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Abstract

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

Keywords: macroprudential policy, Residential Mortgages, Solvency vs. Liquidity Tradeoff, household leverage, Loan-to-value Ratio

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Take It to the Limit? The Effects of Household Leverage Caps*

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December 11, 2019

Abstract

We analyze the effects of borrower-based macroprudential policy at the *household-level*. For identification, we exploit administrative Dutch tax-return and property ownership data linked to the universe of housing transactions, and the introduction of a mortgage loan-to-value limit. The regulation reduces mortgage leverage, with bunching in its limit. Ex-ante more-affected households substantially reduce overall leverage and debt servicing costs but consume greater liquidity to satisfy the regulation. Improvements in household solvency result in less financial distress and, given negative idiosyncratic shocks, better liquidity management. However, fewer households transition from renting into ownership. All of these effects are stronger for liquidity-constrained households.

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Household leverage booms have been identified as a key cause of financial crises and deep recessions, not only in the U.S. and Europe in 2008, but also around the world and in many other time periods (Mian et al., 2017; Reinhart and Rogoff, 2009). Underlying this pattern are households that take on excessive mortgage debt 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 during the credit boom, academics and policymakers have advocated for the use of macroprudential tools, especially in highly-levered housing markets.[1]

Maximum limits on loan-to-value (LTV) ratios on residential mortgages have proven to be a very popular policy response (at least 60% of advanced countries have used them), since, in principle, they may directly reduce mortgage borrowing thereby restraining household leverage (Fuster and Zafar, 2015, 2016; Gete and Reher, 2016).[2] Consequently, lower leverage may improve households' ability to service debt, potentially resulting in fewer defaults and less sensitivity to adverse shocks (Corbae and Quintin, 2015; 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, and default dynamics (Allen and Carletti, 2013). Furthermore, there has not been any evidence on the implications of macroprudential 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.[3] We build a unique data set that matches administrative 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 understand the efficacy and mechanisms of the LTV limit by observing transitions from renting into homeownership as well as disaggregated data on income, assets, and liabilities at the household level. Our analysis focuses on first-time homebuyers for whom measurement

[1]For example, see Bianchi and Mendoza (2010), Brunnermeier et al. (2009), Claessens (2015), Farhi and Werning (2016), Freixas et al. (2015), and Jeanne and Korinek (2013).

[2]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`).

[3]The Netherlands has one of the highest household debt-to-GDP ratios in the world. Similar to the U.S., it also experienced a collapse in house prices and slowdown of household debt in the aftermath of the 2008 crisis. Section 1.1 provides further detail on why the Dutch context is highly-relevant.

of our variables of interest is 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).[4]

We analyze home purchases and parallel adjustments in household balance sheets—mortgage debt take-up, debt servicing costs, liquidity, and overall leverage—both before and after the introduction of the LTV limit. Our main empirical challenge is that changing macroeconomic and financial conditions (e.g., rapid house price appreciation) could cause both the macroprudential policy and the observed adjustments in household finances. While the dramatic shift in mortgage issuance satisfying the regulation (i.e., LTV below the limit) immediately following the policy announcement suggests this is unlikely (see Figure 1), we implement a difference-in-differences (DiD) model that controls for potential time effects. Since the same household cannot be a first-time buyer more than once, we develop comparison groups of first-time homebuyers before and after the introduction of the limit by predicting household LTV choices based on observable characteristics (Abadie, 2005).

Our main findings are as follows. We first confirm that the macroprudential lending limit bites. Graphical evidence (without any controls) and regression analysis (saturating with controls) shows that households respond by reducing LTV ratios to comply with the new regulation, consistent with lower mortgage borrowing among first-time homebuyers. We document a sharp 36 percentage point increase in loan issuance satisfying the new regulation immediately after it is introduced (Figure 1). Moreover, we find 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. Within our DiD framework, we estimate that, on average, at-origination LTV ratios among ex-ante more-affected households drop by 6.4 percentage points after policy implementation.[5] We find that this decline in LTV ratios is pronounced among liquidity-constrained households (lower bank savings or financial assets) suggesting distributional consequences of the new policy.

We then provide new evidence on the transmission of macroprudential policy to 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. These data allow us to examine how mortgage debt, the costs of servicing mortgage debt,

[4]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`).

[5]We do not find any evidence of preexisting differential trends between affected and control households' LTV choices or balance sheet outcomes, consistent with the parallel trends assumption of our DiD design.

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. Since we find that households do not replace lower mortgage debt with more affordable houses or lightly-regulated credit (e.g., personal loans or credit card debt) to finance their home purchase, overall household leverage—and hence debt servicing costs—fall in lock step with the lower mortgage leverage. As a result of the lower funding availability, households carry substantially lower liquid assets—private bank deposits and savings—after the purchase. Thus, while the LTV limit improves the solvency position of households, they must consume liquidity in the short-term to meet the now-higher upfront costs of buying a home.

In the second half of the paper, we investigate two economic consequences of LTV limit for households. First, we examine household financial distress. Despite the improvements in solvency, by consuming additional liquidity to accommodate the borrowing limit households may face heightened risks of financial distress should an adverse event occur in the short run.[6] To investigate this household liquidity-solvency 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.[7] We find that improvements in household solvency translate into significantly lower mortgage arrears, at least in the short-term (18 month horizon). In addition, we examine whether borrowing subject to the LTV limit makes households more "resilient," in the sense that they are better able to handle adverse shocks. We find that, after the LTV limit comes into effect, now-lower-leverage households experiencing negative income shocks are far less likely to run down bank balances to meet their ongoing mortgage payments.[8] Moreover, improvements in financial health occur only among low-liquidity households for whom excessively high leverage and interest payments are more likely to be troublesome.

Second, as motivated by survey evidence (Fuster and Zafar, 2015, 2016) and theoretical work (Gete and Reher, 2016), we estimate how the lending limit impacts the extensive margin

[6] Bhutta et al. (2017) argue that most mortgage defaults are due to liquidity shocks at moderately negative equity (see also, Elul et al., 2010).

[7] Dutch mortgage debt is full recourse and therefore unlikely to go into foreclosure (0.03% in 2010). 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; Mian and Sufi, 2014). 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).

[8] Aside from a lower likelihood of default, maintaining a healthy liquidity position after a negative income shock may benefit households by allowing them to sustain consumption (e.g., Jensen and Johannesen, 2017).

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 (at the one- and two-year-ahead horizons). We find that the introduction of the LTV limit sharply reduces this transition rate at the one-year horizon (21.6% of the pre-policy mean), slowing to a meaningful reduction at the two-year horizon (8.9%). However, these effects are larger in magnitude and precisely estimated only among liquidity-constrained households. Thus, facing lower availability of credit, liquidity-constrained households are deterred from transitioning into homeownership, at least in the short-run.

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). Despite the large interest by policymakers and the growing theoretical macro-finance literature on such policies, there has been scarce micro-evidence possibly due to lack of data availability. Our novel contribution is to comprehensively examine the *household-level* response to borrower-based macroprudential limits using administrative housing and tax records for the Dutch population.

The majority of prior studies conduct cross-country analyses using aggregates (among others, see Akinci and Olmstead-Rumsey, 2018; Alam et al., 2019; 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., Auer and Ongena, 2016; Jiménez et al., 2017) and households (e.g., Basten and Koch, 2015). 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, first, the fact that the policy we study is immune to "leakages" across lenders by design (in contrast to, say, Aiyar et al., 2014) and, second, our finding that households do not substitute to lightly-regulated credit (e.g., expensive unsecured consumer credit) to plug funding shortfalls due to the limit.

Three recent studies examine the supply-side (e.g., bank-level) 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—consistent with unintended consequences of the policy and regulatory arbitrage. 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. Moreover, DeFusco et al. (Forthcoming) 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 U.S. residential mortgages (see also, Bhutta and Ringo, 2015).[9,10] Thanks to our household-level data, we instead demonstrate that regulating mortgage leverage is 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 have positive effects for loan repayment and improve the resilience of household balance sheets to negative income shocks. Borrower-based lending limits may therefore have the potential to mitigate the severe negative effects of household leverage for defaults and consumption during bad times (e.g., Mian et al., 2017). However, on the negative side, the policy reduces the transition among renters into homeownership, especially among liquidity-constrained households. To the extent that there are positive externalities associated with homeownership—such as life-cycle consumption smoothing or tax advantages (e.g., Glaeser and Shapiro, 2003)—regulating mortgage borrowing may therefore entail important welfare costs to households.

