The Real Effects of Borrower-Based Macroprudential Policy: Evidence from Administrative Household-Level Data*

Sjoerd Van Bekkum Marc Gabarro Rustom M. Irani José-Luis Peydró

March 1, 2024

Abstract

We analyze the effects of borrower-based macroprudential policy at the *household level*. We exploit administrative Dutch tax and housing records in conjunction with the introduction of a mortgage loan-to-value (LTV) limit. We find that the regulation sharply reduces mortgage leverage with bunching at the LTV limit. While (regulation) affected households reduce total leverage and interest expenses, they also decrease cash balances to satisfy the LTV limit, generating an important solvency-liquidity trade-off. Nevertheless, affected households experience less financial distress after the introduction of the LTV regulation. Moreover, these households experience better liquidity management and smoother consumption following income loss. Overall, our results highlight the key financial stability and real effects of borrower-based macroprudential policy.

JEL Classification: E21; E58; G21; G28; G51

Keywords: Macroprudential Policy; Mortgage; Household Leverage; LTV; Solvency-Liquidity Trade-Off; Consumption Smoothing

*Van Bekkum (vanbekkum@ese.eur.nl) and Gabarro (gabarro@ese.eur.nl) are with the Erasmus School of Economics, Irani (corresponding author, rirani@illinois.edu) is with the Gies College of Business at the University of Illinois at Urbana-Champaign and CEPR, and Peydró (j.peydro-alcalde@imperial.ac.uk) is with Imperial College London, ICREA-UPF-BSE, and CEPR. This paper was previously circulated under the title "Take It to the Limit? The Effects of Household Leverage Caps." We thank Aditya Aladangady, Jason Allen, Matteo Crosignani, Filippo De Marco, Anthony DeFusco, Janice Eberly, Andreas Fuster, Debroah Lucas, Christopher Palmer, Adriano Rampini, Lars Svensson, Annette Vissing-Jorgensen, and Paul Willen, as well as audience participants at the 2023 BdF-CEPR-ECB conference on "Monetary Policy Challenges for European Macroeconomies," NBER Summer Institute, American Economic Association Annual Meetings, Federal Reserve Bank of New York, Virginia Tech (Pamplin), Erasmus Rotterdam, Hong Kong University, Hong Kong University of Science and Technology, City University of Hong Kong, Central Bank of Canada Annual Research Conference, Chicago Financial Institutions Conference, USC Fixed Income and Financial Institutions Conference, Financial Intermediation Research Society Conference, Wabash River Finance Conference at Purdue University, European Commission workshop on "Addressing Housing Market Imbalances," the Banca d'Italia and ECB Workshop on "Macroprudential Policy: Effectiveness, Interactions and Spillovers," and the International Banking, Economics, and Finance Association Annual Meetings. Any opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of Statistics Netherlands. All results have been reviewed to ensure that no confidential information is disclosed. Project supported by a 2018 Leonardo Grant for Researchers and Cultural Creators, BBVA Foundation, by the PGC2018-102133-B-I00 (MCIU/AEI/FEDER, UE) grant and the Spanish Ministry of Economy and Competitiveness, through the Severo Ochoa Programme for Centres of Excellence in R&D (SEV-2015-0563), and by the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (grant agreement No 648398).

Household leverage booms have been identified as a key cause of financial crises or generally lower GDP growth, not only in the U.S. and Europe in 2008, but also around the world and in many other time periods (Jorda et al., 2015). Underlying this pattern are households that take on excessive mortgage debt during good times, but go on to struggle with payments, negative equity, and default during the bust (Mian et al., 2013). To curb the build-up of risk during the credit boom, academics and policymakers have advocated for the use of macroprudential tools, especially in highly-levered housing markets (e.g., Freixas et al., 2015; Korinek and Simsek, 2016).

Maximum limits on loan-to-value (LTV) ratios on residential mortgages have proven to be a very popular policy response, since, in principle, they may directly reduce mortgage borrowing thereby restraining household leverage (Gete and Reher, 2016).[1] Consequently, households may be better able to service debt, potentially resulting in fewer defaults and less sensitivity to adverse shocks (Gete and Zecchetto, 2018). Despite the prevalence of such borrower-based macroprudential policies, there is scant empirical evidence on their effectiveness, in particular, how they influence household leverage, liquidity, default, and consumption (Allen and Carletti, 2013). Importantly, there has not been any evidence on the implications of macroprudential policy measures for balance sheets and decision-making *at the household level*, using administrative household-level data, rather than data covering credit market outcomes (e.g., bank lending) or country-level aggregates.

In this paper, we fill this void in the literature by carrying out the first comprehensive study of the effects of macroprudential policy at the household-level. We focus on the first introduction of a limit on the LTV ratios for new mortgages issued in the Netherlands in August 2011. We build a unique data set that matches administrative income and wealth tax filings and property ownership records for the entire Dutch population from Statistics Netherlands to the universe of housing transactions from the Land Registry. These data allow us to understand the efficacy and mechanisms of the LTV limit by observing home purchases, as well as disaggregated data on income, assets and liabilities, and consumption at the household level.

In the first half of the paper, we analyze how households respond to the LTV limit. Our analysis focuses on first-time homebuyers for whom measurement of our variables of interest and the identification of policy effects is most straightforward. In addition, this segment of the population is interesting per se, since lending limits are often criticized for rationing out those most in need of credit (e.g., the young and currently poor).[2] Broadly speaking, we examine the adjustments in household balance sheets—mortgage debt take-up, debt servicing costs, liquidity, and overall leverage—conditional on buying a home. Among these first-time homebuyers, we document a dramatic shift in mortgage issuance satisfying the regulation (i.e., LTV below the limit) in the period following the policy implementation. Moreover, we find a significant bunching of mortgage debt precisely at the LTV limit, an increase from 2 to 20% of issuance within one notch of the limit (see Figure 1).

We measure how the LTV limit affects first-time homebuyers' financial decisions in the year of purchase using a matched-sample analysis. In particular, we conduct a before-versus-after comparison that matches households borrowing within one notch of the LTV limit—that is, ex-post constrained—with observationally similar households borrowing (unconstrained) in the year before the LTV limit. Within our matching framework, we estimate that, on average, at-origination LTV ratios among affected households drop by 4.9 percentage points after policy implementation. These households reduce mortgage debt (9.5 percentage points lower) and buy cheaper homes (5.7 percentage points lower), i.e., the numerator in the LTV ratio falls by more than the denominator.

We then examine the parallel adjustments in household balance sheets. Dutch income and wealth tax records provide each household's stock of assets and liabilities, in addition to the annual flow of income. These data allow us to examine how mortgage debt, the costs of servicing mortgage debt, as well as overall leverage and liquidity evolve in the period immediately following the home purchase. We document an important trade-off between solvency and liquidity as households respond to the policy. We find that households do not replace lower mortgage debt with other credit (e.g., personal loans or credit card debt) to finance their home transaction, so that overall household leverage—and hence debt servicing costs—fall in lock step with the lower mortgage leverage. On the other hand, as a result of the stronger debt reduction compared to house value purchased, households

[1] By 2017, LTV limits had been adopted by about 60% of advanced economies, up from 10% in 2000 (Cerutti et al., 2017, see also, voxeu.org/article/increasing-faith-macroprudential-policies).

