Macroprudential Policy and Household Leverage: Micro-Evidence*

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Abstract

We examine the effects of macrorprudential policy on household debt, liquidity, and default. We focus on the first introduction of a limit on loan-to-value ratios on mortgage originations in the Netherlands beginning in August 2011. Using population tax-return and property ownership data linked to the universe of housing transactions, we find that first-time homebuyers most affected by the policy shock reduce household leverage and mortgage debt servicing costs by taking on less mortgage debt. Households consume greater liquidity in the year of home purchase to plug the funding gap. Consequently, mortgage repayment performance improves; however, fewer households transition from renting into ownership, especially liquidity-constrained households.

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Vulnerable household balance sheets have been identified as a proximate cause of financial crises and anemic economic recoveries around the world (Mian et al., 2017; Reinhart and Rogoff, 2009). Underlying this pattern are households that take on excessive mortgage debt and leverage as real estate prices appreciate, but go on to struggle with payments, negative equity, and default during the bust (Mian et al., 2013; Mian and Sufi, 2014). To curb the build-up of risk over the credit cycle, academics and policymakers have advocated for the use of macroprudential tools, especially in highly-levered housing markets (Brunnermeier et al., 2009; Claessens, 2015; Freixas et al., 2015). Maximum limits on loan-to-value (LTV) ratios on residential mortgages have proven to be popular, since, in principle, they constrain leverage directly thereby improving households' resilience to adverse shocks and reducing mortgage defaults (Demyanyk and Van Hemert, 2009; Fuster and Willen, 2017). Despite the prevalence of such borrower-based macroprudential policies, there is limited empirical evidence on their effectiveness (Allen and Carletti, 2013); in particular, how they influence household leverage, liquidity, and default dynamics.

In this paper, we carry out the first comprehensive study of how *households* respond to borrower-based macroprudential lending limits. We focus on the August 2011 introduction of a limit on the LTV ratios for mortgages issued in the Netherlands for the first time.[1] We build a unique data set that matches income and wealth tax 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 observe transitions into homeownership as well as disaggregated data on income, assets, and liabilities at the household level. We restrict our analysis to first-time homebuyers for whom the lending limit was binding and measurement of our variables of interest is unambiguous. Moreover, this segment of the population is interesting per se, since lending limits are often criticized for being regressive

[1]The Netherlands has one of the highest household debt-to-GDP ratios in the world. It also experienced a severe collapse in house prices and slowdown of household debt in the aftermath of the 2008 financial crisis. Section 1.1 provides further detail on why the Netherlands is an important case study.

in the sense that those most in need of credit are rationed out of the market (e.g., the young and currently poor).

We analyze home purchases and parallel adjustments in household balance sheets—mortgage debt take-up, debt servicing costs, and overall leverage—both before and after the introduction of the LTV ratio limit. Our main identification challenge is that changing macroeconomic and financial conditions (e.g., rising interest rates) could cause both the macroprudential policy and the observed adjustments in household finances. While the remarkable shift in "conforming" mortgage issuance (i.e., LTV ratio below the limit) in the months following the policy announcement suggests this is unlikely (see Figure 1), we develop a difference-in-differences (DiD) model that controls for potential time effects. Along the way, we address a secondary identification problem—that we cannot readily identify "affected" households with counterfactual mortgage leverage in violation of the lending limit after it is imposed—by predicting household LTV choices based on characteristics that we do observe in both periods (Abadie, 2005).

Our main findings are as follows. We first establish that the macroprudential lending limits are effective at reducing mortgage leverage ratios among first-time homebuyers. Graphical evidence indicates that households respond by reducing LTV ratios to comply with the new regulation. First, there is a sharp 36 percentage point increase in conforming loan issuance immediately after the policy introduction (Figure 1). Second, there is a significant bunching of mortgage issuance precisely at the LTV limit (Figure 2), an increase from 2% to 20% of issuance within one notch of the limit. Our empirical tests confirm these patterns hold up in our DiD framework: we estimate a 6.4 percentage point drop in the LTV ratios in the wake of the policy shock among affected households. We also examine the cross-section of households and find the decline in LTV ratios is particularly strong among households with lower income, liquidity, and wealth, suggesting that borrowing constrained households are squeezed by the policy shock.

We then provide novel evidence on the connections between macroprudential policy and household balance sheets. Dutch income and wealth tax records provide accurate data on each household's stock of assets and liabilities, in addition to the annual flow of labor income. We use this data to examine how household debt, the costs of servicing mortgage debt, leverage, and liquidity evolve in the period immediately following the home purchase. Our regression evidence uncovers an important trade-off between household solvency and liquidity in response to the policy shock. We demonstrate that households do not replace lower mortgage debt with other sources of credit (e.g., personal loans or credit card debt) to finance the purchase of their first home.[2] Total household leverage and debt servicing costs fall in lock step with the lower mortgage leverage due to the lending limit. As a result, the post-purchase liquid assets of affected households, in the form of private bank deposits and savings, are relatively lower in order to cover the costs associated with the purchase. Thus, the solvency position of households appears to improve, although households must consume liquidity in the short-term to meet higher upfront costs of buying a home.

In the final part of the paper, we investigate two important consequences of this macroprudential policy for households. First, mortgage default. By consuming savings at a greater rate, households may risk liquidity default if an adverse event occurs in the short run (e.g., unemployment). On the other hand, lower debt, leverage, and interest expenses may outweigh these risks over the longer-term. We draw on unique loan-level data that details the loan repayment performance of both securitized and non-securitized mortgages for a sizable segment of the Dutch market. Our key finding is that improvements in household solvency translate into significantly better repayment performance, at least in the short-term (18 month horizon). Moreover, lower mortgage defaults only occur only among low income households for whom excessively high leverage and debt payments are more likely to be

[2]We do not find that affected households purchase relatively more affordable homes after the introduction of the lending limit.

troublesome. Second, homeownership. We take a step back and estimate how the lending limit impacts the extensive margin decision to purchase a house. We revisit the population data to classify households as renters and owners, and measure the rate at which renters transition into buying their first home. We find that after the policy shock this transition rate declines by a meaningful 6.8% of the mean, on average. However, this effect doubles in magnitude among households lacking sufficient liquid assets on-hand. This latter result suggests that households most in need of credit may be more impacted by the LTV limit.

Our paper contributes to the empirical literature on the consequences of macroprudential policies. These policies often place quantitative restrictions on either lenders or borrowers, are usually activated during credit expansions, and are predicated on the idea that households and banks take on excessive risk (Freixas et al., 2015). The majority of prior studies conduct cross-country analyses using aggregates (among others, see Akinci and Olmstead-Rumsey, 2018; Cerutti et al., 2017). Research incorporating micro-data has made significant improvements on the identification front, as well as allowing 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 a dampening effect on credit supplied to firms (e.g., Aiyar et al., 2014; Auer and Ongena, 2016; Jiménez et al., 2017) and households (e.g., Basten and Koch, 2015) over the cycle.

Two recent studies examine the supply-side effects of borrower-based policies using credit registry data for specific countries. Acharya et al. (2018) examine how Irish banks rebalance their asset portfolio in response to lending limits on residential mortgages. 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. In the Romanian context, Epure et al. (2017) use a credit register containing all mortgages and consumer loans granted to households and examine how banks respond to a range of bank- and borrower-based macroprudential instruments over a full credit cycle.