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 Sections 3 and 4 present and discuss the results. Section 5 concludes with some open questions and avenues for future research.

[9] Passed by Congress in 2010, implemented in 2014 by the Consumer Financial Protection Bureau, and set to expire in 2021, this law has temporarily created a class of lender-liability-exempt "qualifying mortgages" that meet certain underwriting criteria, including a ceiling on the mortgage payment-to-income ratio.

[10] Outside prudential policies, a handful of finance papers analyze household responses to shocks to debt servicing costs and borrowing capacity due to shocks 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.

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.[11] 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. Such costs include 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).[12] 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 mortgages with LTV ratios as high as 120 could be sustained as an equilibrium.[13] 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 worth, consumption, employment, and economic growth 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 to the economy coming from the housing market.

To limit the potentially harmful effects of boom-bust cycles in property lending and house prices, policymakers instituted a number of mortgage market reforms beginning in 2011. The

[11] 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).

[12] 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, `www.economist.com/finance-and-economics/2017/11/09/americas-republicans-take-aim-at-mortgage-subsidies`.

[13] High LTV lending—as much as 120% of the property value—to both creditworthy and subprime borrowers was not uncommon in the U.S. beginning in the early 1990s (e.g., Calomiris and Mason, 1999).

first notable change were macroprudential lending 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.[14] 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.[15] The maximum LTV ratio was initially set at 106. 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.[16] On the borrower side, there were 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. In essence, households with prior outstanding loans were grandfathered in under the new rule. By contrast, the 106 LTV ratio limit applied most cleanly to first-time homebuyers seeking a mortgage after August 1, 2011. We say "most cleanly" because some exemptions exist for first-time homebuyers (that we cannot reliably identify in the data). For example, as indicated in the Code of Conduct, households may violate the rule in order to finance certain "energy-saving facilities"—home improvements such as energy efficient windows. To minimize potential mismeasurement—as well as for the distributional considerations mentioned in the Introduction—first-time homebuyers are the focus of this study.

1.2 Data and summary statistics

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 in-

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

[15]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.

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

clude 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 transactions at the monthly frequency.

Homeownership is identified in the data based on tax filings and the housing register.[17] 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 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 1,278,960 meeting our data requirements outlined below. 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 introduction of the LTV limit 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 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, student loans, and other debt (including credit card debt and other personal loans). Having this broken down by

[17] All variables are defined in Appendix A.

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 and "leakages" (e.g., Aiyar et al., 2014). 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.[18] Second, mortgage amounts (and thus LTV ratios) may be mechanically lower due to payments occurring during the year of origination. We address both of these potential measurement issues in robustness tests described below.

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 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,

[18] Lenders are also legally permitted to use the appraisal value, which we do not observe.

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).

We analyze mortgage repayment performance using proprietary mortgage servicing data from a Dutch software company combined with publicly-available data from the European Datawarehouse (ED).[19] 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 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.[20] 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.

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 (i.e., reported in the data).

[19]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`.

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

Table I shows summary statistics. We cut the data based on time period before and after introduction of the LTV limit. Panel A shows information on first-time homebuyers home purchase transactions as well as their balance sheets. After the regulation is introduced, as we can see, the LTV, mortgage size, and property value unconditional distributions show a leftwards shift. 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 also improve as households take up less debt. Panels B and C are suggestive of the tradeoffs involved with the policy. Panel B shows that mortgage arrears 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 in the immediate aftermath (3.7% before versus 3.3% after at the one-year horizon).

2 Empirical Methodology

Our identification strategy exploits a policy introduction in conjunction with administrative household-level data. 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. In this section, we develop a DiD model that leverages this data structure to measure the effects of the LTV limit. We first measure the limit's effect on LTV choices, but, as will become apparent later in the paper, we adapt this framework to analyze other household-level outcomes. To do so we estimate the following regression using ordinary least squares (OLS):

$$y_{ht} = \beta \; After_t + \alpha_{l(h)} + \alpha_{y(h)} + \alpha_{w(h)} + \alpha_{s(h)} + \epsilon_{ht}, \tag{1}$$

where h indexes households, l indexes household location (two-digit postal codes), the y, w, and s index household income, wealth, and savings (percentiles), respectively, and t indexes time (months). In the first instance, the dependent variable, y_{ht}, will be either LTV_{ht} measured continuously or a dummy variable for whether it is greater than the 106 limit, $d(LTV_{ht} > 106)$. $After_t$ 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).[21] The postal code fixed effects ($\alpha_{l(h)}$) control for fixed differences

[21] Our conversations with practitioners suggests that *lenders* began to immediately conform with the finalized rule. In our main tests, we use the implementation date to define $After_t$ and further investigate timing and announcement issues in Section 3.3.

across regions, such as housing affordability. The income, wealth, and savings (percentile) fixed effects ($\alpha_{y(h)}$, $\alpha_{w(h)}$, and $\alpha_{s(h)}$) account for the financial position of household h in the year prior to purchasing the house. The error term, ϵ_{ht}, is clustered at the origination month level, since the key independent variable is a time-varying policy outcome (Petersen, 2009).[22]

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 there are no confounding macroeconomic, financial, or regulatory events (e.g., interest rate rises or changes in tax incentives) driving both the macroprudential policy response and the change in the use of debt conditional on home purchase.[23]

The dramatic shift in loan issuance satisfying the regulation (i.e., LTV ratio below 106) in a tight window around the policy announcement and implementation shown in Figure 1 suggests that the identifying assumption is reasonable. Nevertheless, to buttress our approach, we consider a DiD framework that controls for potential time effects. In addition, this allows us to measure the response of first-time homebuyers that are ex-ante more likely to be "affected" by the regulation. Following Abadie (2005), we classify households into affected and control groups based on: (i) the (unconstrained) LTV choices made by households in the before period; (ii) relevant household and property characteristics that we observe in both periods. We focus on household income and wealth since these variables are directly related to a borrower's capacity to repay and are theoretically well-motivated (e.g., Gete and Reher, 2016), as well as the property's location (postal code). Precisely, based on data before the policy shock we first predict LTV ratios via OLS as:

$$LTV_{ht} = \alpha_{w(h)} \times \alpha_{l(h)} + \beta_1 Income_{ht} + \beta_2 Income_{ht}^2 + \epsilon_{ht}, \tag{2}$$

where the first explanatory variable denotes household wealth times (two-digit) postal code fixed effects.[24] We then use the out-of-sample fitted values from this model to predict an LTV

[22]Our results are robust to double-clustering standard errors at the origination month, location level.

[23]In the run up to the policy, the macroeconomic environment in the Netherlands was relatively benign as compared with the struggling economies of southern Europe (as discussed in Van Bekkum et al., 2018). For example, there was stability in the average interest on new mortgages (4.52% in 2010 and 4.55% in 2011), the average purchase price of owner-occupied homes (€239,530 in 2010 and €240,059 in 2011), and GDP growth (3.9% in 2010 and 2.4% in 2011); see `opendata.cbs.nl/statline`. In addition, as mentioned in Section 1.1, there were no major regulatory initiatives impacting mortgage demand at the same time as the first introduction of the LTV limit.

[24]We cannot include further interacted fixed effects in the model due to an empty cells problem. Including

choice ($\widehat{LTV}_{ht}$) 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 ($d(\widehat{LTV}_{ht} > 106) = 1$), and they are part of the control group otherwise ($d(\widehat{LTV}_{ht} > 106) = 0$).[25] Then, based on this classification of households, we estimate:

$$\begin{aligned} y_{ht} \;=\; & \beta\ After_t \times d(\widehat{LTV}_{ht} > 106) + \alpha_1\ After_t + \alpha_2\ d(\widehat{LTV}_{ht} > 106) \\ & + \alpha_{l(h)} + \alpha_{y(h)} + \alpha_{w(h)} + \alpha_{s(h)} + \epsilon_{ht}, \end{aligned} \tag{3}$$

where β now captures the incremental policy response of ex-ante more 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 limit had not been introduced.[26]

Finally, we also implement a matching estimator as a complementary, nonparametric method to measure the effects of the policy shock (see, Abadie and Imbens, 2006). Rather than making use of an LTV prediction model to identify households that are constrained by the lending limit, we utilize the observed choices of households in the period after policy implementation. By revealed preference, we identify affected households as those that borrow just below the LTV limit after the policy is implemented. These households are then matched to the nearest first-time homebuyer from the period before implementation based on observable characteristics. We then take the simple difference between affected and control households to measure the (local treatment) effect of the policy on mortgage leverage choices and household balance sheets.

