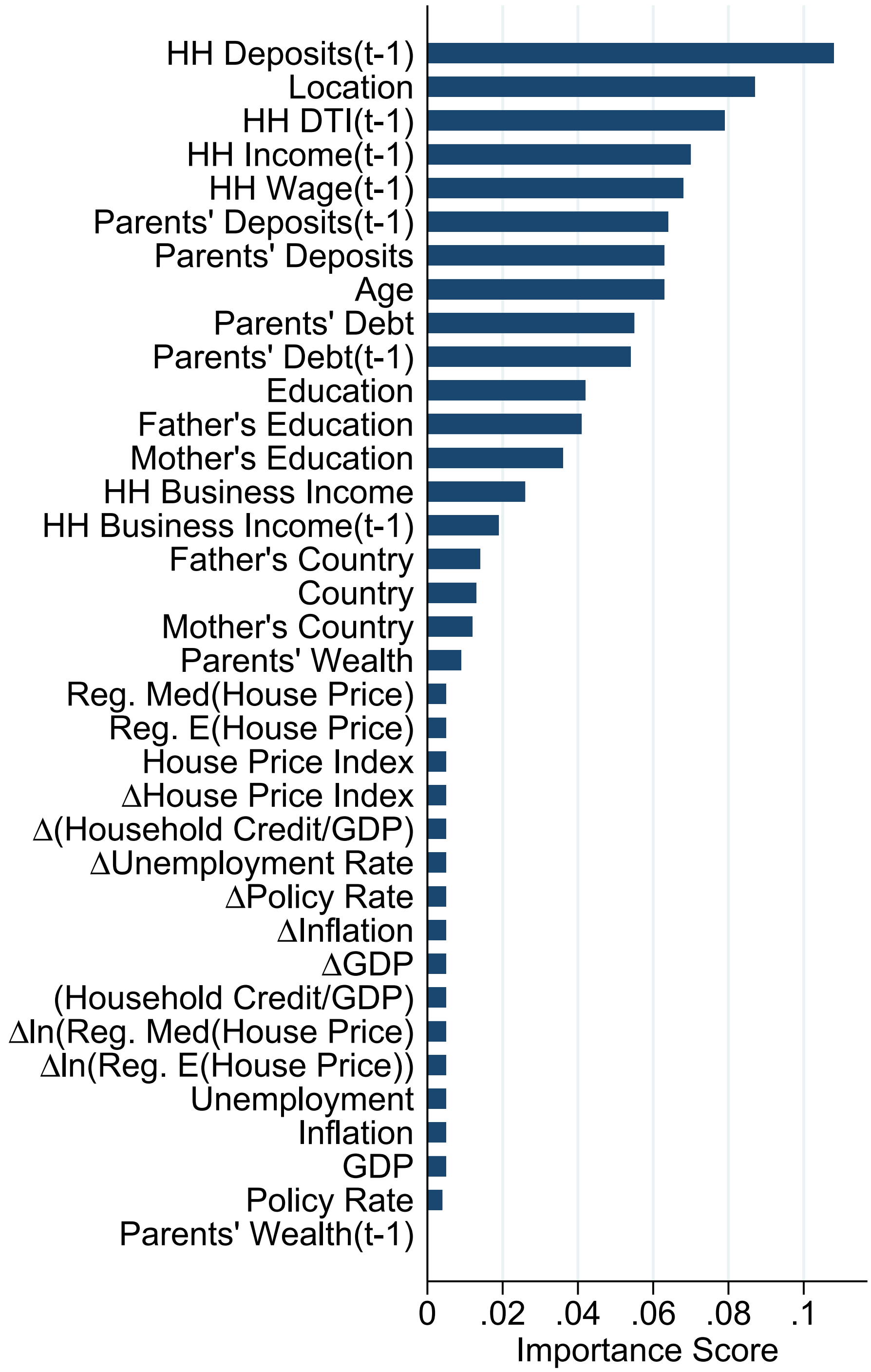


Figure A9: **Simulation exercise for placebo test**

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 four estimated coefficients are significant in the saturated model.