Our novel contribution is to instead focus on the micro-level response of *households* to the borrower-based lending limits using linked housing and tax records for the Dutch population. A handful of finance papers analyze similar household responses to shocks to debt servicing costs and borrowing capacity 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 and increase consumption. Jensen and Johannesen (2017) show how impaired Danish banks reduce lending to their customers, which has negative consequences for household liquidity and consumption. Our results indicate that macroprudential policies that directly constrain mortgage leverage are effective in reducing overall household leverage and debt servicing costs, at the expense of reducing household liquidity in the short-term. Improvements in households' solvency has positive effects for defaults; however, the policy reduces transitions among renters into homeownership, especially among liquidity-constrained households. To the extent that there are positive externalities associated with homeownership (Glaeser and Shapiro, 2003), macroprudential policies targeting the residential mortgage market could have important welfare consequences.

The next section describes the institutional setting in the Netherlands and presents our data, variable construction, and summary statistics. Section 2 discusses the empirical strategy, whereas Section 3 presents and discusses the results. Section 4 concludes with some avenues for future research.

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 loan-to-value (LTV) ratio in excess of 100 at the time of origination. Funds from the loan that were in excess of the home value were often permitted by the lender to be used to finance transaction costs, including property transfer taxes (e.g., a 6% stamp duty as of March 2011), and home improvements. 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).[3] Since lenders have full recourse—borrowers remain liable for any residual mortgage balance (mortgage value minus home value) even in personal bankruptcy—mortgages with high LTV ratios 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 until the trough in 2013, nominal house prices fell by about 20% (about 25% in real terms). Given the prevalence of high LTV mortgages, the fraction of negative equity mortgages—those with an underlying real estate value below value of the associated loans—increased from 5% to 30% over the same time period. Home equity, household net worth, consumption, employment, and economic growth all collapsed. The contraction in the Netherlands was more severe than in neighboring Belgium—where the buildup in household mortgage debt and leverage was far more limited—underscoring the vulnerabilities coming from the housing market.[4]

To limit the potentially harmful effects of boom-bust cycles in property lending and house prices, policymakers instituted a number of mortgage market reforms. Notably, the mortgage interest deduction was curbed, especially for interest-only loans, and macroprudential lending limits were introduced on mortgages. Legally-binding changes in underwriting criteria for

[3] In 2017, the Dutch mortgage interest deduction amounts to 2% of GDP. In the U.S., the subsidy stood at 0.05% of GDP; see, `www.economist.com/finance-and-economics/2017/11/09/americas-republicans-take-aim-at-mortgage-subsidies`.

[4] For example, `ftalphaville.ft.com/2016/06/16/2166258/why-is-the-netherlands-doing-so-badly`.

residential mortgages were introduced in the revised Code of Conduct for Mortgage Loans in 2011.[5] These rules included a statutory limit on the LTV ratio that was announced on March 21, 2011, clarified on April 11, 2011, and implemented for new mortgages issued after August 1, 2011.[6] The maximum LTV ratio was initially set at 106. The limit applied to all mortgages underwritten by domestic and foreign banks, as well as non-banks (e.g., insurance companies), with two main exceptions: first, mortgage refinances where the household does not move; 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. Notably, the 106 LTV ratio limit applies most cleanly to first-time homebuyers seeking a mortgage loan after August 1, 2011. These households will be the focus of this study.

1.2 Data and Summary Statistics

Our analysis is based on non-public microdata from the tax authority that covers the universe of Dutch residents throughout our period of interest from 2010 until 2012. 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). We obtain information on the universe of property transactions from the Land Registry (Kadaster). Thus, the data include both the stock and flow of residential real estate in the Netherlands. 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) through property ownership records in 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 property

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

[6]Afterwards, LTV ratio limits decreased by one percentage point per year beginning January 1, 2013 until it eventually reached 100 on January 1, 2018. In addition, pre-existing mortgage payment-to-income (PTI) ratios were tightened on January 1, 2011 and again on January 1, 2013.

transactions at the monthly frequency.

Homeownership is identified in the data based on tax filings and the housing register.[7] In particular, tax filings indicate whether a household has any mortgage debt on a primary residence. The housing register identifies the person 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 label 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 greatly reduces the size of our data set from the entire (tax filing) population of Dutch households to only 1,376,729 with valid data. Among the set of renters, we then identify first-time homebuyers as households ending the period with an owner-occupied property registered in their name: 15,367 do so in the year before the LTV limit was introduce and 21,192 in the year after.

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 flow of labor income over the tax year. Wealth is taxed differently depending on whether it is residential real estate, non-residential real estate, or other wealth, and so the tax filings distinguish between real estate, bank deposits, bonds, and direct holdings (or indirect holdings via investment funds) of equity. The latter may include entrepreneurs' wealth from business ownership, although we exclude self-employed households—who self-identify in the tax data—to simplify our analysis and its interpretation. This data source allows us to proxy for households' financial position with wealth (total assets) and family income (pre-tax labor-related income, measured over the year). We measure liquid assets as bank deposits and savings, since these funds can be liquidated

[7]All variables are defined in Appendix A.

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, student loans, and other debt (including credit card debt and other personal loans). Interest expense paid on mortgages over the calendar year is also itemized. 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. 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 unambiguously calculate the LTV ratio at the time of origination as the ratio of end-of-year mortgage amount (declared in the subsequent tax filing) to the actual transaction price of the property, as recorded in the housing registry. The only caveat with this measurement is that mortgage amounts (and thus LTV ratios) may be mechanically lower due to payments occurring during the year of origination.

We apply some minimal filters to the data to ensure we are measuring the effects of the lending limit on ordinary households. To this end, we drop households with missing income or negative assets. Households with negative assets most often declare business interests with negative equity, although they do not self-identify as entrepreneurs. We remove institutional households (e.g., living in a retirement home) and first-time buyers who own non-residential property (e.g., vacation homes). We also drop households with LTV ratios that are missing or unusually low (below 80) or high (above 120). Finally, we trim households at the 1st and 99th percentiles of the wealth, income, mortgage size, home value, debt-to-income, and interest expense, since these households are either extremely indebted (e.g., in personal bankruptcy) or affluent (e.g., members of the royal family).

We analyze mortgage default using proprietary data from a Dutch software company combined with publicly-available data from the European Datawarehouse (ED).[8] 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 non-securitized mortgages for a number of Dutch lenders. Both data sets are compiled to ensure that the loans fulfill reporting requirements set by the ECB. Each loan includes information on the contract (origination date, mortgage size, etc.), underlying property (two-digit postal code and valuation), and borrower (labor 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.[9] We measure repayment performance using payment arrears (for example, Keys et al., 2010), as foreclosures are rare among mortgages issued in our short timeframe (less than 0.05%). Precisely, we define an indicator variable equal to one if a loan is in arrears 18 months after the end of our sample (end of 2013) and zero otherwise.

Measurement of mortgage default 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 includes 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 we cannot isolate first-time homebuyers. They are not flagged in the data. While the data

[8] Van Bekkum et al. (2018) provide a more detailed data description of the proprietary data. The ED data are made available under the loan-level initiative of the European Central Bank (ECB); see, `www.ecb.europa.eu/paym/coll/loanlevel`.

[9] Strict reporting requirements ensure that non-performing loans remain in the asset pool underlying ABS and therefore do not drop out of the data.

do provide borrower and property identifiers, they are anonymized and so they 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 not self-employed or an institution and they must report positive labor income) and do not have other mortgages before the purchase (i.e., reported in the data).