[2] As noted by Francesco Mazzaferro of the European Systemic Risk Board: "This is a political issue. A lot of borrower-based initiatives hit younger people and recently married couples who don't have enough money for a downpayment. So they are unpopular in some countries." (see, ft.com/content/6d5ee188-e292-11e9-9743-db5a370481bc).

carry substantially fewer cash reserves—bank deposits and savings—after the purchase. Thus, while the LTV limit improves the solvency position of households, they must reduce liquid assets in the short-term to meet the now-higher upfront costs of buying a home; creating a "solvency-liquidity trade off."

In the second half of the paper, we investigate the economic implications of the LTV limit for households. First, we examine household financial distress. Despite the improvements in solvency, by drawing down on their liquidity buffers to accommodate the borrowing limit, households may have less flexibility to deal with an adverse event in the short run. To investigate this household solvency-liquidity trade off, we conduct two complementary analyses. We first analyze novel mortgage servicing data that details the loan repayment performance of mortgages for a sizable chunk of the market.[3] We find that improvements in household solvency translate into significantly lower mortgage arrears, at least in the short-term (18-month horizon). Furthermore, we examine whether borrowing subject to the LTV limit makes households more "resilient," in terms of liquidity buffers and consumption smoothing for households experiencing income loss. We find that, after the LTV limit comes into effect, now-lower-leverage households experiencing income loss are far less likely to run down bank balances to meet their ongoing expenditures. In addition, after the income loss, these households are better able to sustain (durable goods) consumption, which we proxy for using car ownership records from the Dutch Vehicle Authority. Moreover, improvements in financial resilience (liquidity buffers and consumption smoothing) occur only among low income households for whom high leverage and debt servicing costs are more likely to constrain behavior.

Our paper contributes to the empirical literature on the consequences of macroprudential policies. These policies often place quantitative restrictions on either lenders or borrowers and are usually activated during credit expansions. Moreover, they are predicated on the idea that households do not internalize the effects of their private borrowing decisions on other households via collateral values (i.e., house prices) or aggregate demand (e.g., Korinek and Simsek, 2016). Despite the large interest by policymakers and the growing theoretical macro-finance literature on such policies, there has been limited micro-evidence perhaps due to lack of data availability. Our novel contribution is to comprehensively examine the *household-level* response to borrower-based macroprudential limits using Dutch administrative housing and tax records.

The majority of prior studies conduct cross-country analyses using aggregates (among others, see Cerutti et al., 2017). Significant improvements on the identification front have been made by research incorporating micro-data, which also allows for a better understanding of the underlying transmission mechanisms. Recent papers examining lender-based policies—such as countercyclical capital capital buffers or dynamic loan provisioning—show how the resulting changes in capital requirements, when activated, have effects on credit supplied to firms (e.g., Jiménez et al., 2017) and households (e.g., Basten and Koch, 2015). Acharya et al. (2022) examine bank risk-taking through asset portfolio rebalancing in response to lending limits on residential mortgages in Ireland. Despite the constraint on lending, they find that banks are able to maintain their risk exposure by increasing risk-taking within the mortgage portfolio, as well as increasing exposure to risky corporate debt—consistent with unintended consequences of the policy and regulatory arbitrage. They study a lender-based macroprudential policy: the share of high-LTV or high-LTI loans in the mortgage portfolio that a lender may extend is capped. A central finding of this work is that lender-based macroprudential measures succeed in supporting credit during a crisis rather than curbing a strong credit boom, while, in our case, we find that a borrower-based macroprudential LTV limit is effective at curbing household leverage during a stable economic environment. This outcome arises from the fact that the policy we study is immune to "leakages" across lenders by design (in contrast to, say, Aiyar et al., 2014). Specifically, this borrower-based reform restricts the LTV on all new loans irrespective of the lender (or characteristics of any lender's portfolio of non-exempt mortgage loans). In addition, we find that households do not substitute to less-regulated credit (e.g., unsecured consumer credit) to plug funding shortfalls due to the limit, thus household leverage falls in lockstep with lower leverage coming from mortgage borrowing.

Other research examines the supply-side (e.g., bank-level) effects of borrower-based policies using credit registry data for specific countries. Epure et al. (Forthcoming) use the Romanian credit register that includes all mortgages

[3]Dutch mortgage debt is full recourse and therefore unlikely to go into foreclosure. Nevertheless, since households must continue to pay interest or carry negative equity forward in case of default, recourse mortgages may amplify the adverse effects of liquidity or house price shocks for households and the wider economy (Gete and Zecchetto, 2018; Mian et al., 2013). Furthermore, mortgage arrears may negatively impact household credit histories and therefore employment (Bos et al., 2018), as well as banks' non-performing loans, capital, and credit supply (Jiménez et al., 2017).

granted to households and examine how banks respond to a range of bank- and borrower-based macroprudential instruments over a full credit cycle. These authors emphasize the difference between loans denominated in foreign versus the local currency. Focusing on outcomes in the U.S. mortgage market, DeFusco et al. (2020) provide loan-level evidence that the "ability-to-repay" provision of the Dodd-Frank Act—another borrower-based lending limit—had mild pricing but large quantity effects for the jumbo segment of the residential mortgage market.[4]

Thanks to our unique administrative data, we contribute to this literature by showing the *household-level* effects of regulating mortgage leverage. We demonstrate that regulation can be effective in reducing overall household leverage and debt servicing costs, at the expense of reducing household liquidity in the short-term (creating a solvency-liquidity trade-off). In our context, improvements in households' solvency (despite lower initial liquidity after the home purchase) have positive effects for loan repayment and improve the resilience of households following income loss in terms of consumption smoothing and liquidity buffers. This indicates that borrower-based lending limits may potentially mitigate the severe negative effects of household leverage for defaults and consumption during bad times, a key externality highlighted in theoretical models (e.g., Freixas et al., 2015; Korinek and Simsek, 2016).[5]

1 Institutional Setting and Data

1.1 Macroprudential policy in the Dutch mortgage market

Historically, it was common for a residential mortgage in the Netherlands to have a LTV ratio in excess of 100 at the time of origination.[6] Funds from the loan that were in excess of the home value could be used to finance transaction costs such as property transfer taxes (e.g., a 6% stamp duty as of March 2011), legal and real estate agent fees, moving costs, as well as expenditures on home improvements and durables. Borrowers were happy to carry high levels of mortgage debt due to very favorable tax subsidies—unlimited deductions of mortgage interest from taxable income on a borrower's primary residence—especially households with personal marginal tax rates as high as 52% (Mastrogiacomo and van der Molen, 2015).[7] Since lenders have full recourse—borrowers remain liable for any residual mortgage balance (mortgage value minus home value) even in personal bankruptcy—defaults are very unlikely (e.g., a foreclosure rate of 0.03% in 2010) and high-LTV mortgages could be sustained as an equilibrium. Against this backdrop, household debt-to-GDP stood at 119.6% in 2010, as compared with the 99.2% peak in the United States occurring in 2008.