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 (3,852 households after below-mentioned restrictions are applied). By revealed preference, we assume that these households are constrained by the policy shock, i.e., this is the affected group. Candidate control households include all first-time homebuyers from the year before the policy change, i.e., households that do not face an LTV limit. Each affected household is matched with candidate controls in the same two-digit post code, and then its nearest neighbor based on $Income_t$, $Liquid\ Assets_{t-1}$, and $Wealth_{t-1}$. We match with replacement

further explanatory variables does not clearly improve (in-sample) model fit.

[25]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 ex ante liquidity in Section 3.2.

[26]In Section 3.3, we confirm that there are no preexisting trends among the key outcome variables.

and based on euclidean distance. We drop matches where the difference between any matching variable (Δ_{i-j}) is in the 1st and 99th percentile of the distribution, and we also discard post codes with fewer than five successful matches.

3 Effects of the LTV Limit on Households' Finances

3.1 Mortgage LTV ratios for first-time homebuyers

Graphical evidence shows the striking effect of the August 2011 introduction of the LTV limit. Figure 1 shows the time-series regime shift into loans satisfying the regulation (i.e., at-origination LTV below 106). The share of such 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.[27] Figure 2 shows the corresponding shift in the distribution of mortgages conditional on LTV. 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.[28,29]

Table II shows the adjustments in LTV ratios among first-time homebuyers after the implementation of the LTV limit based on our DiD framework. In panel A, we estimate Equation (1) with a 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 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 indicates that observable differences between households buying before versus after the policy are unlikely to drive our results. The most saturated 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 the continuously-measured LTV as the outcome variable. Columns

[27] The fraction of mortgages satisfying the regulation increases to 85% when we incorporate information on WOZ values into the LTV. The fraction is below 100% due to the exemptions described in Section 1.1.

[28] A similar distributional shift occur in the number of mortgage transactions by LTV.

[29] As far as we are aware, bunching at other LTV notches (e.g., at 100) does not reflect other regulation.

[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.[30] 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 prediction model (2). In column [6] we restrict the sample to 22,800 "affected" first-time homebuyers with predicted LTV ratios above the limit. 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 LTV ratio below the limit. This latter finding suggests that there may be time trends in the data that we should account for. Our DiD specification (3) does so and, as shown in column [8], yields an estimate of –0.065, again significant at the 1% confidence level. This last estimate corresponds to a 6.5 percentage point drop in the LTV ratios in affected households' mortgages in the wake of the policy shock.

3.1.1 Results by initial household liquidity

While tax subsidies may induce wealthy households to carry high levels of mortgage debt in excess of the limit, the new regulation may bind—and have potentially important real effects—for cash-poor households lacking alternative financing options. Figure 3 examines the role of ex-ante liquidity by plotting the average pre-purchase household liquid assets across realized LTV ratios in the year before (dashed line) and the year after (solid line) the introduction of the LTV limit. We can see two important patterns in the figure. First, household liquidity is monotonically decreasing in LTV, indicating that cash-poor households tend to finance a greater fraction of the housing transaction. Second, precisely at the threshold (vertical line) there appears to a sharp drop off in liquidity among households borrowing

[30] The stability of the point estimate in parallel with the increase in R^2 from 0.04 (column [1]) to 0.29 (column [5]) suggests a limited role for selection on unobseravbles within our empirical framework (Altonji et al., 2005). We further confirm this result using the bounding method proposed by Oster (2019) based on the fully-specified DiD model in column [8]. We find that the share of variation explained by unobservables would have to be 5.68 times as large as the share explained by observables to reduce the coefficient of interest to zero, which seems implausible.

in the *after* period. In contrast, no such drop off is present at the threshold in the *before* period. These facts suggest that household liquidity constraints may play an important role.

Table III measures the importance of household liquidity constraints using our regression framework. We stratify households by their stock of liquid assets (columns [1] and [2]) and total financial assets ([3] and [4]) in the year prior to the purchase. For each variable, we create "Low" and "High" subgroups (bottom and top tercile of the distribution, respectively) on which we separately re-estimate our DiD model (3). Two main results emerge from the table. First, the sign and statistical significance of the point estimates indicate that both relatively high and low liquidity 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 40% larger for the constrained group in all three cases.[31] Thus, we confirm that the LTV limit is particularly effective at reducing leverage among the set of ex-ante liquidity-constrained households.

3.2 Household balance sheet response

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 to estimate our DiD model shown in Equation (3) 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 at the end of the year of house purchase. For example, we consider $Liquid\ Assets_t$—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 less affected by the policy shock. On the one hand, β 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

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

were unaffected by the regulation (e.g., personal loans), then β could be non-negative.

Table IV shows how household debt and liquidity respond 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 slightly more expensive on average (about 1.8 percentage points, see column [3]), thus increasing 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 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 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 €2,355 (significant at the 5% level). Column [2] 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 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 (including 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 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.

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 [5] confirms this intuition: by year-end, household liquidity drops by about €1,668. In terms of economic magnitudes, this is about 9.6% of mean liquid assets (€17,410) prior to the home purchase.[32] 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 suggests that households also reduce consumption, including home improvements and home-related durables (Benmelech et al., 2017).

3.3 Robustness tests

Before studying the important implications of these household balance sheet adjustments, we conduct several robustness tests to buttress our main findings. First, we examine the sensitivity of our DiD estimates under an alternative timing that excludes households buying homes after the policy announcement (March 21, 2011) but before implementation (August 1, 2011). Column [1] of panel A of Table V repeats our baseline estimation excluding these observations. The point estimate of –0.061 is essentially the same as the prior estimates, indicating that our results are not driven by changes in behavior during the period between policy announcement and implementation.

Second, we address a potential issue with how we measure LTV ratios at the time of mortgage origination. While the denominator is based on the actual transaction price, the loan balance is recovered from the end-of-year tax filing. If the distribution of mortgage originations is unequal throughout the year between the pre- and post-periods, then this might introduce mechanical differences in LTV ratios (due to differences in accumulated payments over the year). The tight bunching of loans precisely at the threshold after the limit was imposed suggests that this is unlikely (see Figure 2). Moreover, column [2] of panel A of Table V shows that the estimate of β is unchanged after appending the baseline model with month-of-year fixed effects.

Third, we further examine whether coincident changes in economic conditions—that disproportionately influences mortgage demand among affected households—are responsible for the adjustments in LTV ratios. While we focus on observationally similar first-time homebuyers in a narrow event window of one year around a salient policy shock, demand effects could still plausibly be in play. We therefore append the baseline estimation with postal code-by-month fixed effects to control for changing economic conditions in a given location in a given month. Under this restrictive specification, we lose about one-third of our first-

[32]Column [6] indicates that this corresponds to a 13.0 percentage point drop in liquid assets.

time homebuyers who turn out of be singletons within their respective cells. Nevertheless, the magnitude of borrowers' response remains stable at –0.054 (see column [3]), indicating that the downwards adjustment in LTV ratios is not a response to some coincidental macroeconomic trends in different locations.

Fourth, we assess whether stress in banking sector could explain the contraction in mortgage credit. For example, binding capital constraints and attempts by banks to deleverage could also rationalize lower LTV ratios. To rule out this concern, we use an important institutional feature of the Dutch mortgage market—the fact that nearly all eligible mortgages receive a government guarantee. Guaranteed loans are default risk free and have no capital requirements, are more likely to be sold and securitized, and are thus supplied inelastically by banks and other financial intermediaries (Van Bekkum et al., 2018).[33]

While we do not have information on whether the household receives a guaranteed mortgage, we can can proxy for its presence based on the conforming loan size limit.[34] We therefore split our households according to whether they borrow above or below the conforming size limit over our event window (€350,000), and estimate our baseline empirical model on these two subsamples. Two notable facts emerge. First, nearly all first-time homebuyers in our sample satisfy the conforming loan limit. This follows naturally from our sample selection criteria, which are set to ensure that we are examining ordinary households. Second, as shown in column [4], the estimate of β once we exclude ineligible mortgages is in line with the baseline estimate. We therefore conclude that the reduction in mortgage credit take up reflects binding household borrowing constraints as a consequence of the borrower-based macroprudential policy, as opposed to lenders retrenching from the market.