Table I presents the sample summary statistics. We cut the data based on time period before and after introduction of the 106 LTV limit. Panel A shows information on first-time homebuyers home purchase transactions as well as their balance sheets. While the number of transactions does not decline, the LTV, mortgage size, and property value unconditional distributions show a leftwards shift after the regulation is introduced. For example, the average (median) LTV falls from 1.083 (1.096) to 1.054 (1.059) suggesting that the new restrictions bind. Household leverage ratios appear to improve as households take up less debt. Panels B and C underscore the tradeoffs involved with the policy. Panel B shows that mortgage default rates are lower among mortgages originated after the policy change (3.3% end up in arrears before versus 2.9% after), whereas panel C suggests that fewer renters transition into homeownership (4.4% before versus 3.1% after).

2 Empirical Methodology

Our data is a repeated cross-section covering the universe of housing transactions by first-time homebuyers. To measure the impact of the lending limit on the LTV ratios for these households, we first estimate the following regression using ordinary least squares (OLS):

$$y_{hlt} = \alpha_l + \beta \; After_t + \theta' \mathbf{X}_{ht} + \epsilon_{hlt}, \tag{1}$$

where h indexes households, l indexes house locations (two-digit postal codes), and t indexes time (months). In the first instance, the dependent variable, y_{hlt}, will be either LTV_{hlt} itself or a dummy variable for whether it is greater than the 106 limit, $d(LTV_{hlt} >106)$.[10] $After_t$ is an indicator variable equal to one in the months after the lending limit was implemented (August 2011 until July 2012), and zero otherwise (August 2010 until July 2011). The postal code fixed effects (α_l) control for fixed differences across regions, such as housing affordability. $\mathbf{X}_{ht}$ is a vector of control variables measured at the household-level, and ϵ_{hlt} is the error term. Since households only appear in the sample once in a cross-sectional regression (that is, in the month they purchase a home), we cluster standard errors at the origination month level (Petersen, 2009). The main parameter of interest, β, measures the average households' LTV ratio in the year following the introduction of the LTV ratio limit relative to (unconstrained) households receiving mortgages in the year before. If the LTV limit binds for at least some first-time homebuyers then our estimate of β will be strictly negative. Identification of β in Equation (1) requires that borrowers (or lenders) do not anticipate the policy and that there are no confounding macroeconomic or financial events (e.g., interest rate rises) driving the both the macroprudential policy response and the change in the use of debt conditional on home purchase.

The dramatic shift in "conforming" loan issuance (i.e., LTV ratio below 106) in a tight window around the policy announcement and implementation shown in Figure 1 suggests that these identifying assumptions are reasonable. Nevertheless, to buttress our approach we consider a difference-in-differences (DiD) framework, which allows us to further control for time effects. In addition, this allows us to measure the effects of the lending limit on borrowers that, arguably, are more likely to run up against the LTV ratio constraint. The main obstacle in estimating a DiD model using our repeated cross-section data is that we cannot readily identify a group of "affected" households after the lending limit is imposed.

[10] We describe how our models are adapted to measure responses along other dimensions in later sections.

This is not a problem before the policy shock, as we can observe households that choose LTV ratios both above and below the 106 threshold.

To address this issue, we classify households from the after period cross-section into affected and control groups based on household characteristics that we do observe in both periods (Abadie, 2005). Based on data before the policy shock, we first predict LTV ratios within household wealth percentile times postal code cells based on household annual income (both in level terms and squared).[11] We then use the out-of-sample fitted values from this model to predict a counterfactual LTV ratio ($\widehat{LTV}_{hlt}$) for each household buying a house after the policy shock. Finally, households from both periods are classified as affected if they have a predicted LTV strictly above 106 ($d(\widehat{LTV}_{hlt} > 106) = 1$), and they are part of the control group otherwise ($d(\widehat{LTV}_{hlt} > 106) = 0$).[12] Based on this classification of households, we estimate:

$$\begin{aligned} y_{hlt} &= \alpha_l + \alpha_1 \; After_t + \alpha_2 \; d(\widehat{LTV}_{hlt} > 106) + \beta \; After_t \times d(\widehat{LTV}_{hlt} > 106) \\ &\quad + \theta' \mathbf{X}_{ht} + \epsilon_{hlt}, \end{aligned} \tag{2}$$

where β now captures the incremental policy response of affected households, controlling for the trend in mortgage leverage choices among control households. The (weaker) identifying assumption is now that there is no specific trend in LTVs in the period immediately surrounding the policy shock that would have caused trends in LTV ratios to differ between the two groups if the lending limits had not been introduced.

[11] Precisely, we estimate the linear regression $LTV_{hlt} = \alpha_{w(h)} \times \alpha_l + \beta_1 Income_{ht} + \beta_2 Income_{ht}^2 + \epsilon_{hlt}$, where the first explanatory variable denotes household wealth times (two-digit) postal code fixed effects. We cannot include further interacted fixed effects in the model due to an empty cells problem. Including additional explanatory variables does not improve explanatory power.

[12] It is important to recognize that both households in a weak and financially strong position can end up with high predicted LTV ratios. For example, the former may be liquidity-constrained, whereas the latter may wish to exploit tax subsidies. We revisit the role of household financial strength in Section 3.2.

3 Effects of the LTV Limit on Households

Graphical evidence shows the striking effect of the August 2011 introduction of the 106 LTV limit on first-time homebuyers. Figure 2 shows data for the cross-section of mortgages. Panels (a) and (b) show the frequency and number of transactions, respectively. In the year prior to the regulation (i.e., from July 31, 2010 until July 31, 2011), first-time homebuyers' mortgages had LTV ratios clearly in excess of 106. In the following year, there is a bunching in the density of mortgages at 105 and 106, about 20% of issuance versus 2% in the year before. Figure 1 shows the time-series regime shift from ex post non-conforming loan issuance (i.e., LTV strictly above 106) into conforming loans (below 106). Conforming loans increased 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.

The remainder of our empirical analysis moves beyond this aggregate evidence. The goal is to better understand the transmission of macroprudential lending limits to household finances and decision-making. In Section 3.1, we show that the basic finding in the figures is statistically robust in a multivariate regression framework that accounts for heterogeneity across households and potential time effects. In Section 3.2, we analyze how household balance sheets adjust. We show that lower mortgage debt take-up under the new regime reduces both household leverage and liquidity. Finally, in Section 3.3, we document two important consequences of the regulation for households: lower mortgage default but also lower transition rate into homeownership, especially among liquidity-constrained households.

3.1 LTV Ratios for First-Time Homebuyers

3.1.1 Baseline results

Table II shows the adjustments in LTV ratios among first-time homebuyers after the implementation of the 106 limit. In panel A, we estimate Equation (1) putting the dummy variable for whether a household is above the threshold as dependent variable. Column [1] shows a –0.355 estimate of β without including any control variables. It can be seen that the likelihood of having an LTV above the 106 threshold decreased by 35.5 percentage points for the average household after the rule change. The point estimate is statistically significant at the 1% confidence level. The remaining columns of panel A progressively include location and household balance sheet control variables: postal code fixed effects, and $Income_t$, $Wealth_{t-1}$, and $LiquidAssets_{t-1}$ percentile dummies. The coefficient on $After_t$ remains essentially unchanged, which supports our assumption that households are not timing their house purchases to avoid the lending limit. The strictest specification in column [5] shows a 36.8 percentage point reduction, a magnitude that is in line with the time-series average shown in Figure 1.

Panel B instead uses LTV as the outcome variable. Columns [1] to [5] estimate Equation (1), again progressively including the location and household level control variables. The point estimate of β (approximately –0.030) is very stable across specifications and always significant at the 1% level. In terms of economic magnitudes, it corresponds to about a three percentage point drop in LTV, on average, across similar households between the before and after periods, and is in line with the unconditional statistics in Figure 2 and Table I.