Beginning in the mid-1980s, Dutch residential real estate prices experienced a long boom that ended abruptly in the second half of 2008. From the peak in 2008:Q3 until the end of 2009, the nominal prices of owner-occupied housing fell by 6.1%. At the same time, given the prevalence of high LTV mortgages, the number of households with negative equity mortgages—those with an underlying real estate value below value of the associated loans—grew by about 31.1%. Household net wealth, consumption, employment, and economic growth declined. The contraction in the Netherlands was more acute than in the rest of Europe—where the buildup in household mortgage debt and leverage was less pronounced—underscoring the vulnerabilities to the economy coming from the housing market.[8]

To limit the potentially harmful effects of boom-bust cycles in property lending and house prices, policymakers instituted mortgage market reforms beginning in 2011. The first notable change were macroprudential lending

[4] Peydró et al. (2023) use loan-level data from the UK to show how a lender-based policy slows down credit supplied to low income households during a boom, resulting in less severe effects for house prices and mortgage defaults during a correction. Benetton (2021) analyzes this UK policy in a structural model. Moreover, outside prudential policies, a handful of finance papers analyze household responses to shocks to debt servicing costs and borrowing capacity occurring during the Great Recession. Notably, Di Maggio et al. (2017) analyze how steep and persistent declines in interest rates on adjustable-rate mortgages enable U.S. households to reduce leverage. Jensen and Johannesen (2017) show how impaired Danish banks reduce lending to their customers, which has negative consequences for household balance sheets.

[5] Building off our work, Aastveit et al. (2022) analyze the liquidity-solvency trade-off in the context of Norway.

[6] Dutch mortgages are typically fixed rate (resetting every 10 years) and 30-year maturity. The majority of mortgages are originated by banks and insurance companies, and subsequently securitized (AFME, 2014).

[7] In 2017, the mortgage interest deduction amounted to 2% of GDP in the Netherlands, as compared with an aggregate subsidy in the U.S. of 0.05% of GDP. The homeownership rate stood at 69% (versus 64% in the U.S.) in the same year. See, economist.com/finance-and-economics/2017/11/09/americas-republicans-take-aim-at-mortgage-subsidies.

[8] See, ftalphaville.ft.com/2016/06/16/2166258/why-is-the-netherlands-doing-so-badly.

limits that were introduced for the first time on residential mortgages via changes in underwriting criteria in the revised Code of Conduct for Mortgage Loans.[9] These rules included a new statutory limit on the LTV ratio that was announced on March 21, 2011, clarified on April 11, 2011, and implemented for mortgages issued after August 1, 2011. The maximum LTV ratio was initially set at 106.[10] The LTV limit applied to all mortgages underwritten in the Netherlands—by banks (both domestic and foreign) and non-banks such as insurance companies—regardless of whether the loan was retained in the balance sheet or distributed.[11]

There were some notable exceptions to the rule so it should therefore be considered a "soft limit." These included two groups of "exempt" households with preexisting mortgages that were permitted to borrow in excess of the limit: first, mortgage refinances where the household does not move and, second, negative equity households selling homes were allowed to finance the residual debt (mortgage value minus sale value) and carry it over to a new mortgage. These households with preexisting homes and mortgages are observable in the data. In addition, the Code of Conduct permitted first-time homebuyers to violate the rule in order to finance certain "energy-saving facilities" that included improvements such as energy efficient windows. Unfortunately, we cannot observe these energy efficient home improvements in the data.

We design our empirical tests to both address and exploit these features of the regulation. First, our tests focus exclusively on the set of first-time homebuyers that are borrowing at the 106-LTV limit after August 1, 2011. These households are clearly constrained by the introduction of the LTV limit. Second, we make use of the (observable) groups of exempted households (i.e., the grandfathered-in households mentioned above) as a way of falsifying our main results.

1.2 Data and measurement

A major challenge in assessing the effects of LTV limits is building an accurate picture of how households respond. We overcome this challenge by analyzing non-public, administrative microdata from the tax authority that covers the universe of Dutch residents. Data on household income and balance sheets (including property ownership records) are provided by Statistics Netherlands, which is also known as the Central Bureau for Statistics (CBS). These data cover our period of interest from 2010 until 2012. We obtain information on the universe of property transactions from the Land Registry (Kadaster). Thus, these data include both the stock and flow of residential real estate in the Netherlands.[12] The data sources are linked together at the individual level through a common personal identification code. We assign individuals to households through tax filings and households to properties (owned versus rented) by connecting property ownership records to the housing register. Our final linked data set contains information on households' assets, liabilities, and income at the annual frequency, as well as homeownership and monthly property transactions.

Homeownership is identified in the data based on tax filings and the housing register (all variables are defined in Appendix Table IA.I). In particular, tax filings indicate whether a household has any mortgage debt on a primary residence. The housing register identifies the household to whom each property is registered and whether it is owner-occupied (as a primary residence or not). Through the Land Registry, we have information on all domestic house purchases, including the month of sale, transaction price, and whether it is owner-occupied or not. We identify households as renters in a given time period if they enter without any reported property (primary residence) and have zero mortgage debt. Naturally, this excludes households that are always homeowners. This reduces the size of our data set from the entire (tax filing) population of Dutch households as of 2010, to 406,981 meeting our data requirements outlined below. Among renters, we are then able to identify first-time homebuyers as households ending the period with an owner-occupied property registered in their name.

[9] See, www.nvb.nl/english/2275/codes-of-conduct.html

[10] This initial limit covered the value of the home plus the six-percent stamp duty at that time. Subsequently, LTV limits were decreased by one percentage point per year beginning January 1, 2013 until it eventually reached 100 on January 1, 2018. Two additional mortgage market reforms came into effect on January 1, 2013, after the end of our event window. First, pre-existing mortgage payment-to-income (PTI) ratio limits were tightened. Second, non-amortizing loans became ineligible for the mortgage interest deduction. We therefore eliminate concerns regarding other confounding policies by focusing on the first introduction of an LTV limit.

[11] In this sense, the policy is immune to "leakages" across lenders (e.g., Aiyar et al., 2014).

[12] Note that our database of housing transactions does not include new construction since, in this case, a house (land plus home with a recorded value) does not change hands in the Land Registry.

Information on household balance sheets comes directly from annual tax filings. Since there is a wealth tax in the Netherlands, we have high-quality data on each household's stock of assets and liabilities, in addition to the flow of labor income over the tax year. Wealth is taxed differently depending on whether it is owner-occupied primary residential real estate, non-residential real estate, or other wealth, and so the tax filings distinguish between real estate, bank deposits, and securities holdings. This data source allows us to proxy for households' financial position with either total wealth (sum of all assets) or liquid assets (bank deposits and savings only), where the latter assets can be liquidated immediately with minimal transaction costs.

The tax filings also detail the liability side of households' balance sheets. The total stock of household debt is itemized into end-of-year mortgage balances (for primary versus other mortgages), student loans, and other debt (which may include credit card debt and other personal loans). Having this broken down by credit type at the level of the household, rather coming directly from credit agreements, is crucial for at least two reasons identified by the prior literature on macroprudential policy leakages. First, to circumvent the regulation, households could substitute to mortgage credit from non-regulated lenders (e.g., foreign banks or domestic non-banks). Second, households could substitute to non-mortgage credit. While the former is benign in our context—the LTV limit applied to all mortgages originated by all lenders in the Netherlands—leakages to less-regulated debt might be an unintended consequence of the policy.