Fifth, we verify the robustness of our results to our measurement of LTV ratios. In our baseline estimation, we use the actual property transaction price as the "V" in LTV. These data are taken from the housing register and have the advantage of having no missing values in our data. In practice, banks are also allowed to base their lending decisions on the *Wet Waardering Onroerende Zaken* (WOZ) value, which is an administrative measure of the property's value used for property taxation purposes. While the bunching of LTVs below the threshold indicates that our baseline measurement is a good approximation, some

[33] Government guarantees for Dutch mortgages (Nationale Hypotheek Garantie, NHG) are issued by the Homeownership Guarantee Fund. NHG guaranteed mortgages are fully-backed by the Dutch government, which covers the outstanding principal, accrued unpaid interest, and foreclosure costs in case of default. During our event window, under Basel II, banks applied a zero risk weight to mortgages covered by NHG guarantees for the purposes of calculating capital requirements.

[34] Approximately 90% of all mortgages qualifying by loan size (below €350,000) were backed by the NHG (Van Bekkum et al., 2018).

lenders may use this alternative method. We therefore use the WOZ value—where this data is available—as an complementary approach. Column [6] repeats our baseline estimation under this alternative measurement and finds essentially unchanged results. This suggests that using transaction prices does not introduce systematic error into our estimation framework.

Sixth, we examine the dynamic effects of the LTV limit. We partition the two-year window from August 2010 until July 2012 around the policy change into four semiannual periods: from August 2010 until January 2011 inclusive, February 2011 until July 2011, August 2011 until January 2012, and February 2012 until July 2012. We modify Equation (3) to include three indicator variables corresponding to the latter three time periods. The coefficients on each of these indicator variables therefore measure adjustments relative to the corresponding change in the period from August 2010 until January 2011 (i.e., the omitted group). As shown in Panel B of Table V, two important results emerge. First, consistent with Figure 1, all household balance sheet adjustments occur within six months of the introduction of the limit. Second, the coefficients are generally small in magnitude and statistically insignificant in the period before the policy change, confirming that there are no preexisting trends between affected and control households.[35]

Finally, as an alternative to using LTV prediction models to classify households into affected and control groups, we implement a nearest-neighbor matching estimator to measure the effects of the new regulation. Table VI shows the results. Panel A presents the summary statistics for the affected households—the set of households borrowing at an LTV of 105 or 106 in the year following the policy implementation—and differences with matched controls from the before period. The matching procedure brings down the number of affected households from 21,192 to 3,852, and these households tend to have slightly lower income and wealth as compared with the full sample. The summary statistics indicate that the matching achieves covariate balance among the two groups of households along the dimensions we match on, i.e., none of the Δ_{i-j} are statistically different from zero.

Panels B and C verify that the macroprudential lending limit continues to have important effects on affected household finances using our matched sample approach. Panel B indicates that LTV ratios fall by about 3.6 percentage points. This is smaller than the baseline effect (6.4 percentage points) and is driven by the cheaper home purchases made by the (less wealthy) affected households in this sample. Given the LTV limit, households borrow even less to finance the home purchase (8.6 percentage points), as compared with the baseline estimation (4.2 percentage points), and they buy cheaper homes (5.4 percentage points lower

[35] *Liquid Assets* exhibits a small drop (significant at the 10% level) six months prior to the policy change.

transaction price). Panel C shows the same resulting tradeoff between solvency and liquidity: there is a sharp improvement in household leverage but this is associated with less liquidity in the short run. Thus, our main findings hold in this nonparametric framework and are therefore unlikely to be an artifact of our LTV prediction model assumptions.

4 Economic Consequences of the LTV Limit

4.1 Household financial distress

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

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).[36] 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.[37]

[36]Poor mortgage repayment performance may spillover to other important household-level outcomes such as employment and earnings through negative information in credit registers (Bos et al., 2018).

[37]The LTV limit may indirectly have positive externalities for the government in the form of lower sovereign credit risk, since the majority of mortgages in our sample are government guaranteed.

To measure the effects of the regulation on arrears, we estimate Equation (3) on the sample of mortgage originations in the mortgage servicing data set. These data are at the loan-level and cannot be matched to the other administrative data, so we must select our sample to approximate first-time homebuyers. In addition, we must predict LTV ratios using at-origination family income only (in level terms and squared). Since the servicing 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 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 VII 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 –0.023 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] consider subgroups of mortgages based on family income at the time of origination (bottom and top terciles of the distribution). We find that the reduction in mortgage arrears is larger in magnitude (2.6 percentage points) and statistically significant only among low income borrowers.[38,39] 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.

4.1.2 Dynamics of household liquidity in years following home purchase

These findings indicate that the short-run depletion of liquidity required to finance the additional housing transaction costs does not impede the ability of households to repay their

[38]Recall that the servicing data does not contain information on household liquidity nor financial assets. We therefore instead use household income to proxy for household financial constraints.

[39]One potential concern with this approach is that we have a shorter performance window post-policy and this could mechanically generate better performance for these (lower LTV) loans. Unfortunately, the data provider only gives a single snapshot of loan arrears in January 2013 and so we cannot simply choose a fixed performance window for all loans. Nevertheless, we can take two steps to alleviate this concern. First, we include origination month-of-year fixed effects in our DiD model. We are therefore, for example, comparing the performance differential between affected and unaffected loans originated in January 2011 with the differential between comparable (lower LTV) loans originated in January 2012. Second, we can approximate an equal performance window for loans by shortening the pre-period (post-period) window to include only loans originated three-months before (after) the policy shock. Under this equal performance window of about two years, we uncover a similar result: a 1.4 percentage point reduction in mortgage arrears (significant at the 10% level) only among low income borrowers.

mortgage debt. One potential explanation is that households affected by the LTV limit may quickly rebuild their liquidity buffers in the years following the home purchase.

Table VIII provides evidence consistent with this dynamic. Rather than focusing narrowly on differences in households' liquid assets in the year of the home purchase, we now also consider liquid assets one year ($t + 1$) and two years ($t + 2$) hence. The table shows the impact of the LTV limit on liquidity in both level terms and logs, where the latter can be interpreted as the percentage point difference in liquid assets over time. Columns [1] and [4] indicate that, as previously established, the liquidity position of affected households takes a substantial hit in the year of the purchase. However, as shown in the remaining columns, affected households rebuild their liquidity buffers in the year following the purchase such that the difference in liquidity falls from 13.0 to 5.8 percentage points (difference significant at the 5% confidence level). Moreover, this pattern continues into the next year, so that by the end of the second year affected and control households' liquidity has converged (i.e., the difference in liquidity is statistically indistinguishable).

While it is unclear whether this is the result of households actively changing their behavior (e.g., delaying consumption) or the passive pass-through of lower mortgage payments, the liquidity position of first-time homebuyers appears to have fully recovered within two years.

4.1.3 Do households become more resilient to negative income shocks?

One of the central objectives of borrower-based lending limits is to ensure that excessive debt burdens do not amplify cutbacks by households *in response to adverse shocks*. To examine whether the policy is successful in this regard, we now test whether improvements in solvency due to the LTV limit make households more "resilient" in the sense that they are better able to handle negative shocks. Prior research has shown that households respond to liquidity shocks by running down bank accounts in order to mitigate the effects on consumption (e.g., Jensen and Johannesen, 2017). We conjecture 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. They may also be less likely to sustain consumption as a result.[40] In contrast, after the LTV limit lowers indebtedness, households may be less likely

[40] We would like to directly examine consumption and default behavior, however, this is infeasible due to the following data limitations. First, we cannot reliably identify shocks to net worth given our short event horizon and the fact that households' wealth is mostly derived from housing. We therefore focus on negative income shocks, which are easily identified from the tax return data. Second, given we examine negative income shocks, we cannot reliably impute consumption by inverting a budget constraint (e.g., Baker, 2018).

to exhaust their savings, since mortgage payments are now more manageable.