We next examine how the policy shock impacts LTV ratios among households more likely to exceed the lending limit absent its imposition. Columns [6] to [7] classify households in the after period based on our LTV ratio prediction model. In column [6] we restrict the sample to 22,800 "affected" first-time homebuyers with predicted LTV ratios above 106. We re-estimate Equation (1) for this subsample and now find a larger coefficient estimate on $After_t$ of –0.046. This corresponds to a 4.6 percentage point decrease in the average LTV ratio. Conversely, column [7] shows an increase in average LTV ratio (of 1.7 percentage points) among the remaining 10,788 "control" households that we predict would choose a counterfactual LTV ratio below 106. This latter finding suggests that there may be time trends in the data that we should account for. Our DiD specification (2) does so and, as shown in column [8], yields an estimate of –0.064, again significant at the 1% confidence level. This last estimate corresponds to a 6.4 percentage point drop in the LTV ratios in affected households' mortgages in the wake of the policy shock.

3.1.2 Results by household financial strength

We have shown that the policy shock binds, particularly among households that are predicted to take on mortgage leverage in excess of the limit. However, we have yet to understand which groups of households are more exposed to the lending limits, which is essential if we are to shed light on the potential welfare consequences of the policy, especially for vulnerable households. Note that, in principle, liquidity constraints and tax subsidies (i.e., the mortgage interest deduction) may motivate both financially-constrained and savvy households to load up on mortgage debt. We therefore examine whether the reduction in LTV ratios is present or stronger among households that are in a weaker financial position.

Graphical evidence in Figure 3 shows average household income, liquid assets, and wealth across LTV ratios and over time. Notice that precisely at the threshold there appears to a sharp drop off in average income, liquidity, and wealth when comparing the after period (solid line) to the before (dashed line) period. This suggests that the policy shock induces financially constrained households to bunch just below the lending limit.

Table III tests whether households' financial positions are relevant using our regression framework. We stratify households according to income (columns [1] and [2]), liquidity ([3] and [4]), and wealth ([5] and [6]), taking the flow of income during the year of home purchase and the stock of liquid assets and wealth from year before the purchase. For each variable, we split at the median value and create "Low" and "High" subgroups (below and above median, respectively) on which we estimate our DiD model (2).

Two main results emerge from the table. First, the sign and statistical significance of the point estimates indicate that both financially strong and weak groups respond to the lending limit by reducing leverage. Second, there are meaningful differences in the magnitudes of the policy response between the groups. In particular, the size of the coefficient of interest, β, is estimated to be at least 20% larger for the constrained group in all three cases.[13]

Thus, we find evidence that introduction of the LTV limit is particularly effective at reducing leverage among the set of financially constrained households. Whether this has important implications for household liquidity and solvency is our next question.

3.2 Household Debt and Liquidity Adjustments After Purchase

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 expand our analysis to consider household debt, more broadly defined, as well as liquidity.

We continue estimate our DiD model shown in Equation (2) on our repeated cross-section data, but now using variables intended to capture the important facets of households' balance sheets. These variables are measured in level terms in the year immediately following the house purchase. For example, we consider $\textit{Liquid Assets}_t$—the level of household liquidity held in bank deposit and savings accounts—in the first tax filing following the home purchase. In this case, β measures the incremental effect of the policy for the liquid asset holdings of affected households, while controlling for the trend in post-purchase liquidity among households whose LTV ratio choices are unaffected by the policy shock. In this case, our expectation is that β is negative because affected households are now required to make larger down payments on their homes or contribute more towards the transaction costs. However, we do not have a sense of how sizable the effect will be, especially relative to initial

[13]These differences are significant at least at the 10 percent level for each measure of household financial constraints. We reach this conclusion in a triple-differences specification (unreported) that stacks together both the constrained and unconstrained groups.

liquidity position of the household.

Table IV shows how household debt and liquidity responds to the policy shock. In panel A, we examine housing choices and mortgage credit. Column [1] repeats the baseline estimation of the LTV policy response, for ease of comparison. Column [2] puts the (log) mortgage amount, i.e., the numerator in the LTV ratio, as the dependent variable. The point estimate of –0.042 indicates that affected first-time homebuyers reduce borrowing by 4.2 percentage points relative to the control group. This estimate is significant at the 1% confidence level. Interestingly, affected households buy houses that are about 1.8 percentage points more expensive on average (column [3]), thus increasing the denominator in the LTV ratio as well. Evidently, the first-time homebuyers do not appear to compromise on housing market choices when faced with binding lending limits. Taking the average home value and mortgage amount in the period before the lending limit (about €206,100 and €222,200, respectively), the DiD estimates indicate that the average affected household borrows €9,332 less to purchase a house that costs an additional €3,710, a funding gap of about €13,000.

Panel B moves beyond the LTV ratio to other aspects of household debt and liquidity. We first examine the mortgage payments for households buying homes under the new regime. The DiD estimates in columns [1] and [2] show that the average annual mortgage payment and interest expense fall by €2,355 and €210.80, respectively. These estimates are both significant at at least the 10% level. Column [3] estimates that the ratio of annual mortgage payment to household income drops by 3.2 percentage points (statistically significant at the 1% level). These findings follow naturally from the lower mortgage borrowing by affected households, and illustrate how the policy is effective in reducing households' mortgage debt servicing costs.

We next analyze changes in household leverage by examining the ratios of mortgage debt and total debt to income. The latter includes student debt and "other" debt (including both credit cards and personal loans), in addition to the newly obtained mortgage. This allows us to assess whether households substitute to other costlier forms of credit in order to finance the housing transaction, which may be an undesirable consequence of the policy. The point estimates in columns [4] and [5] reject any such substitution effect: we estimate approximately a ten percentage point reduction in both mortgage debt- and household debt-to-income (both statistically significant at the 1% level).

We also examine household liquid assets in the year of the house purchase. Having shown that debt from other sources does not increase, it seems highly likely that households consume liquidity in order to finance the home purchase and transaction costs. The estimate in column [6] confirms this intuition: by year-end, household liquidity drops by about €1,668. In terms of economic magnitudes, this about 9.6% of mean liquid assets (€17,410) prior to the home purchase. However, given the skewness of the liquid assets distribution, this economic effect doubles to 23.5% when evaluated at the median (€7,094). Finally, note that the reduction in year-end liquidity (€1,668) is less than the €9,332 reduction in borrowing (or the €13,000 funding gap). Some of this effect is driven by the €2,355 lower annual mortgage payment, but it raises the possibility that households might also reduce consumption, including home improvements and home-related durables (Benmelech et al., 2017).

Taken together, these results indicate 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 as a result of the macroprudential policy. On the other hand, households appear to consume liquidity in the short-term to meet higher upfront costs of buying a home. Drawing down on liquid buffers may heighten the risk of default for some households in short run. Understanding net effect of this trade-off is our next line of investigation.

3.3 Consequences for Household Default and Homeownership

3.3.1 Mortgage repayment performance

We have shown so far that the macroprudential lending limit lowers LTV ratios and debt servicing costs, at the expense of temporarily reducing household liquidity. The overall effect on mortgage default therefore trades off the improved solvency of households against the heightened risk of liquidity default in the short run.