Interest expense paid on mortgages over the calendar year is also itemized in the tax data. We calculate the annual mortgage payment as the reduction in the mortgage balance plus interest expense. For our subsample of first-time homebuyers that transition to owning just one house, this measurement is straightforward. Key measures of household debt and leverage follow naturally: mortgage payment-to-income, mortgage debt-to-income, and total debt-to-income (i.e., overall household leverage accounting for both mortgage and non-mortgage debt). These variables are central to our analysis of how household debt and debt servicing costs evolve in the period immediately before and after a home purchase.

Conditional on a first-time home purchase, we calculate the LTV ratio at the time of origination as the ratio of mortgage amount (declared in the subsequent tax filing) to the actual transaction price of the property, as recorded in the housing registry. There are two potential caveats associated with this measurement. First, while property transaction prices have the advantage of having no missing values in our data, lenders often tie decisions to the *Wet Waardering Onroerende Zaken* (WOZ) value—an administrative measure of property value used for property taxation purposes. Second, mortgage amounts (and thus LTV ratios) may be mechanically lower due to payments occurring during the year of origination.

We apply filters to the data to ensure we are measuring the effects of the lending limit on "ordinary" households. To this end, we focus on the subset of salaried first-time homebuyers (not entrepreneurs, retirees, and so on). We drop households with missing income or negative assets. Households with negative assets most often declare business interests with negative equity. We remove households who own non-residential property (e.g., vacation homes). We also drop households with LTV ratios that are missing or unusually low for the Netherlands (below 80) or high (above 120). Finally, we trim households at the 1st and 99th percentiles of the wealth, income, mortgage size, home value, and debt-to-income, since these households are either extremely indebted (e.g., in personal bankruptcy) or affluent (e.g., members of the royal family).

2 Effects of the LTV Limit on Households' Finances

2.1 Empirical Methodology

Figure 1 shows the striking effect of the August 2011 introduction of the LTV limit. Panel (a) shows the shift in the distribution of mortgages by LTV. In the year prior to the regulation (i.e., from July 31, 2010 until July 31, 2011), the majority of first-time homebuyers' mortgages had LTV ratios in excess of 106. In the following year, the share of mortgages satisfying the regulation (i.e., at-origination LTV below 106) abruptly increases from about 20% to over 65%, a shift that happens over the course of several months, beginning in April 2011 immediately after the announcement of the rule. Furthermore, there is a clear bunching in the density of mortgages at 105 and 106: about 20% of issuance versus 2% in the year before according to panel (b).

Our research design leverages administrative household-level data as well as this policy shock to measure the

effects of the LTV limit. Note that our data is a repeated cross-section—households are only a first-time buyer on one occasion—covering the universe of housing transactions and balance sheet adjustments among first-time homebuyers. We first measure the LTV limit's effect on financing decisions, but, as will become apparent later in the paper, we adapt this framework to analyze other household-level outcomes.

We implement a matching estimator as a nonparametric method to measure the effects of the policy shock (see, Abadie and Imbens, 2006). To identify households that are constrained by the lending limit, we utilize the observed choices of households in the period after policy implementation. We identify "affected households" as those that borrow almost exactly at the LTV limit after the policy is implemented (see Panel (b) of Figure 1). By revealed preference, we assume that these households are constrained by the policy shock. These households are then matched with replacement based on observables to the nearest first-time homebuyer from the period before implementation. These candidate control households include all first-time homebuyers from the year before the policy change, i.e., unconstrained households that do not face an LTV limit. We then report the difference between affected and control households to measure the (local treatment) effect of the policy on mortgage leverage choices, household balance sheets, and so on.

More precisely, among the households from our main sample, we begin with those borrowing at an LTV of 105 or 106 in the year following the policy implementation (1,308 households after below-mentioned restrictions are applied). About 20% of buyers bunch within a notch of the 106 LTV limit—this is the affected group. Each affected household is matched with replacement to a household drawn from the set of 10,479 candidate control first-time home buying households borrowing unconstrained in the before period. The control household must be in the same two-digit post code, and then is a nearest neighbor based on financial variables—contemporaneous *Income*, and lagged *Liquid Assets* and *Wealth*—as well as the family characteristics shown in the summary statistics (Appendix Table IA.II). We match with replacement and based on euclidean distance. We drop matches where the difference between any matching variable (Δ_{i-j}) is in the 5th and 95th percentile of the distribution, and we also discard post codes with fewer than five successful matches. Note also that the matching achieves covariate balance among the two groups of households, i.e., none of the differences between the matching variables are economically meaningful (nor statistically different from zero). We cluster standard errors at the month-of-purchase level but also verify robustness with respect to alternative clustering approaches.

We therefore use a matched sample approach that compares households with an LTV of 105 or 106 (clearly constrained) purchasing after the introduction of the limit with observationally-similar households borrowing unconstrained before the limit. Controlling for observable differences takes care of the key dimensions of borrower financial condition (i.e., age, income, assets, and other debts). The remaining challenge we face here is that this analysis is conditional on a home purchase and therefore subject to a selection problem: the extensive margin homeownership decision distorts the pool of households buying after the policy is implemented. Conceptually, it is unclear if there is positive sample selection (do financially savvy households anticipate the regulation?) or negative sample selection (are weaker households rationed by the regulation?). To show selection concerns are minimized after matching and to validate our empirical approach, we examine "exempt households" (defined above) and households experiencing "life events" (childbirth, marriage, etc.). Exempt households are unaffected by the regulation and so if the pool of home-purchasing-households is changing over time, then we may detect changes in decision-making among this group. In addition, the pool of households experiencing life events may be less likely to strategically time entry into homeownership in response to the introduction of the LTV limit.

2.2 Household balance sheet adjustments

We next examine the adjustments in balance sheets of first-time homebuyers in the year of the house purchase. We first examine the borrowing and housing choices that underpin the adjustment in mortgage leverage. Then, we consider household debt, more broadly defined, as well as liquidity. Our matching estimator incorporates variables intended to capture the important facets of households' balance sheets. These variables are measured in level terms at the end of the year of house purchase. For example, we consider *Liquid Assets*—the level of household liquidity held in bank deposit and savings accounts—in the first tax filing following the home purchase. In this case, we measure the incremental effect of the policy for the liquid asset holdings of affected households vis-a-vis the adjustment in post-purchase liquidity among observationally-equivalent households borrowing unconstrained

before the policy shock. On the one hand, the response could be negative if affected households are now required to make larger down payments on their homes or contribute more towards the transaction costs. On the other, if households buy smaller homes or supplement mortgage debt with loans from other sources of credit that were unaffected by the regulation (e.g., personal loans), then the coefficient could be non-negative.