We examine this hypothesis using our main sample of first-time homebuyers. For simplicity, 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 $t+1$), some households receive a negative income shock. During the final year (labeled $t+2$), we measure households' liquidity position. We consider the following triple-differences model:[41]

$$\begin{aligned} Liquidity_{h,t+2} = &\beta \; After_t \times d(\widehat{LTV}_{ht} > 106) \times Negative\ Shock_{h,t+1} \\ &+ \alpha_1 \; After_t + \alpha_2 \; d(\widehat{LTV}_{ht} > 106) + \alpha_3 \; Negative\ Shock_{h,t+1} \\ &+ \alpha_4 \; Negative\ Shock_{h,t+1} \times d(\widehat{LTV}_{ht} > 106) \\ &+ \alpha_5 \; Negative\ Shock_{h,t+1} \times After_t + \alpha_6 \; After_t \times d(\widehat{LTV}_{ht} > 106) \\ &+ \alpha_{l(h)} + \alpha_{y(h)} + \alpha_{w(h)} + \alpha_{s(h)} + \epsilon_{ht}, \end{aligned} \tag{4}$$

where, as in our DiD model (3), $After_t = 1$ identifies households buying after the lending limit was implemented (from August 2011 until July 2012) and $d(\widehat{LTV}_{ht} > 106) = 1$ identifies "affected" households that have predicted LTVs strictly above 106. The newly introduced variable $Negative\ Shock_{t+1}$ is an indicator variable that is set equal to one if the household experiences a severely negative income shock in the year following the home purchase; in particular, if the household loses more than 50% of its income between the end of year t and the end of year $t+1$ ($\frac{Income_{t+1}}{Income_t} < 0.50$).[42] We consider two outcome variables, *log(Liquid Assets)*$_{h,t+2}$ and *Low Liquidity*$_{h,t+2}$, that are both measured in the year after the income shock. The latter is an indicator variable set equal to one if the household reaches very low levels of liquidity by the end of year $t+2$, i.e., the bottom decile of the liquid assets distribution. The triple-differences coefficient, β, captures the incremental liquidity carried by affected households that bought homes while subject to the lending limit and experienced a negative income shock in the following year.

Table IX shows the results.[43] Before diving in, note that households have, on average, 17.8 percentage points lower cash in bank accounts in the year following a negative income

Third, the mortgage servicing data set only provides information on income at-origination, so we cannot infer how loan performance responds to job loss.

[41]This model essentially takes specification [6] from Table VIII and introduces a negative shock at $t+1$.

[42]Approximately 2% of the sample (712 households) experiences a negative income shock. We do not find significant differences between households in terms of ex ante income, liquidity, and wealth.

[43]Note that all main effects and lower-level interaction terms shown in Equation (4) are included in the regressions, but suppressed from the table output for ease of interpretation.

shock (significant at the 1% level).[44] The table shows how reductions in borrowing under the LTV limit interacts with this household-level response to the income shock. We report average effects (column [1]) as well as estimates across liquidity constrained and unconstrained households (columns [2] to [5]). 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 an income shock (about 16.6 percentage points higher liquid assets). This finding becomes much larger in magnitude and precisely estimated when we examine the subset of financially constrained households. As shown in column [2], households with low initial liquidity have about 150 percentage points higher liquid assets after the shock (significant at the 5% level). Given that average savings among this subgroup is about €1,688, this means that households subject to the LTV limit have a liquidity buffer that is approximately €2,532 larger in size after the income shock. Panel B shows that these households are also far less likely to reach extremely low levels of liquidity. Among low initial liquidity households buying under the LTV limit, the probability of savings falling within the bottom decile (€710) decreases by 23.8 percentage points after an income shock, as compared with similar households that bought before the limit came into effect.

Taken together, the results of this section indicate that, by reducing the indebtedness of households, the LTV limit reduces instances of financial distress. This happens both in terms of the frequency of missed mortgage payments, as well as households' ability to maintain a healthy liquidity position after a negative income shock.

4.2 Transition rate into homeownership

We have thus far characterized the impacts of the lending limit *conditional on homeownership*. In theory, when the LTV constraint tightens, households may prefer to rent rather than incur the transaction costs of buying or purchasing a less desirable house (Gete and Reher, 2016). In support of this argument, survey evidence finds that households respond to a hypothetically lower downpayment requirement by stating a stronger intention of buying a house, especially households with characteristics indicative of financial constraints (Fuster and Zafar, 2015, 2016). In this final section, we therefore take a step back and measure how the lending limit impacts the extensive margin decision to purchase a house among

[44] This measurement (unreported) is based on estimating equation (4) only including the *Negative Shock* main effect and control variables.

observationally similar households coming to the market before and after the policy change.

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 1,278,960 renters entering either in the year before or in the year after the policy shock (i.e., two years). These are households meeting our data requirements 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. The indicator variable $Homeownership^{1yr}$ encodes this transition at the household level at the one-year horizon. In the aggregate, $Homeownership^{1yr}$ captures the one-year-ahead rate at which households transition from renting to owning, as opposed to the level of homeownership in the Netherlands. About 3.7% of the 637,751 renters end up buying a house in the one-year period before policy shock. Afterward, this rate drops to 3.3% suggesting that the policy might have curtailed homeownership in the immediate short term.

Panel A of Table X shows the results of estimating Equation (3) on the sample of renting households with $Homeownership^{1yr}$ as the outcome variable.[45] The point estimate in column [1] is negative (–0.008) and statistically significant at the 1% confidence level. The magnitude indicates that affected households reduce their probability of purchasing a home by 80 basis points, which is measured relative to the control group of renters with predicted LTV ratios less than the limit if they were to purchase a home. This reduction is about 21.6% of the average one-year-ahead probability of transitioning into homeownership in the before period (0.037), which is clearly a sizable effect. Moreover, when we partition the households on the basis of ex ante liquidity or total financial assets, the DiD estimates increase in magnitude. Thus, household liquidity constraints appear to amplify the negative effects of the LTV limit on renters' transition into homeownership (at least at the one-year horizon).

4.2.1 Lower *or slower* transition rate into homeownership?

It is unclear, however, whether this reduction in access to homeownership is a purely temporary shift or rather a permanent response among liquidity-constrained households. For example, if the majority of constrained households are able to privately save or receive funds from third parties (e.g., gifts from family members) to finance the transaction, then homeownership may eventually become feasible. On the other hand, some households may

[45] Parameters of the LTV prediction model continue to be based on the (unconstrained) LTV ratios of first-time homebuyers in the period before the policy implementation.

lack the private means to save leading to longer-term exclusion from the housing market.

We consider the two-year transition rate from renting into homeownership in the years surrounding the introduction of the limit to shed light on this issue.[46] We double the length of the before period (August 2009 to July 2011) and after period (August 2011 to July 2013). The outcome variable, $Homeownership^{2yr}$, is now set equal to one if the initially renting household transitions into owning by the end of the two-year period. Comparing the one- and two-year transition rates provides suggestive evidence as to whether households' extensive margin response to the regulation is longer-term or temporary.

Panel B of Table X shows that the two-year transition rates into homeownership are indeed lower both on average as well as among the subset of liquidity-constrained households. In particular, the average effect attenuates from the aforementioned –0.008 (column [1] of panel A) to –0.005 (column [1] of panel B). Importantly, given the mean transition rate into homeownership at the two-year horizon is larger in size (5.6% in the before period; see Table I), this point estimate corresponds to a smaller economic effect—roughly a 8.9% reduction when evaluated at the mean. This suggests that the policy shock may have had a temporary effect. When we split households according to ex ante financial assets, two interesting results support this notion. First, we see that the estimate becomes more negative and remains significant at the 1% level among liquidity-constrained households. Second, among relatively wealthy households, the negative effect becomes smaller in magnitude and is less precisely estimated. In particular, when evaluated at the before period mean, households with low (high) liquidity reduce the two-year transition rate into homeownership to 13.5% (5.2%).[47] Thus, at the two year horizon, the extensive margin effects of the policy persist but appear to be less severe, particularly among relatively wealthy households. Put differently, the LTV limit slowed the transition rate into homeownership for these households.

5 Conclusion

We provide new insights 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

[46]We refrain from examining longer-term household responses to minimize concerns regarding confounding economic events. For example, as mentioned in Section 1.1, in 2013 adjustments in eligibility were made to the mortgage interest deduction that may have impacted incentives to buy.