We examine the impact of the LTV limit on mortgage repayment performance by estimating Equation (2) on the sample of mortgage originations in the mortgage defaults data set. As described in Section 1.2, the data is at the loan-level and cannot be matched to the other administrative data sets provided by the CBS, so we must select our sample to approximate first-time homebuyers. In addition, we modify our DiD framework to, first, predict LTV ratios using at-origination family income only (in level terms and squared). Second, since the default data contains detailed loan contract characteristics, we also control for origination month and payment type fixed effects (amortizing versus interest only). The dependent variable in the regression is our measure of default, *Payment Arrears*, set equal to one if the mortgage is in payment arrears as of 18 months after the end of our sample.

Table V provides evidence that mortgages granted to affected households after the LTV limits 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 –0.023 and statistically significant at the 1% level. Given the average default rate among mortgages issued before the policy shock is 3.3%, a 2.3 percentage point reduction is a sizable effect. Columns [2] and [3] partition the set of mortgages by family income at the time of origination, splitting at the median. We find that the reduction in mortgage arrears is entirely concentrated among low income borrowers.

These results indicate that the reduction in household leverage and debt servicing costs translates into significant improvements in the repayment behavior of borrowers. This is particularly true among low income households for whom excessively high mortgage leverage and debt payments are more likely to be problematic.

3.3.2 Rate of transition into homeownership

We have thus far characterized the impacts of the lending limit on credit market outcomes and housing choices conditional on homeownership. In this section, we take a step back and ask how the lending limit impacts the extensive margin decision to purchase a house. Since the LTV limit reduces the amount of debt financing available for the home purchase, it is plausible that it will lead to a reduction in homeownership among liquidity-constrained households.

Testing this conjecture is relatively straightforward using the CBS tax-return data combined with the housing register. These data allow us to identify the universe of 1,376,729 renters entering either the year before or after the policy shock. These are simply households that do not report any mortgage debt nor have an owner-occupied home registered in any family member's name, as of the beginning of either period. We can then track which households transition into homeownership by observing property transactions in the Land Registry both in the before and after period. The indicator variable *Homeownership* encodes this transition at the household level. In the aggregate, *Homeownership* captures the rate at which households transition from renting to owning, as opposed to the level of homeownership in the Netherlands. About 4.4% of the 403,360 renters end up buying a house in the period before policy shock. Afterward, this rate drops to 3.1% suggesting that the policy might have curtailed homeownership.

Table VI shows the results of estimating Equation (2) on the sample of renting households with *Homeownership* as the outcome variable.[14] The point estimate in column [1] is negative

[14]Parameters of the LTV prediction model continue to be based on the (unconstrained) LTV ratios of

(–0.003) and statistically significant at the 1% confidence level. The magnitude indicates that affected households reduce their probability of purchasing a home by three basis points, which is measured relative to the control group of renters with counterfactual LTV ratios less than 106 if they were to purchase a home. Note that this reduction is about 6.8% of the average probability of transitioning into homeownership in the before period, which is a mild but meaningful effect.

In the remaining columns of the table, we partition households on the basis of ex ante financial strength. Two notable results emerge. First, the lending limits appear to reduce the homeownership rate both among financially strong and weak households. Second, the stock of financial assets is an important determinant of the home purchase decision. Notably, the DiD estimate doubles in magnitude from –0.003 to –0.006 for the subsample of households with low liquid asset holdings. For this group, the estimate corresponds to a sizable 13.8% reduction in the probability of transitioning into homeownership. When we split households according to total wealth—which includes less liquid financial assets, such as stocks and bonds, in addition to bank accounts—the wedge between the DiD estimates between the high and low households attenuates. Thus, household liquidity-constraints amplify the negative effects of the LTV limit on renters' transition into homeownership.

4 Conclusion

We provide evidence on how *households* respond to 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. We map out the effects of the policy shock using novel administrative population data covering the universe of Dutch households. These data allow us to track household balance sheets

first-time homebuyers in the period before the policy implementation.

and home ownership to characterize important credit and housing market outcomes. We focus on the subset of first-time homebuyers for whom the lending limits are binding.

The policy succeeded in inducing households to use less mortgage debt to finance new home purchases. We do not find any evidence that home buyers buy cheaper houses. We examine household balance sheets in the year of the purchase and find that households do not substitute to other sources of credit to make up for the funding gap: overall household leverage and debt servicing costs fall in lock step with lower leverage coming from mortgage debt. Furthermore, we demonstrate that post-purchase liquid private savings (e.g., bank savings) are lower. Thus, after the policy shock, households consume liquidity in order to finance the purchase of their first home.

We provide direct evidence on some of the trade-offs associated with this macroprudential policy. First, the reduction in mortgage debt, as well as overall household leverage and debt servicing costs, results in lower mortgage default. The overall improvement in household solvency therefore outweighs the heightened risk of liquidity default in the immediate short term.[15] Second, the rate at which renters transition into buying their first home declines, especially among households without liquid financial assets on-hand to finance the transaction. To the extent that homeownership yields positive externalities (for a review, see Glaeser and Shapiro, 2003), the macroprudential policy could have negative welfare effects. Thus, policymakers should therefore carefully consider the benefits for mortgage risk and the costs associated with lower homeownership when evaluating the efficacy of these policies.

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

[15]Note that the repayment performance improvements we uncover represent an average effect over time and across households. It would be interesting to extend this analysis to understand how lower household leverage affects default conditional upon a negative idiosyncratic shock (e.g., job loss) or aggregate shock (e.g., during a recession).

other classes of investors (e.g., speculators), and so this still remains an empirical question.

Second, consumption. While we do not examine consumption behavior, the magnitude of our estimated liquidity effect relative to the funding gap raise the possibility that households reduce consumption, such as home improvements or home-related durables. Given the extent that house purchases stimulate consumption and investment (Benmelech et al., 2017), this suggests a channel through which macroprudential lending limits could have a chilling effect on aggregate demand. Relatedly, it would be important to understand how lower household leverage and debt servicing costs allow households to better smooth consumption in response to negative income shocks (e.g., due to unemployment). This is especially important in countries like the Netherlands where mortgage debt is full recourse and mortgage payments have priority even in personal bankruptcy. On the other hand, constrained households that are rationed out of housing markets altogether may no longer be able to consumption smooth in a life-cycle sense.

Third, private wealth accumulation. In many countries, households' net worth is tied up in their homes and the Netherlands is no exception. It is unclear from our analysis how households that are excluded from the housing market by the lending limits—for several years or even longer—go on to accumulate wealth. These are longer run effects that are clearly beyond the scope of our study.

Finally, macroprudential regulation that targets mortgage markets might have spillovers to housing rental markets, both in terms of cost and availability. 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.