Table I shows how household debt and liquidity respond to the policy shock. In panel A, we examine housing choices and mortgage credit. Column [1] shows the adjustments in LTV ratios among first-time homebuyers after the implementation of the LTV limit. As intended, and consistent with the (unconditional) graphical evidence, we see that affected households reduce LTVs by 4.9 percentage points. Column [2] considers the (log) mortgage amount, i.e., the numerator in the LTV ratio, as the dependent variable. The point estimate of –0.095 indicates that affected first-time homebuyers reduce borrowing by 9.5 percentage points relative to matched controls. This estimate is significant at the 1% confidence level. Affected households buy houses that are cheaper on average (about 5.7 percentage points, see column [3]), thus decreasing the denominator in the LTV ratio as well. Taking the average home value and mortgage amount in the period before the lending limit (about €206,100 and €222,200, respectively), the estimates indicate that the average affected household borrows €21k less to purchase a house that costs €12k less, a funding gap of about €9k. Note that this gap equals about 100% of liquid assets for the average household buying before the policy shock. Thus, households must reduce liquid assets, borrow from other sources, or cut consumption (or potentially make early pension withdrawals, receive gifts from parents, and so on) to fund the home purchase and transaction costs.

Panel B considers other aspects of household debt and liquidity. We first examine the mortgage payments for households buying homes under the new regime. Column [1] shows that the average annual mortgage payment falls by 8.8 percentage points (significant at the 1% level). Column [2] estimates that the ratio of annual mortgage payment to household income drops with a coefficient of –0.009 (statistically significant at the 1% level). These findings follow from the lower mortgage borrowing by affected households and illustrate how the policy reduces mortgage debt servicing costs.

We next analyze changes in household leverage. We examine the ratios of mortgage debt and total debt to income, where the latter includes student debt and "other" debt (which may include both credit cards and personal loans) in addition to the newly obtained mortgage. This allows us to assess whether households take on other costlier forms of credit in order to finance the housing transaction, which may be an undesirable consequence of the policy. Columns [3] and [4] reject any such substitution effect: we estimate approximately a ten percentage point reduction in both mortgage debt- and total household debt-to-income (both statistically significant at the 1% level). Given that household leverage declines in lockstep with mortgage leverage this indicates that there are no measurable "leakages" from now-regulated mortgage debt to other lightly-regulated sources of credit.

Finally, we examine household liquid assets (bank account balances) in the year of the house purchase. Having shown that debt from other sources does not increase, it seems plausible that households reduce liquid assets in order to finance the home purchase and transaction costs. The estimate in column [5] confirms this intuition: by year-end, household liquidity drops by 33 percentage points. Evidently, affected households reduce a significant share of their cash reserves in order to meet the funding gap.[13]

3 Economic Implications of the LTV Limit

3.1 Effect on mortgage arrears

We have established that the LTV limit is effective at reducing household leverage and debt servicing costs. This suggests that the solvency position of borrowing households will improve and therefore the likelihood of default due to excessive debt will diminish. On the other hand, we have also demonstrated that households reduce liquid assets to meet higher upfront costs of buying a home that result from the LTV limit. Since liquidity shortfalls due to adverse events such as job loss often translate into mortgage repayment difficulties (Bhutta et al., 2017; Elul et al., 2010), in the short-term households may face heightened risks of financial distress. The overall effect on households' mortgage repayment behavior—which we shall now attempt to measure in the data—therefore trades

[13] Appendix A considers a number of extensions and robustness checks of these household balance sheet adjustments.

off the improved solvency against the worsened liquidity position in the short run.

We analyze mortgage repayment using proprietary mortgage servicing data from a Dutch software company combined with publicly-available data from the European Datawarehouse (ED).[14] The ED data contain loan-level information for all loans underlying asset-backed securities (ABS) that may be pledged as collateral in Eurosystem credit operations. This includes a large sample of Dutch mortgages, since these are often securitized (AFME, 2014). The software company data has the identical format, but includes both securitized and balance sheet mortgages for a number of Dutch lenders. Both data sets are compiled to ensure that the loans fulfill ECB reporting requirements. Each loan includes information on the contract (origination date, mortgage size, etc.), underlying property (two-digit postal code and valuation), and borrower (income and employment status). While most fields are fixed at the time of origination, loan repayment performance—whether the loan is currently performing or in arrears or foreclosure—is updated over time on (at least) a quarterly basis.[15] We have been provided with a single snapshot (cross-section) of the data as of the end of 2013, which corresponds to 18 months after the end of our sample. We measure loan repayment performance using payment arrears (for example, Keys et al., 2010), as foreclosures are rare among mortgages issued in our short timeframe. We utilize an indicator variable equal to one if a loan is in arrears as of 18 months after the end of our sample, and zero otherwise.[16]

We examine the impact of the LTV regulation on households' financial health, as measured by mortgage payment arrears. Since Dutch mortgage debt is full recourse, we do not analyze foreclosures which are exceedingly rare in the data (e.g., 0.03% in 2010). It is important to recognize that poor repayment performance (absent foreclosure) is of critical importance for households and lenders. For households, since they must continue to pay interest or carry negative equity forward whether they perform on the loan or not, the LTV limit might be highly consequential in terms of their ability to service mortgage debt and overall financial health (and potentially consumer demand in the aggregate, see Mian and Sufi, 2014).[17] For lenders, delays and delinquencies in mortgage repayment matter for the classification of non-performing loans, which may adversely impact capital charges associated with lending.

To measure the effects of the regulation on arrears, we adapt our matched sample analysis to a loan-level sample of mortgage originations in the mortgage servicing data set. These data are anonymized and cannot be merged with our other administrative data, unfortunately. We construct our sample to approximate the set of salaried first-time homebuyers that are most impacted by the LTV limit, as in our previous tests. We require that the purpose of the mortgage is to purchase a home (as opposed to, say, refinance) and the borrower is a salaried employee (as opposed to a pensioner, student, self-employed individual, etc.). We match households borrowing within one-notch of the 106-LTV limit with (unconstrained) households borrowing beforehand in excess of the limit. Matching with replacement is carried out using at-origination family income as well as an exact match on postal code. The dependent variable in the regression is our measure of loan repayment, *Payment Arrears*, set equal to one if the mortgage is in payment arrears as of 18 months after the end of our sample.

Table II shows that mortgages granted to affected households after the LTV limit came into effect are less likely to enter into payment arrears. Column [1] shows the average effect among the sample of mortgages. The point estimate is –0.008 (statistically significant at the 12% level). Given the average default rate among mortgages issued before the policy shock is 3.3%, a 0.8 percentage point reduction is a sizable effect. Columns [2] and [3] consider subgroups of mortgages based on family income at the time of origination (split at the median of the distribution) to proxy for household financial constraints. We find that the reduction in mortgage arrears

[14] Van Bekkum et al. (2018) provide a more detailed description of the servicing data. The ED data are made available by the European Central Bank (ECB); see, `www.ecb.europa.eu/paym/coll/loanlevel`.

[15] Strict reporting requirements ensure that non-performing loans remain in the data.

[16] Measurement of mortgage repayment using these data has advantages and disadvantages. The data is reliable and accurate, since banks that fail to report are barred from ECB borrowing facilities. The data provide a representative sample of securitized and non-securitized mortgages and include those issued by three of the four largest banks, as well as several smaller lenders (Van Bekkum et al., 2018). The main drawback of the ED data is that they are anonymized and so its borrower and property identifiers cannot be matched to administrative records. To approximate first-time homebuyers, we restrict the sample: first, to mortgages for home purchase, thus excluding refinancing, investment properties, or home equity extraction; second, to borrowers that are employed (i.e., they are not self-employed and must report positive labor income) and do not have other mortgages before the purchase.