[47]The average two-year rate in the before period among low (high) liquidity households is 5.2% (5.8%).

the universe of Dutch households. These data allow us to track household balance sheets and home ownership to characterize important credit and housing market outcomes.

The policy succeeded in inducing households to use less mortgage debt to finance their first housing transaction. We examine household balance sheets in the year of the purchase and find that households do not substitute to other, less regulated 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 assets (i.e., bank deposits and savings) are lower. Thus, after the policy shock, households consume liquidity in order to finance the purchase of their first home (a solvency-liquidity trade-off).

We provide direct evidence on some of the trade-offs associated with this macroprudential policy. First, the drop in mortgage debt, as well as overall household leverage and debt servicing costs, reduces the likelihood of financial distress. This occurs both in terms of mortgage repayment behavior and also households' ability to sustain a liquidity buffer after experiencing a negative income shock. Second, the rate at which renters transition into buying their first home declines, especially among households lacking sufficient cash 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 household solvency 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 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 suggests 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 directly investigate how lower indebtedness enables households to smooth consumption in response to negative income or wealth shocks. Our results on household liquidity suggest that the lending limit may also render consumption more resilient to negative shocks, but additional confirmatory evidence is required before drawing a strong conclusion. Understanding the effects on household consumption is especially important in countries like the Netherlands where mortgage debt is full recourse and mortgage payments have priority even in personal bankruptcy.

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.

Fourth, 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.

Finally, 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.

All of these important questions are beyond the scope of this paper.

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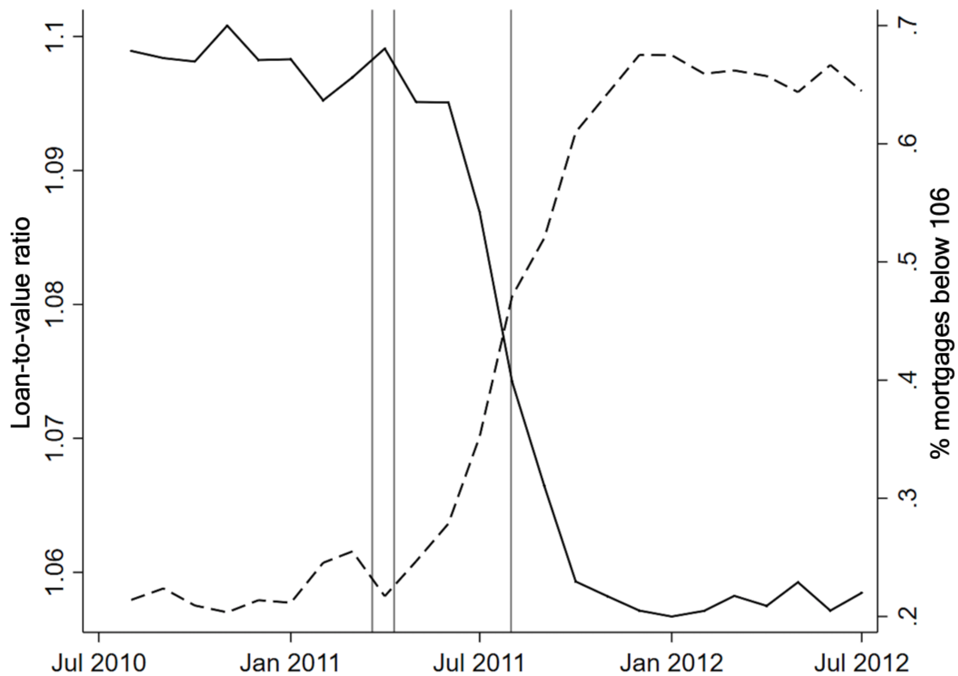

Figure 1

LTV dynamics around introduction of the limit

This figure presents the time-series for the median loan-to-value (LTV) ratio at-origination (left axis, solid line) and the fraction of loans satisfying the regulatory limit, i.e., at-origination LTV below 106 (right axis, dashed line), among first-time homebuyers. LTV is calculated as the household's mortgage amount as reported in the year after 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 vertical lines indicate when the rule was announced on March 21, 2011, confirmed and clarified on April 11, 2011, and implemented on August 1, 2011, respectively. Mortgage data comes from Statistics Netherlands (CBS) and transaction prices and property ownership information come from the Land Registry (Kadaster).

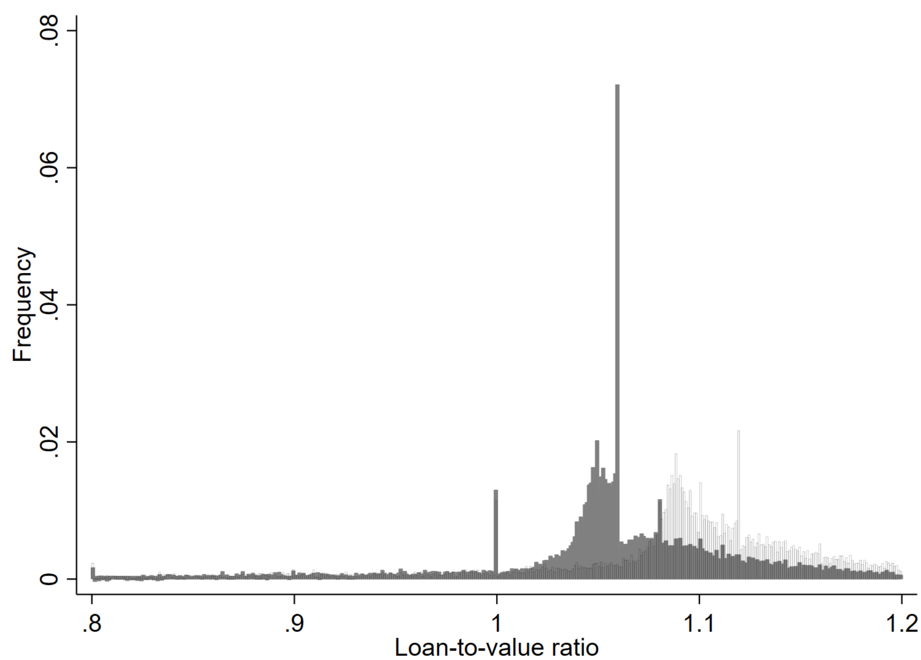

Figure 2

LTV distribution around introduction of the limit

This figure presents the frequency of mortgage transactions for first-time homebuyers for each at-origination loan-to-value (LTV) bucket both in the year before (light gray) and the year after (dark gray) the August 1, 2011 introduction of the 106 LTV limit. LTV is calculated as the household's mortgage amount as reported in the year after 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. Mortgage data comes from Statistics Netherlands (CBS) and transaction prices and property ownership information come from the Land Registry (Kadaster).

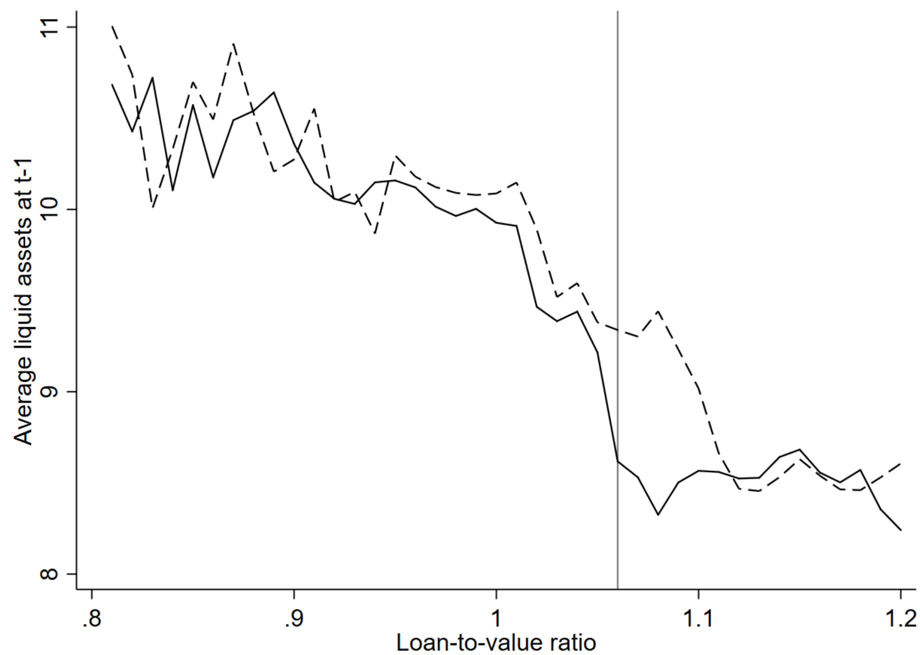

Figure 3

Pre-purchase household liquidity around introduction of the limit

This figure show average (log) liquid assets in the tax filing immediately prior to purchase for first-time homebuyers for each at-origination loan-to-value (LTV) bucket both in the year before (dashed line) and the year after (solid line) the August 1, 2011 introduction of the 106 LTV limit. LTV is calculated as the household's mortgage amount as reported in the year after 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 vertical line indicates the LTV limit of 106. Mortgage data comes from Statistics Netherlands (CBS) and transaction prices and property ownership information come from the Land Registry (Kadaster).