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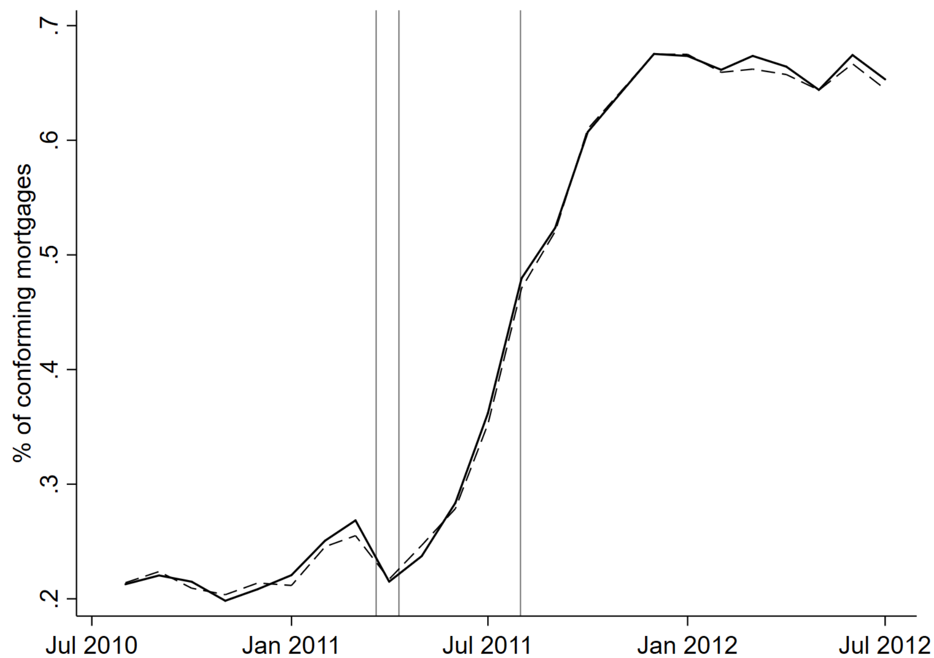
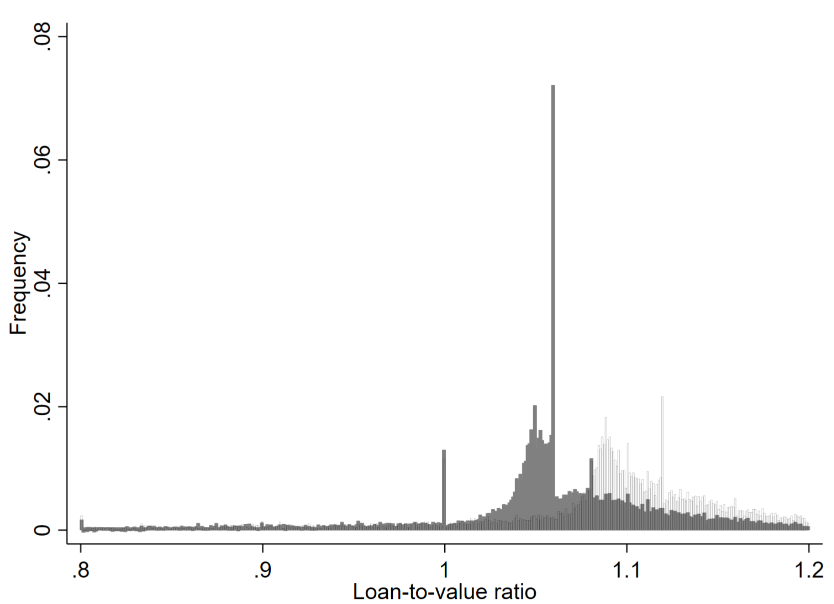


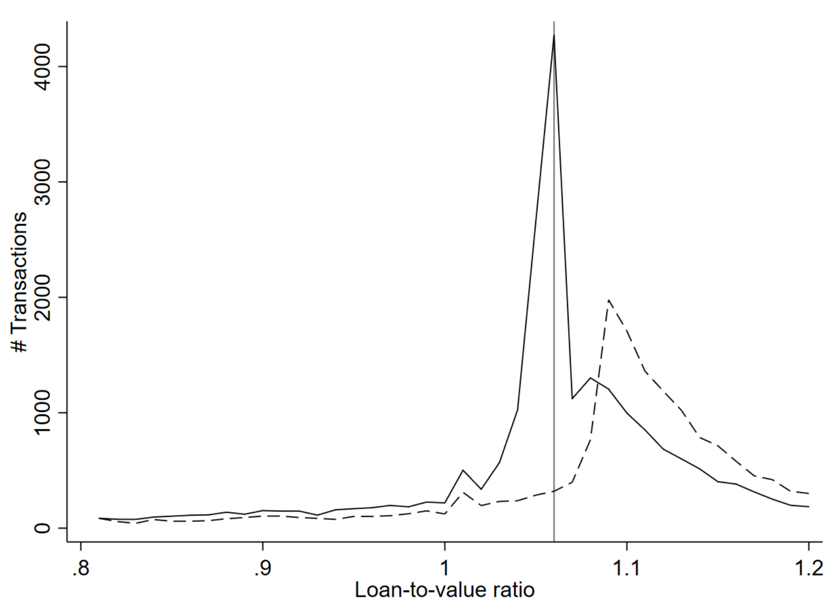
Figure 1

LTV limit and time-series of "conforming" mortgages

This figure presents the fraction of loans that conforms to the first LTV limit (i.e., LTV of 106 or less), value-weighted by mortgage size (solid line) or equally-weighted (dashed line). The vertical lines indicate when the rule was announced on March 21, 2011, confirmed and clarified on April 11, 2011, and introduced on August 1, 2011. Mortgage data comes from Statistics Netherlands (CBS) and transaction prices come from the Land Registry (Kadaster).



(a) Frequency of transactions by LTV

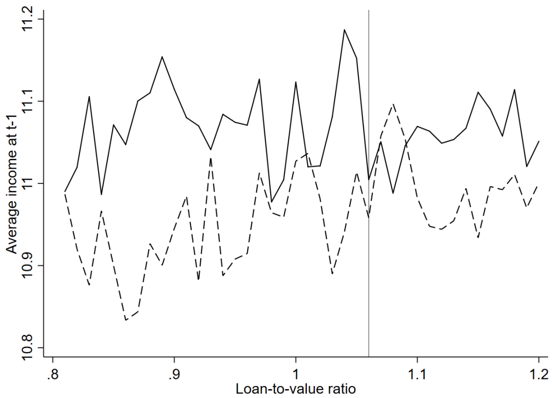


(b) Number of transactions by LTV

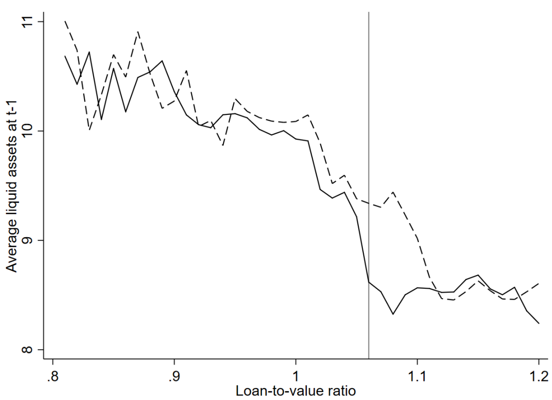
Figure 2

LTV limit and distribution of LTV ratios

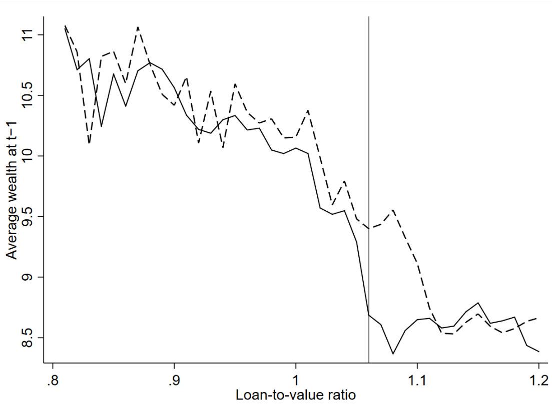
Panels (a) and (b) present frequency and the number of mortgage transactions for each loan-to-value (LTV) bucket both before and after the introduction of the LTV limit at 106. LTV is calculated as the household's mortgage amount as reported in the year after the property was purchased divided by the transaction price. Mortgage data comes from Statistics Netherlands (CBS) and transaction prices come from the Land Registry (Kadaster).