[17] Mortgage delinquencies are not costless and may spillover to other important household-level outcomes such as employment and earnings through negative information in credit registers (Bos et al., 2018).

is larger in magnitude (1.4 percentage points) and statistically significant at conventional levels only among low income borrowers. Thus, the reduction in household leverage and debt servicing costs translates into significant improvements in the repayment behavior of borrowers. This is particularly true among the low income households, who tend to be more liquidity constrained and for whom excessively high mortgage leverage and debt payments are more likely to be problematic.

3.2 Do households become more resilient to income loss?

One of the central objectives of borrower-based lending limits is to make household demand (consumption) more resilient *in response to adverse shocks*. To examine whether the policy is successful in this regard, we now test whether debt reductions due to the LTV limit make households better able to handle income loss. Theoretical work points to this outcome (e.g., Korinek and Simsek, 2016), and we therefore consider that, before the LTV limit comes into effect, highly-levered households experiencing negative income shocks may be more likely to liquidate bank accounts to meet the ongoing payments associated with their (full recourse) mortgage debt. As a result, they may also be less likely to sustain consumption. In contrast, after the LTV limit lowers borrowing, deleveraged households may be less likely to exhaust their savings, since mortgage payments are now more manageable. On the other hand, in practice, the success of the policy may depend on how households react. For example, households may be more concerned about rebuilding their depleted liquidity buffers and therefore it is not obvious how they will react in terms of consumption.[18]

We examine this question using our main sample of first-time homebuyers and the following thought experiment. We track these homeowners over three years during which three non-overlapping events occur. In the first year, each household gets a mortgage and buys a house (we label end-of-year-one data with a t subscript). During the subsequent year (labeled $t+1$), households experience an adverse income event. During the final year (labeled $t+2$), we measure households' liquidity position and consumption. As usual, to measure the how the LTV limit affects liquidity buffers and consumption smoothing conditional on income loss, we need to compare first-time homebuyers purchasing before versus after the LTV limit comes into effect.

To operationalize this thought experiment, we turn to our matched sample analysis. As before, we identify affected households as those buying at the lending limit after it was implemented (i.e., at an LTV of 105 or 106 from August 2011 until July 2012). We then take an extra step: we identify 280 households experiencing *any income loss* in the year following the home purchase, i.e., between the end of year t and the end of year $t+1$.[19] We consider two sets of outcome variables, capturing liquidity buffers and consumption smoothing, both measured in the terminal year. For liquidity, we use *log(Liquid Assets)* and *Low Liquidity*, where the latter is an indicator variable set equal to one if the household reaches very low levels of liquidity by the end of year (i.e., the bottom decile of the liquid assets distribution). For consumption, we use vehicles (e.g., cars) as a proxy for durable goods consumption. Focusing on vehicles ("cars") allows us to sidestep pitfalls associated with imputed consumption. The downside of using durable goods consumption, however, is that these are large, sluggish expenditures and so we may be missing part of the picture. We examine the quantity (*Number of Cars*) and quality (total *Car Weight*) of consumption based on administrative vehicle registration data provided to the Dutch government (i.e., the Netherlands Vehicle Authority or RDW). Among households experiencing income loss, our matched sample analysis therefore measures incremental differences in liquidity carried and durables consumption by affected households that bought homes while subject to the lending limit.

Table III shows how reductions in borrowing under the LTV limit interacts with this household-level response to income loss. The table reports average effects (columns [1] and [4]) as well as estimates across households sorted by initial income (columns [2], [3] and [5], [6]). Starting with liquidity, Panel A uses the level of liquid assets as the outcome variable. Column [1] shows that the LTV limit has a positive impact on household liquidity: affected households borrowing subject to the LTV limit (lower realized leverage and mortgage payments) are in a stronger liquidity position after being hit by income loss (about 16.3 percentage points higher liquid assets). This finding becomes much larger in magnitude when we examine the subset of financially constrained households. As shown

[18] Appendix A indicates that affected households quickly rebuild their liquidity in the years following the home purchase.

[19] Approximately 18% of the matched sample experiences income loss. Moreover, within our matched sample analysis, we do not find significant differences between these affected households and matched controls in terms of ex-ante income, liquidity, and wealth.

in column [2], households with low-initial-income have about 27.2 percentage points higher liquid assets after the income loss event (significant at the 5% level). Columns [4] to [6] show that these households are also far less likely to reach extremely low levels of liquidity. Among low-initial-income households buying under the LTV limit, the probability of savings falling within the bottom decile (€710) decreases by 12.9 percentage points after the income loss event, as compared with observationally-similar households that bought before the limit came into effect.

Panel B of Table III shows the sensitivity of durable goods consumption to the loss of income between otherwise comparable high- and low-debt households. Proxying for either quantity or quality effects, a similar pattern emerges in both cases: we see that the low-debt households—those borrowing under the LTV limit—are better able to smooth consumption in response to a loss of income. As with liquidity, these differences are pronounced among low-income households suggesting that lower debt burden reduces their sensitivity to a loss of income.[20]

4 Conclusion

We provide new insights on how *households* respond to leverage using macroprudential lending limits. We focus on the implementation of a lending limit in the Netherlands on August 1, 2011 that, for the first time, restricted the loan-to-value ratios on all new residential mortgages. Our main findings are the following. The regulation reduces mortgage leverage among first-time homeowners with bunching at the LTV limit. Households reduce total leverage and debt servicing costs. They do not less regulated sources of credit to make up for the lower availability of mortgage credit, but instead reduce cash balances to satisfy the limit, which creates a solvency-liquidity trade-off. We find that affected households experience less financial distress, and better liquidity management and smoother consumption following income loss. Finally, the drop in mortgage debt, as well as overall household leverage and debt servicing costs, reduces the likelihood of financial distress and improves economic resilience.

Moving forward, our analysis suggests several important areas for future research. First, asset prices. We have shown the policy reduces homeownership, which may reduce demand pressure and restrain real estate prices. However, we have not analyzed how the lending limits impact other classes of investors (e.g., speculators) or households that bought in the past (and now hold a potentially less liquid asset), and so this still remains an empirical question. Second, macroprudential regulation that targets mortgage markets might have spillovers to housing rental markets both in terms of cost and availability—do rents go up after the reform? These effects may be exacerbated if speculators in the buy-to-let market—a class of owner we exclude from our study—are particularly squeezed by mortgage lending limits. Third, as we show, borrower-based macropudential policies appear to be immune to "leakages" (regulatory arbitrage), in contrast to research analyzing lender-based policies. This difference begs the question of whether lender-based macroprudential policies are more effective when introduced alongside borrower-based measures or not.

[20] Appendix A conducts several specification checks and placebo analyses for this analysis of household resilience.

References

Aastveit, K. A., Juelsrud, R. E., Wold, E. G., 2022. The Leverage-Liquidity Trade-Off of Mortgage Regulation. Working Paper, Norges Bank.

Abadie, A., Imbens, G. W., 2006. Large Sample Properties of Matching Estimators for Average Treatment Effects. Econometrica 74, 235–267.