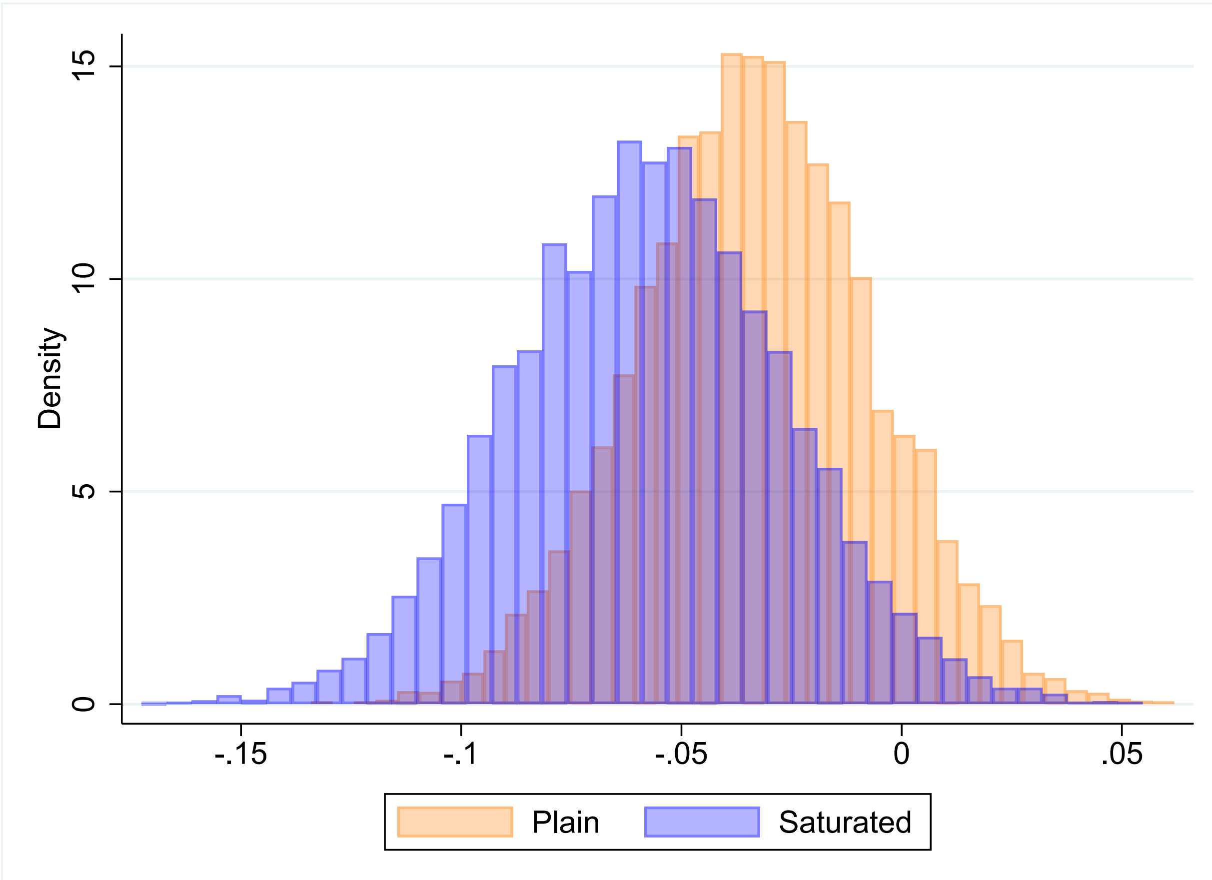


Table A1: **Impact of macroprudential policy on LTV ratio**

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

	LTV					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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. $d(\hat{LTV} > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Mortgage		House Price		Deposits	
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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 expense**

This table documents the effect of the LTV ratio policy on the workers' interest expense. Each column uses worker level data between 2006 and 2013, where interest expense is measured at household level and observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is interest expense, measured in NOK 1000. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Interest Expense					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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 debt (net of deposits)-to-income (DTI) ratios. Each column uses household level data between 2006 and 2013, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is DTI ratio calculated from tax filings and is the ratio of total debt minus deposits to total income. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	$\frac{Debt-Dep.}{Income}$					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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 that cannot afford the down payment before the policy**

This table documents that removing the households that cannot afford the down payment does not affect the impact of the LTV ratio policy on wage growth for displaced workers, after controlling for the available liquidity by including a cubic function of the available liquidity. Each column uses individual level data between 2006 and 2013, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The households that purchase a house before the policy, obtain a mortgage with an LTV ratio higher than the threshold and do not have enough deposits for the hypothetical down payment are removed from the sample. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. *Liquidity* is calculated as the deposits after taking out the down payment required by the LTV ratio restriction for pre- and post-treatment periods. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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 variables differently. Each column uses worker level data between 2006 and 2013, where observations between first and second policy implementation are excluded. The sample consists of workers who lost their jobs due to mass layoffs and bought their houses up to 12 months before being laid off. The sample is restricted to LTV ratios between 0.50 and 1.50. Dependent variable is wage growth between the wage in the previous job and the wage in the new job. $d(L\hat{T}V > 0.85)$ takes the value of 1 if the predicted LTV ratio is larger than then the LTV threshold value. *Post* equals 1 for the years 2012 and 2013 and equals 0 for earlier years. Control variables are indicated at the bottom of each column. Standard errors are two-way clustered at location and industry level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Wage Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
d($\widehat{LTV}$>0.85) × 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					