(a) Household income



(b) Household liquid assets



(c) Household wealth

Figure 3

Household balance sheet characteristics by LTV ratio

Panels (a), (b), and (c), respectively, show average (log) contemporaneous income, lagged liquid assets, and lagged wealth for first-time homebuyers granted mortgages with different LTVs both one year before the rule change (dashed line) and one year after (solid line). The vertical line indicates the LTV limit of 106, which was introduced on August 1, 2011.

Table I
Summary statistics

This table provides sample summary statistics for the household balance sheets and housing transactions data. The sample includes data from August 2010 until July 2012. The sample is split into the period one year before the rule change and one year after. The new LTV limit of 106, was introduced on August 1, 2011. The sample is restricted to LTV ratios between 80 and 120. Panel A summarizes the data for the sample of first-time homebuyers. The unit of observation is a household. Where indicated, t $(t-1)$ denotes measurement based on the first tax filing after (before) the home purchase. Panel B summarizes the data on mortgage payment default among homebuyers. In this panel, the unit of observation is a mortgage. *Payment Arrears* is measured as of the end of 2013 (denoted time $t = T$) and *Income* is measured at the time of mortgage origination. Panel C shows data for the population of households that rent homes at the beginning of each period. All variables are defined in Appendix A.

	Before LTV limit introduced						After LTV limit introduced					
	N	Mean	Std.	p25	Med.	p75	N	Mean	Std.	p25	Med.	p75
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Panel A: Summary statistics for first-time homebuyers												
LTV_t	15,367	1.083	0.076	1.071	1.096	1.127	21,192	1.054	0.069	1.040	1.059	1.091
$Mortgage\ Amount_t$	15,367	222,200	91,650	166,000	205,000	255,000	21,192	212,100	85,150	160,000	197,200	243,800
$Home\ Value_t$	15,367	206,100	87,640	155,000	190,000	235,000	21,192	202,200	83,990	152,000	187,000	230,500
$Income_t$	15,367	75,590	40,900	51,790	68,440	90,410	21,192	76,460	37,790	52,710	69,660	91,040
$Liquid\ Assets_{t-1}$	15,367	18,915	37,849	2,562	7,751	21,960	21,192	17,410	33,070	2,253	7,094	20,550
$Wealth_{t-1}$	15,367	28,270	62,170	2,795	8,732	24,730	21,192	26,130	68,990	2,428	7,953	24,120
$Mortgage\ Payment_t$	14,354	12,580	40,590	7,129	9,537	12,646	19,954	12,730	37,470	7,123	9,316	12,160
$Interest\ Expense_t$	14,354	10,160	4,843	7,316	9,365	11,920	19,954	9,649	4,368	7,084	9,031	11,390
$Payment/Income_t$	14,354	0.183	0.664	0.114	0.143	0.170	19,954	0.182	0.592	0.111	0.137	0.164
$Mortgage\ Debt/Income_t$	15,173	3.096	0.780	2.584	3.052	3.525	20,966	2.919	0.741	2.450	2.871	3.311
$Total\ Debt/Income_t$	15,367	3.334	8.675	2.664	3.140	3.640	21,192	3.102	2.571	2.529	2.959	3.411
Panel B: Summary statistics for mortgage defaults among homebuyers												
$Payment\ Arrears_T$	35,771	0.033	0.178	0	0	0	41,980	0.029	0.169	0	0	0
$Income_0$	35,771	47,470	26,720	32,400	40,880	54,430	41,980	46,450	30,300	32,090	39,870	52,600
Panel C: Summary statistics for population of beginning-of-period renters												
$Homeowner_t$	403,360	0.044	0.206	0	0	0	973,369	0.031	0.172	0	0	0
$Income_t$	403,360	59,500	39,910	41,340	51,160	68,040	973,369	80,650	53,540	49,550	69,750	98,010
$Liquid\ Assets_{t-1}$	403,360	18,840	62,340	2,071	6,512	19,970	973,369	25,310	59,400	2,111	8,227	25,870
$Wealth_{t-1}$	403,360	38,710	104,220	2,270	7,939	26,280	973,369	141,330	199,480	4,605	116,270	224,270

Table II
Effect of LTV limit on LTVs among first-time homebuyers

This table shows the shift in LTV among first-time homebuyers around the implementation of the 106 LTV limit. The unit of observation in each regression is a household. The sample includes home purchase data from August 2010 until July 2012. The sample is restricted to LTV ratios between 80 and 120. In panel A, the dependent variable *d(LTV >106)* is an indicator that equals one if the LTV ratio is above 106. The LTV is calculated as the ratio of the mortgage amount to the home value (transaction price). The model is estimated using ordinary least squares. In panel B the dependent variable LTV is the continuously-measured LTV. *After* is an indicator equal to one from August 2011 until the end of the sample, and zero otherwise. Predicted LTV ($\widehat{LTV}$) is based on a linear regression model estimated on mortgages originated from August 2010 until July 2011 (i.e., the period before the LTV limit), and fitted out-of-sample on mortgages originated from August 2011 until July 2012. LTV is predicted using income and income squared and wealth percentile fixed effects interacted with two-digit postcode fixed effects. Where appropriate, main effects are included in the regressions but suppressed from the tables for ease of interpretation. All variables are defined in Appendix A. Standard errors are clustered by month. *, **, and *** indicate significance at the 10% level, 5% level, and 1% level, respectively.

Panel A: LTV above 106 threshold					
Dependent variable: *d(LTV >106)*					
	[1]	[2]	[3]	[4]	[5]
After	–0.355***	–0.358***	–0.358***	–0.366***	–0.368***
	(0.023)	(0.023)	(0.023)	(0.024)	(0.024)
Postcode fixed effects	N	Y	Y	Y	Y
Income percentile fixed effects	N	N	Y	Y	Y
Wealth percentile fixed effects	N	N	N	Y	Y
Liquid Assets percentile fixed effects	N	N	N	N	Y
N	36,559	36,104	35,596	34,673	34,223
R^2	0.13	0.20	0.21	0.26	0.28

Panel B: LTV measured continuously								
Dependent variable: LTV								
Sample:	All	All	All	All	All	$\widehat{LTV} > 106$	$\widehat{LTV} \leq 106$	All
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
After	–0.029***	–0.030***	–0.030***	–0.031***	–0.032***	–0.046***	0.017**	
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	
$After \times d(\widehat{LTV} > 106)$								–0.064***
								(0.002)
Postcode fixed effects	N	Y	Y	Y	Y	Y	Y	Y
Income percentile fixed effects	N	N	Y	Y	Y	Y	Y	Y
Wealth percentile fixed effects	N	N	N	Y	Y	Y	Y	Y
Liquid Assets percentile fixed effects	N	N	N	N	Y	Y	Y	Y
N	36,559	36,104	35,596	34,673	34,223	22,800	10,788	34,223
R^2	0.04	0.13	0.14	0.26	0.29	0.31	0.33	0.34

Table III
Effect of LTV limit on LTV by household financial strength

This table shows the shift in LTV among first-time homebuyers around the implementation of the 106 LTV limit by household financial strength. For contemporaneous (log) income, lagged liquid assets, and lagged wealth, we split the sample for above-median ("High") and below-median ("Low") subgroups. The unit of observation in each regression is a household. The sample includes home purchase data from August 2010 until July 2012. The sample is restricted to LTV ratios between 80 and 120. The dependent variable LTV is calculated as the ratio of the mortgage amount to the home value (transaction price). *After* is an indicator equal to one from August 2011 until the end of the sample, and zero otherwise. Predicted LTV ($\widehat{LTV}$) is based on a linear regression model estimated on mortgages originated from August 2010 until July 2011 (i.e., the period before the LTV limit), and fitted out-of-sample on mortgages originated from August 2011 until July 2012. LTV is predicted using income and income squared and wealth percentile fixed effects interacted with two-digit postcode fixed effects. Borrower control variables include income, wealth, and liquid assets percentile fixed effects. Main effects are included in the regressions but suppressed from the tables for ease of interpretation. All variables are defined in Appendix A. Standard errors are clustered by month. *, **, and *** indicate significance at the 10% level, 5% level, and 1% level, respectively.