Acharya, V., Bergant, K., Crosignani, M., Eisert, T., McCann, F., 2022. The Anatomy of the Transmission of Macroprudential Policies. Journal of Finance 77, 2533–2575.

AFME, 2014. High Quality Securitisation for Europe. Association for Financial Markets in Europe.

Aiyar, S., Calomiris, C. W., Wieladek, T., 2014. Does Macro-Prudential Regulation Leak? Evidence from a UK Policy Experiment. Journal of Money, Credit and Banking 46, 181–214.

Allen, F., Carletti, E., 2013. Systemic Risk from Real Estate and Macro-Prudential Regulation. International Journal of Banking, Accounting and Finance 5, 28–48.

Basten, C., Koch, C., 2015. Higher Bank Capital Requirements and Mortgage Pricing: Evidence from the Counter-Cyclical Capital Buffer. Working Paper, University of Zurich.

Benetton, M., 2021. Leverage Regulation and Market Structure: A Structural Model of the U.K. Mortgage Market. Journal of Finance 76, 2997–3053.

Bhutta, N., Dokko, J., Shan, H., 2017. Consumer Ruthlessness and Mortgage Default during the 2007 to 2009 Housing Bust. Journal of Finance 72, 2433–2466.

Bos, M., Breza, E., Liberman, A., 2018. The Labor Market Effects of Credit Market Information. Review of Financial Studies 31, 2005–2037.

Cerutti, E., Claessens, S., Laeven, L., 2017. The Use and Effectiveness of Macroprudential Policies: New Evidence. Journal of Financial Stability 28, 203–224.

DeFusco, A. A., Johnson, S., Mondragon, J., 2020. Regulating Household Leverage. Review of Economic Studies 87, 914–958.

Di Maggio, M., Kermani, A., Keys, B. J., Piskorski, T., Ramcharan, R., Seru, A., Yao, V., 2017. Interest Rate Pass-Through: Mortgage Rates, Household Consumption, and Voluntary Deleveraging. American Economic Review 107, 3550–88.

Elul, R., Souleles, N. S., Chomsisengphet, S., Glennon, D., Hunt, R., 2010. What "Triggers" Mortgage Default? American Economic Review 100, 490–94.

Epure, M., Mihai, I., Minoiu, C., Peydró, J.-L., Forthcoming. Household Credit, Global Financial Cycle, and Macroprudential Policies: Credit Register Evidence from an Emerging Country. Management Science.

Freixas, X., Laeven, L., Peydró, J.-L., 2015. Systemic Risk, Crises and Macroprudential Policy. MIT Press.

Gete, P., Reher, M., 2016. Two Extensive Margins of Credit and Loan-to-Value Policies. Journal of Money, Credit and Banking 48, 1397–1438.

Gete, P., Zecchetto, F., 2018. Mortgage Design and Slow Recoveries: The Role of Recourse and Default. Working Paper, IE Business School.

Jensen, T., Johannesen, N., 2017. The Consumption Effects of the 2007–2008 Financial Crisis: Evidence from Households in Denmark. American Economic Review 107, 3386–3414.

Jiménez, G., Ongena, S., Peydró, J.-L., Saurina, J., 2017. Macroprudential Policy, Countercyclical Bank Capital Buffers, and Credit Supply: Evidence from the Spanish Dynamic Provisioning Experiments. Journal of Political Economy 125, 2126–2177.

Jorda, O., Schularick, M., Taylor, A., 2015. Leveraged Bubbles. Journal of Monetary Economics 76, 1–20.

Keys, B. J., Mukherjee, T., Seru, A., Vig, V., 2010. Did Securitization Lead to Lax Screening? Evidence from Subprime Loans. Quarterly Journal of Economics 125, 307–362.

Korinek, A., Simsek, A., 2016. Liquidity Trap and Excessive Leverage. American Economic Review 106, 699–738.

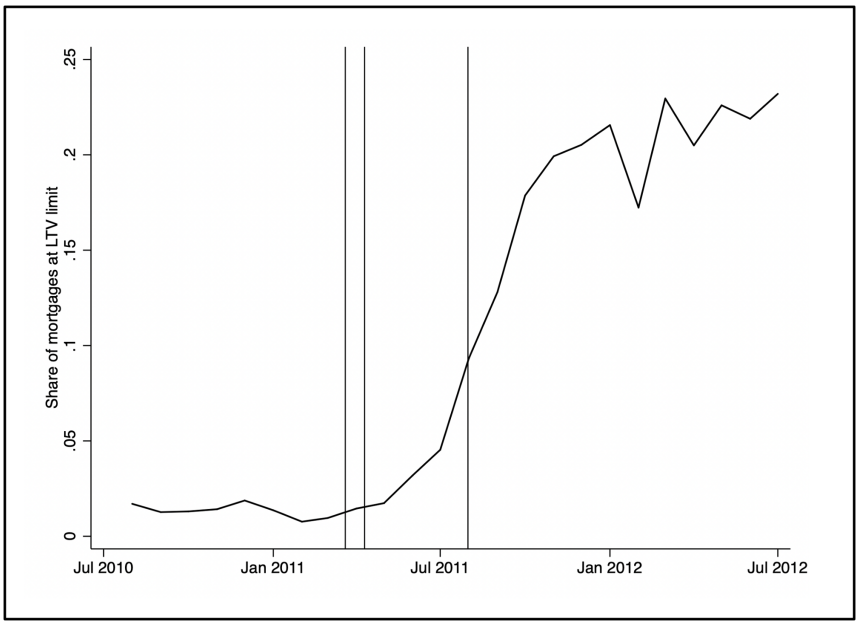
Mastrogiacomo, M., van der Molen, R., 2015. Dutch Mortgages in the DNB Loan Level Data. DNB Occasional Studies 13.

Mian, A., Rao, K., Sufi, A., 2013. Household Balance Sheets, Consumption, and the Economic Slump. Quarterly Journal of Economics 128, 1687–1726.

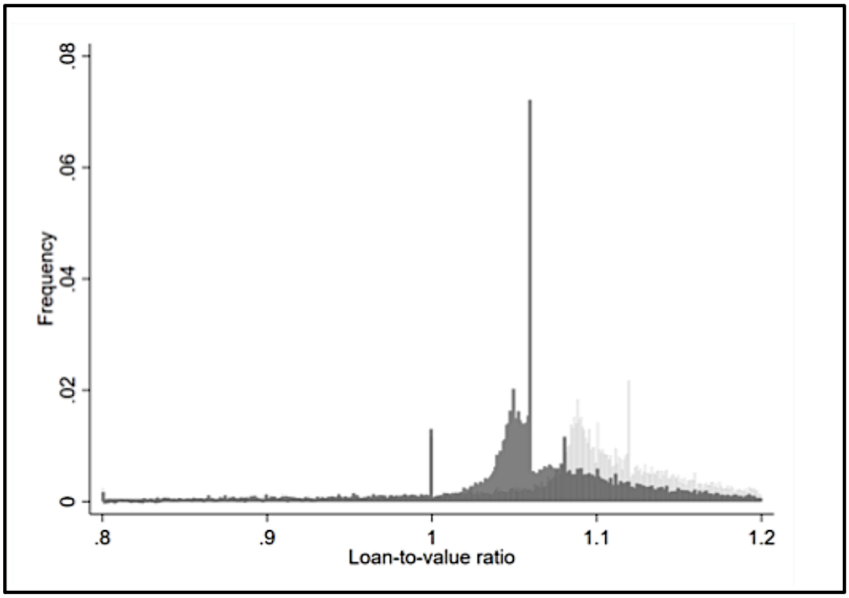
Mian, A., Sufi, A., 2014. What Explains the 2007–2009 Drop in Employment? Econometrica 82, 2197–2223.