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 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 arrears 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 at both the one- and two-year horizons. Superscripts denote the event window used (one- or two-year) and subscripts denote measurement within the event window (beginning or end). 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
$Payment/Income_t$	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 arrears 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 homeownership tests												
$Homeowner^{1yr}$	637,751	0.037	0.188	0	0	0	641,209	0.033	0.178	0	0	0
$Income_1^{1yr}$	637,751	49,447	34,830	29,245	43,821	61,941	641,209	50,447	40,842	29,727	44,057	63,127
$Liquid\ Assets_0^{1yr}$	637,751	89,791	12,221	1,622	4,389	11,586	641,209	87,506	12,568	1,369	3,869	11,041
$Wealth_0^{1yr}$	637,751	95,596	13,164	1,701	4,621	12,254	641,209	92,316	13,369	1,413	4,044	11,580
$Homeowner^{2yr}$	450,991	0.056	0.231	0	0	0	547,920	0.054	0.223	0	0	0
$Income_2^{2yr}$	450,991	44,330	16,350	31,276	41,741	54,845	547,920	46,553	17,128	32,560	44,284	58,504
$Liquid\ Assets_0^{2yr}$	450,991	15,432	36,253	2,126	5,659	16,373	547,920	13,359	30,402	1,248	3,999	13,281
$Wealth_0^{2yr}$	450,991	18,317	94,023	2,206	5,968	17,632	547,920	15,605	43,887	1,288	4,199	14,175

Table II
Effect of LTV limit on LTV ratios

This table shows the shift in LTV among first-time homebuyers around the implementation of the 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. 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 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**	0.019***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
$d(\widehat{LTV} > 106)$								0.069***
								(0.002)
$After \times d(\widehat{LTV} > 106)$								–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 ratios by initial household liquidity

This table shows the shift in LTV among first-time homebuyers around the implementation of the LTV limit by initial household liquidity. For lagged liquid assets (bank deposits and savings only) and lagged wealth (sum of assets), we split the sample for "High" (top tercile) and "Low" (bottom tercile) 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*				
Financial constraint based on:	$Liquid\ Assets_{t-1}$		$Wealth_{t-1}$	
Sample:	Low	High	Low	High
	[1]	[2]	[3]	[4]
$After \times d(\widehat{LTV} > 106)$	–0.084***	–0.061***	–0.085***	–0.059***
	(0.006)	(0.004)	(0.006)	(0.003)
Postcode fixed effects	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y
N	10,606	11,305	8,461	16,104
R^2	0.33	0.38	0.37	0.38

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 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 × $d(\widehat{LTV} > 106)$	–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*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*Liquid Assets*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]	[6]
After × $d(\widehat{LTV} > 106)$	–2,354.52**	–0.032**	–0.104***	–0.109***	–1,668.26***	–0.130***
	(1,002.11)	(0.014)	(0.014)	(0.019)	(460.51)	(0.033)
Postcode fixed effects	Y	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y	Y
N	32,296	32,296	34,001	34,223	34,223	33,542
R^2	0.10	0.09	0.54	0.50	0.59	0.54

Table V
Effects of LTV limit: robustness checks

This table examines the robustness of the estimated changes among first-time homebuyers around the implementation of the LTV limit. 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 *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. Column [1] excludes mortgages originated between the policy announcement and implementation dates (from April until July 2011). Columns [2] and [3] include additional time fixed effects. Column [4] ([5]) considers only loans that are (in)eligible for government (NHG) guarantees, i.e., mortgages below (above) €350,000. Column [6] uses the *Wet Waardering Onroerende Zaken* (WOZ) value, where available, to measure the property value. Panel B shows the dynamics effects of LTV limit on household balance sheets. In this panel, the two-year window from August 2010 until July 2012 is partitioned into four semiannual periods: from August 2010 until January 2011 inclusive (labeled [–12m,–7m]), February 2011 until July 2011 ([–6m,–1m]), August 2011 until January 2012 ([+1m,+6m]), and February 2012 until July 2012 ([+7m,+12m]). In the regression, [–12m,–7m] is the omitted group, so that the point estimates are measured with respect to this time period. 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 (and lower-level interactions in Panel B) 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: Robustness checks for effects of LTV limit on LTVs

Dependent variable: *LTV*

Robustness test:	Alt. timing	Time fixed effects		NHG eligible?		V = WOZ
Sample:	Exc. Apr.-Jul.	All	All	Yes	No	Where avail.
	[1]	[2]	[3]	[4]	[5]	[6]
After × $d(\widehat{LTV} > 106)$	–0.061***	–0.064***	–0.054***	–0.066***	–0.026	–0.061***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.019)	(0.005)
Postcode fixed effects	Y	Y	N	Y	Y	Y
Income percentile fixed effects	Y	Y	Y	Y	Y	Y
Wealth percentile fixed effects	Y	Y	Y	Y	Y	Y
Liquid Assets percentile fixed effects	Y	Y	Y	Y	Y	Y
Month-of-year fixed effects	N	Y	N	N	N	N
Postcode×month fixed effects	N	N	Y	N	N	N
N	28,003	34,223	20,451	32,942	924	26,732
R^2	0.33	0.34	0.55	0.34	0.56	0.28

Panel B: Dynamic effects of LTV limit

Dependent variable:	*LTV*	*log(Mortgage Amount)*	*log(Home Value)*	*Mortgage Payment*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*Liquid Assets*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
[–6m,–1m] $\times\ d(\widehat{LTV} > 106)$	0.003	0.007	0.004	–2,002.08	–0.021	0.016	0.036	–803.08*	–0.057
	(0.003)	(0.006)	(0.006)	(1,646.44)	(0.021)	(0.024)	(0.028)	(443.69)	(0.040)
[+1m,+6m] $\times\ d(\widehat{LTV} > 106)$	–0.064***	–0.044***	0.016***	–3,554.89***	–0.044**	–0.113***	–0.117***	–2,807.44***	–0.132***
	(0.003)	(0.006)	(0.006)	(1,222.47)	(0.019)	(0.025)	(0.031)	(438.86)	(0.044)
[+7m,+12m] $\times\ d(\widehat{LTV} > 106)$	–0.061***	–0.029***	0.027***	–3,263.57**	–0.042**	–0.071***	–0.056*	–1,383.19***	–0.175***
	(0.003)	(0.007)	(0.007)	(1,233.62)	(0.019)	(0.025)	(0.028)	(356.14)	(0.045)
Postcode fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Income percentile fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Wealth percentile fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Liquid Assets percentile fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	34,223	34,022	33,950	32,296	32,296	34,001	34,223	34,223	33,542
R^2	0.034	0.69	0.70	0.10	0.09	0.54	0.50	0.59	0.56

Table VI
Effect of LTV limit on household balance sheets: matching estimator

This table reports summary statistics and estimates from a nearest-neighbor matching estimator. "Affected" households are those from the main sample that receive mortgages with an LTV of 105 or 106 in the year following the policy implementation (from August 2011 until July 2012). This results in 3,852 households after the below-mentioned restrictions are applied. Candidate control households receive mortgages in the year before the policy implementation. Each affected household is matched to a candidate control exactly on two-digit post code and then using a nearest-neighbor match based on euclidean distance, matching on $Income_t$, $Liquid\ Assets_{t-1}$, $Wealth_{t-1}$. We then apply the following restrictions to the matched sample: we trim (i) the worst one percent of matches for each matching variable; (ii) post codes with fewer than five affected households. Panel A reports summary statistics for the affected households and differences (Δ_{i-j}) between affected and matched control. Panels B and C show estimates for the effect of the LTV limit on the difference in mortgage characteristics and household balance sheet outcomes for the affected and matched controls accounting for residual differences in household characteristics. Heteroscedasticity-robust standard errors are clustered by month and shown in parentheses. ***, **, and * denote 1%, 5%, and 10% statistical significance, respectively.