Dependent variable: LTV						
Household finance variable:	$Income_t$		$Liquid\ Assets_{t-1}$		$Wealth_{t-1}$	
Sample:	Low	High	Low	High	Low	High
	[1]	[2]	[3]	[4]	[5]	[6]
$After \times d(\widehat{LTV} > 106)$	–0.074***	–0.052***	–0.077***	–0.062***	–0.082***	–0.061***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)
Postcode fixed effects	Y	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y	Y
N	16,227	17,038	16,390	16,892	12,915	20,430
R^2	0.41	0.36	0.29	0.37	0.34	0.36

Table IV
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 106 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. Panel A considers the components of LTV and panel B examines various measures of household debt and liquidity. *After* is an indicator equal to one from August 2011 until the end of the sample, and zero otherwise. Predicted LTV ($\widehat{LTV}$) is based on a linear regression model estimated on mortgages originated from August 2010 until July 2011 (i.e., the period before the LTV limit), and fitted out-of-sample on mortgages originated from August 2011 until July 2012. LTV is predicted using income and income squared and wealth percentile fixed effects interacted with two-digit postcode fixed effects. Borrower control variables include income, wealth, and liquid assets percentile fixed effects. Main effects are included in the regressions but suppressed from the tables for ease of interpretation. All variables are defined in Appendix A. Standard errors are clustered by month. *, **, and *** indicate 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]
After $\times$ $d(\widehat{LTV} > 106)$	–0.064***	–0.042***	0.018***
	(0.002)	(0.004)	(0.004)
Postcode fixed effects	Y	Y	Y
Borrower control variables	Y	Y	Y
N	34,223	34,022	33,950
R^2	0.34	0.69	0.70

Panel B: Household debt and liquidity

Dependent variable:	*Mortgage Payment*	*Interest Expense*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*Liquid Assets*
	[1]	[2]	[3]	[4]	[5]	[6]
After $\times$ $d(\widehat{LTV} > 106)$	–2,354.52**	–210.75*	–0.032**	–0.104***	–0.109***	–1,668.26***
	(1,002.11)	(105.56)	(0.014)	(0.014)	(0.019)	(460.51)
Postcode fixed effects	Y	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y	Y
N	32,296	32,296	32,296	34,001	34,223	34,223
R^2	0.10	0.51	0.09	0.54	0.50	0.59

Table V
Effect of LTV limit on mortgage default

This table shows the effects of the LTV limit on mortgage repayment prospects around the implementation of the 106 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. The sample is restricted to LTV ratios between 80 and 120. *Payment Arrears* is an indicator variable equal to one if a loan enters payment arrears and zero otherwise. *After* is an indicator equal to one from August 2011 until the end of the sample, and zero otherwise. Predicted LTV ($\widehat{LTV}$) is based on a linear regression model estimated on mortgages originated from August 2010 until July 2011 (i.e., the period before the LTV limit), and fitted out-of-sample on mortgages originated from August 2011 until July 2012. LTV is predicted using income and income squared. We split the sample for above-median ("High") and below-median ("Low") subgroups of (log) income at the time of mortgage origination. Borrower control variables include income percentile fixed effects. Loan control variables include origination month and payment type fixed effects. Main effects are included in the regressions but suppressed from the tables for ease of interpretation. All variables are defined in Appendix A. Standard errors are clustered at the origination month level. *, **, and *** indicate significance at the 10% level, 5% level, and 1% level, respectively.

Dependent variable: *Payment Arrears*			
Household finance variable:		$Income_t$	
Sample:	All	Low	High
	[1]	[2]	[3]
After × $d(\widehat{LTV} > 106)$	–0.023***	–0.026**	–0.014
	(0.007)	(0.010)	(0.009)
Postcode fixed effects	Y	Y	Y
Loan control variables	Y	Y	Y
Borrower control variables	Y	Y	Y
N	77,751	38,493	39,258
R^2	0.01	0.02	0.01

Table VI
Effect of LTV limit on homeownership

This table shows the shift in the homeownership rate among the population around the implementation of the 106 LTV limit. For contemporaneous (log) income, lagged liquid assets, and lagged wealth, we split the sample for above-median ("High") and below-median ("Low") subgroups. The unit of observation in each regression is a household. The sample includes home purchase data from August 2010 until July 2012. The sample is restricted to LTV ratios between 80 and 120. *Homeowner* is an indicator variable equal to one if the household transitions from renter to first-time homebuyer in the current period and zero otherwise (i.e., if they remain a renter). *After* is an indicator equal to one from August 2011 until the end of the sample, and zero otherwise. Predicted LTV ($\widehat{LTV}$) is based on a linear regression model estimated on mortgages originated from August 2010 until July 2011 (i.e., the period before the LTV limit), and fitted out-of-sample on mortgages originated from August 2011 until July 2012. LTV is predicted using income and income squared and wealth percentile fixed effects interacted with two-digit postcode fixed effects. Borrower control variables include income, wealth, and liquid assets percentile fixed effects. Main effects are included in the regressions but suppressed from the tables for ease of interpretation. All variables are defined in Appendix A. Standard errors are clustered by month. *, **, and *** indicate significance at the 10% level, 5% level, and 1% level, respectively.

Dependent variable: *Homeowner*							
Household finance variable:		$Income_t$		$Liquid\ Assets_{t-1}$		$Wealth_{t-1}$	
Sample:	All	Low	High	Low	High	Low	High
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
$After \times d(\widehat{LTV} > 106)$	–0.003***	–0.002*	–0.003***	–0.006***	–0.003***	–0.007***	–0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Postcode fixed effects	Y	Y	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y	Y	Y
N	1,376,729	529,995	827,305	610,181	752,209	610,297	760,912
R^2	0.03	0.02	0.04	0.03	0.04	0.04	0.03

Appendix A: Variable definitions

This appendix presents the definitions for the variables used throughout the paper. In the source column, "CBS," "ED," "K," and "SC" stand for Statistics Netherlands (CBS), European Datawarehouse, Land Registry (Kadaster), and the Software Company, respectively.

Variable	Definition	Source
LTV	Mortgage amount divided by home value	K, CBS
Mortgage Amount	Mortgage debt on home property (VEHW1210SHYH)	CBS
Home Value	Transaction price of house (KOOPSOM)	K
Mortgage Payment	Reduction in mortgage amount plus interest expense	CBS
Interest Expense	Interest paid on mortgage amount (INPT3170RBW)	CBS
Income	Pre-tax household labor income (INPPERSBRUT)	CBS
Liquid Assets	Deposits and bank savings (VEHW1111BANH)	CBS
Wealth	Total assets (VEHW1100BEZH)	CBS
Payment/Income	Mortgage payment divided by household income	CBS
Mortgage Debt/Income	Mortgage amount divided by household income	CBS
Total Debt/Income	Total debt (VEHW1200STOH) divided by household income	CBS
Payment Arrears	Indicator variable equal to one if mortgage has payment arrears	ED, SC
Homeowner	Indicator equal to one if household has a property registered in its name	K, CBS