Peydró, J.-L., Rodriguez-Tous, F., Tripathy, J., Uluc, A., 2023. Macroprudential Policy, Mortgage Cycles and Distributional Effects: Evidence from the UK. Working Paper, Imperial College London.

Van Bekkum, S., Gabarro, M., Irani, R. M., 2018. Does a Larger Menu Increase Appetite? Collateral Eligibility and Credit Supply. Review of Financial Studies 31, 943–979.



(a) Share at LTV limit



(b) Distribution of LTVs

Figure 1

Effects of LTV limit on mortgage LTVs among first-time homeowners

Panel (a) shows the share of loans at the LTV limit, i.e., at-origination mortgage LTV equal to 105 or 106 among first-time homebuyers. The vertical lines indicate when the rule was announced (March 21, 2011), confirmed (April 11, 2011), and implemented (August 1, 2011). Panel (b) shows the distribution of mortgage LTV ratios both in the year before (light gray) and the year after (dark gray) the implementation of the LTV limit. LTV is calculated as the household's mortgage amount as reported at the end of year that the property was purchased divided by the transaction price. First-time homebuyers do not report any mortgage debt or property ownership in the year prior to purchase. The sample is restricted to "ordinary households" as defined in Section 1.1. Mortgage data comes from Statistics Netherlands (CBS) and transaction prices and property ownership information come from the Land Registry (Kadaster).

Table I
Effect of LTV limit on household balance sheets in year of home purchase

This table shows the shift in households' balance sheets in the year of home purchase among first-time homebuyers buying before and after the implementation of the LTV limit. The unit of observation in each regression is a household. The sample includes homeowners that purchase houses from August 2010 until July 2012. The sample is restricted to LTV ratios between 80 and 120. Affected households borrow at the LTV limit (i.e., LTV equals 105 or 106) in the after period (i.e., from August 2011 until the end of the sample). Each affected household is matched to a household buying unconstrained before the rule change. N households are matched with replacement based on the characteristics shown in Appendix Table IA.II in the year prior to the home purchase. Panel A examines the components of LTV and panel B examines various measures of household debt and liquidity. All variables are defined in Appendix Table IA.I. Standard errors are clustered by month-of-purchase. *, **, and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Panel A: Components of LTV

Dependent variable:	*LTV*	*log(Mortgage Amount)*	*log(Home Value)*
	[1]	[2]	[3]
Affected	–0.049***	–0.095***	–0.057***
	(0.001)	(0.005)	(0.005)
Δ_{i-j} matching vars.	Y	Y	Y
N	1,308	1,308	1,308

Panel B: Household debt and liquidity

Dependent variable:	*log(Mortgage Payment)*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]
Affected	–0.088***	–0.009***	–0.362***	–0.360***	–0.328***
	(0.016)	(0.003)	(0.020)	(0.024)	(0.075)
Δ_{i-j} matching vars.	Y	Y	Y	Y	Y
N	1,248	1,308	1,308	1,308	1,283

Table II
Effect of LTV limit on mortgage repayment performance

This table shows the effects of the LTV limit on mortgage repayment prospects around the implementation of the LTV limit. The unit of observation in each regression is a mortgage. The sample includes mortgages originated for purchase by employed individuals between August 2010 until July 2012. Included mortgages must be used to purchase a home (as opposed to, say, refinance) and the borrower is a salaried employee (as opposed to a pensioner, student, self-employed individual, etc.). N mortgages are matched with replacement to a nearest-neighbor using at-origination family income as well as an exact match on postal code. *Payment Arrears* is an indicator variable equal to one if a loan enters payment arrears and zero otherwise. *Affected* is an indicator equal to one from August 2011 until the end of the sample, and zero otherwise. Affected households borrow at the LTV limit (i.e., LTV equals 105 or 106) in the after period (i.e., from August 2011 until the end of the sample). We split the sample for above median ("High") and below median ("Low") subgroups of income at the time of mortgage origination. All variables are defined in Appendix Table IA.I. Standard errors are clustered by month-of-purchase. *, **, and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Dependent variable: *Payment Arrears*			
Financial constraint based on:		Initial income	
Sample:	All	Low	High
	[1]	[2]	[3]
Affected	–0.008	–0.014**	–0.003
	(0.005)	(0.006)	(0.005)
Δ_{i-j} matching vars.	Y	Y	Y
N	2,334	1,167	1,167

Table III
Sensitivity of household liquidity and consumption following income loss

This table compares households' liquidity position and durable goods consumption—conditional on income loss—in the years surrounding the home purchase among first-time homebuyers buying before and after the implementation of the LTV limit. Households are tracked for three years after home purchase and we require that: during year zero a home is purchased, in year one the household experiences income loss (reported household income declines between year zero and year one), and then at the end of year two household liquidity is measured. In Panel A, liquidity is measured using the natural logarithm of liquid assets and a dummy variable equal to one if the household has liquid assets in the bottom decile of the population distribution of liquid assets. In Panel B, consumption is measured using the number of cars registered to the household and the total weight of cars registered to the household. The unit of observation in each regression is a household. The sample includes homeowners that purchase houses from August 2010 until July 2012. The sample is restricted to households with LTV ratios between 80 and 120. *Affected* households borrow at the LTV limit (i.e., LTV equals 105 or 106) in the after period (i.e., from August 2011 until the end of the sample). Each affected household is matched to a household buying unconstrained before the rule change. N households are matched with replacement based on the characteristics shown in Appendix Table IA.II in the year prior to the home purchase. All variables are defined in Appendix Table IA.I Standard errors are clustered by month-of-purchase. *, **, and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Panel A: Household liquidity position

Dependent variable:	*log(Liquid Assets)*			*Low Liquidity*		
Financial constraint based on:		Initial income			Initial income	
Sample:	All	Low	High	All	Low	High
	[1]	[2]	[3]	[4]	[5]	[6]
Affected	0.163**	0.272**	0.066	–0.080**	–0.129**	–0.026
	(0.064)	(0.091)	(0.103)	(0.035)	(0.050)	(0.031)
Δ_{i-j} matching vars.	Y	Y	Y	Y	Y	Y
N	280	140	140	280	140	140

Panel B: Household durable goods consumption

Dependent variable:	*Num. Cars*			*Car Weight*		
Financial constraint based on:		Initial income			Initial income	
Sample:	All	Low	High	All	Low	High
	[1]	[2]	[3]	[4]	[5]	[6]
Affected	0.188***	0.239**	0.171*	116.4*	199.9**	55.91
	(0.014)	(0.053)	(0.036)	(61.99)	(75.92)	(118.7)
Δ_{i-j} matching vars.	Y	Y	Y	Y	Y	Y
N	280	140	140	280	140	140