Panel A: Summary statistics for matched sample

	N	Mean	Std.	p25	Med.	p75	[t-stat.]
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
LTV_t	3,852	1.056	0.003	1.053	1.057	1.060	
$Income_t$	3,852	70,411	25,831	52,005	65,970	83,691	
$Liquid\ Assets_{t-1}$	3,852	11,108	15,271	2,233	5,679	13,929	
$Wealth_{t-1}$	3,852	15,303	31,626	2,298	5,979	15,319	
$\Delta_{i-j} Income_t$	3,852	88.57	3,453	–1,574	–80.00	1,520	[1.12]
$\Delta_{i-j} Liquid\ Assets_{t-1}$	3,852	–259.7	8,340	–596.5	–23.00	518.0	[–1.17]
$\Delta_{i-j} Wealth_{t-1}$	3,852	–35.96	8,417	–539.5	–13.00	478.5	[–0.35]

Panel B: Components of LTV

Dependent variable:	*LTV*	*log(Mortgage Amount)*	*log(Home Value)*
	[1]	[2]	[3]
After	–0.036***	–0.086***	–0.054***
	(0.001)	(0.005)	(0.005)
Δ_{i-j} borrower controls	Y	Y	Y

Panel C: Household debt and liquidity

Dependent variable:	*Mortgage Payment*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*Liquid Assets*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]	[6]
After	–932.23***	–0.026**	–0.276***	–0.290***	–2,396.34***	–0.165**
	(62.38)	(0.010)	(0.012)	(0.013)	(428.45)	(0.065)
Δ_{i-j} borrower controls	Y	Y	Y	Y	Y	Y

Table VII
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. 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 top-tercile ("High") and bottom-tercile ("Low") subgroups of 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*			
Financial constraint based on:		$Income_0$	
Sample:	All	Low	High
	[1]	[2]	[3]
$After \times d(\widehat{LTV} > 106)$	–0.023***	–0.028**	–0.015
	(0.007)	(0.012)	(0.013)
Postcode fixed effects	Y	Y	Y
Loan control variables	Y	Y	Y
Borrower control variables	Y	Y	Y
N	77,751	24,803	27,246
R^2	0.01	0.02	0.01

Table VIII
Dynamics of household liquidity in years surrounding home purchase

This table shows the adjustments in households' liquidity in the years surrounding the 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. *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: *Liquid Assets*	Levels			Logs		
Year relative to home purchase:	t	$t+1$	$t+2$	t	$t+1$	$t+2$
	[1]	[2]	[3]	[4]	[5]	[6]
$After \times d(\widehat{LTV} > 106)$	–1,668.26***	–883.59*	–495.48	–0.130***	–0.058**	0.022
	(460.51)	(515.48)	(708.37)	(0.033)	(0.025)	(0.034)
Postcode fixed effects	Y	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y	Y
N	34,223	32,769	31,672	33,542	31,930	30,840
R^2	0.59	0.55	0.48	0.54	0.50	0.49

Table IX
Household liquidity response to a negative income shock

This table shows the adjustments in households' liquidity in response to a negative income shocks 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 with LTV ratios between 80 and 120. For these homeowners, we use the following timeline: buy house during year t, receive negative income shock during year $t+1$, and measure liquidity during year $t+2$. *After* identifies households as buying before or after the LTV limit, i.e., is an indicator equal to one from August 2011 until the end of the sample, and zero otherwise. *Negative Shock* is equal to one if the household loses more than 50% of its income between the end of year t and the end of year $t+1$ ($\frac{Income_{t+1}}{Income_t} < 0.5$). Panel A uses the natural logarithm of liquid assets at the end of year $t+2$ to capture differences in household liquidity in response to the negative income shock. Panel B instead uses a low liquidity dummy that is set equal to one if liquid assets is in the bottom decile of the distribution at the end of year $t+2$, 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. For lagged liquid assets and wealth, we split the sample into "High" (top tercile) and "Low" (bottom tercile) subgroups. Borrower control variables include income, wealth, and liquid assets percentile fixed effects. All main effects and lower-level interaction terms are included in the regressions but suppressed from the table output 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: Level of liquid assets					
Dependent variable: *Liquid Assets*					
Financial constraint based on:		$Liquid\ Assets_{t-1}$		$Wealth_{t-1}$	
Sample:	All	Low	High	Low	High
	[1]	[2]	[3]	[4]	[5]
After × $d(\widehat{LTV} > 106)$ × *Negative Shock*	0.166	1.486**	–0.385	1.499**	–0.418
	(0.379)	(0.617)	(0.593)	(0.677)	(0.588)
Postcode fixed effects	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y
N	30,840	8,960	10,295	8,755	10,555
R^2	0.49	0.29	0.39	0.29	0.46

Panel B: Low liquid assets dummy					
Dependent variable: *Low Liquidity*					
Financial constraint based on:		$Liquid\ Assets_{t-1}$		$Wealth_{t-1}$	
Sample:	All	Low	High	Low	High
	[1]	[2]	[3]	[4]	[5]
After × $d(\widehat{LTV} > 106)$ × *Negative Shock*	–0.076	–0.238*	–0.007	–0.305**	0.022
	(0.056)	(0.116)	(0.051)	(0.144)	(0.053)
Postcode fixed effects	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y
N	34,223	10,225	11,305	9,970	11,610
R^2	0.19	0.23	0.20	0.23	0.26

Table X
Effect of LTV limit on the transition rate into homeownership

This table shows the shift in the homeownership rate among the population around the implementation of the LTV limit. Panel A examines transitions into homeownership at the one-year horizon. The unit of observation in each regression is a household. The sample includes home purchase data from August 2010 until July 2012. $Homeowner^{1yr}$ is an indicator variable equal to one if the household transitions from renter to first-time homebuyer 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. Panel B examines transitions into homeownership over a two-year horizon. The sample covers purchase data from August 2009 until July 2013. $Homeowner^{2yr}$ is an indicator variable equal to one if the household transitions from renter to first-time homebuyer at some point during the following two years 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. The sample is restricted to LTV ratios between 80 and 120. In each panel, the predicted LTV ($\widehat{LTV}$) is based on a linear regression model estimated on mortgages originated in the period before the LTV limit, and fitted out-of-sample on mortgages originated in the after period. LTV is predicted using income and income squared and wealth percentile fixed effects interacted with two-digit postcode fixed effects. For tests based on financial constraints, we split the sample for "High" (top tercile) and "Low" (bottom tercile) subgroups based on initial liquid assets and wealth. 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: Transition rate at one-year horizon					
Dependent variable: $Homeowner^{1yr}$					
Financial constraint based on:		$Liquid\ Assets_0^{1yr}$		$Wealth_0^{1yr}$	
Sample:	All	Low	High	Low	High
	[1]	[2]	[3]	[4]	[5]
$After \times d(\widehat{LTV} > 106)$	–0.008***	–0.011***	–0.008***	–0.011***	–0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Postcode fixed effects	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y
N	1,278,960	421,918	434,676	421,868	434,691
R^2	0.04	0.04	0.05	0.04	0.05

Panel B: Transition rate at two-year horizon					
Dependent variable: $Homeowner^{2yr}$					
Financial constraint based on:		$Liquid\ Assets_0^{2yr}$		$Wealth_0^{2yr}$	
Sample:	All	Low	High	Low	High
	[1]	[2]	[3]	[4]	[5]
$After \times d(\widehat{LTV} > 106)$	–0.005***	–0.007***	–0.003*	–0.007***	–0.004**
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Postcode fixed effects	Y	Y	Y	Y	Y
Borrower control variables	Y	Y	Y	Y	Y
N	998,911	329,485	339,478	329,467	339,480
R^2	0.04	0.05	0.04	0.05	0.05

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 transaction price	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
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