Internet Appendix for

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

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Appendix A: Auxiliary Results

We provide several robustness checks and additional tests to buttress the main findings of the paper:

1. **Dynamics of household liquidity in years following home purchase.** Appendix Table IA.III provides evidence that households affected by the LTV limit may quickly rebuild their liquidity buffers in the years following the home purchase. 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 logs, which can be interpreted as the percentage point difference in liquid assets over time. Column [1] indicates 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 to 13.9 percentage points (difference significant at the 1% confidence level). Moreover, this pattern continues into the next year, so that by the end of the second year affected households are no longer in a weaker position in terms of their cash reserves.

2. **Extensions of household balance sheet effects in Table I.**

   (a) **Appendix Table IA.IV** measures the importance of household financial constraints for the adjustment of household balance sheets. We stratify households by income in the year prior to the purchase and create "Low" (Panel A) and "High" (Panel B) income subgroups (above and below median of the distribution, respectively) on which we separately conduct our matched sample analysis. Two main results emerge from the table. First, the sign and statistical significance of the point estimates indicate that both relatively high and low income groups respond to the lending limit in the same way. Second, there are not meaningful differences in the magnitudes of the policy response between the groups.

   (b) **Appendix Table IA.V** examines whether the reform has different effects across ex-ante "hot" and "cold" housing markets (two-digit postcodes). To this end, we partition housing markets and identify hot markets as those in the year before the reform with: (a) above-median transaction volume (based on total number of home sales); (b) above-median house prices (based on average price per home sale); (c) among the ten largest cities based on population. The results are shown in panels A to C of Appendix Table IA.V. Two interesting results emerge. First, there does not appear to be a larger reduction in LTVs post-reform for hot markets. Second, there is some suggestive evidence that the reform has more bite for hot markets in terms of incremental reductions in the prices of bought homes, mortgage payments, mortgage payment-to-income ratios, mortgage debt-to-income ratios, and overall household leverage. However, the economic magnitudes of these differences are not that large and we do not detect statistically significant differences in these outcomes. Overall, this suggests that the reform has a similar effect on both hot and cold housing markets.

   (c) **Appendix Table IA.VI** conducts several robustness tests and specification checks. First, we confirm that our results hold under alternative timings mitigating concerns about policy anticipation: (i) we exclude households buying homes after the policy announcement but before implementation (Panel A), and (ii) we use the announcement date rather than the implementation date to partition affected from control buyers (Panel B). Second, we find similar results when we relax the definition of affected households to include LTVs between 100 and 106 (Panel C), indicating that there is not something unusual about the 106-LTV households. Third, our results remain the same when we only match households using the financial variables (i.e., those variables likely to be utilized in loan applications; Panel D). Fourth, in Panel E, we falsify our results using the subset of underwater movers; that is, current homeowners that are "grandfathered in" under the new regulation and permitted to carry forward prior debt in violation of the LTV limit. We find that these exempted households behave in line with underwater movers from the period before the regulation, indicating that the findings in Table I are unlikely to reflect some confounding macroeconomic event.[1]

3. **Investigation of potential policy leakages.** In the spirit of Acharya et al. (2022), lenders could substitute from non-exempt to exempt mortgages. For example, lenders could increase the supply of high-LTV loans to exempt households, especially if these borrowers are riskier and command higher interest rates. We conduct two sets of tests to directly investigate credit supply to non-affected borrowers. First, we examine whether exempt borrowers buying homes under the new regime experience new financing conditions. Under the conjecture that the regulation "leaks" into new high-LTV refinancing post-reform, then we may expect to see these households increasing borrowing and

[1] We also confirm that our results are robust (not shown) along two further dimensions. First, we find similar point estimates when we consider the "life events" subsample of households, again consistent with a lack of sample selection in these intensive margin tests. Second, we use alternative clustering of standard errors: we cluster by postcode only, cluster at the postcode-by-month-of-purchase level, and double cluster by month-of-purchase and postcode.

levering up. Panel E of the Appendix Table IA.VI suggests that this is not the case: these exempt households seem to borrow under similar conditions as before the reform. Second, in **Appendix Table IA.VII** we examine the EDW loan-level dataset to investigate the credit supply and the ex-post performance of households borrowing in excess of the LTV limit post-reform. In line with the findings based on exempt households' balance sheets, credit supply appears unchanged post-reform (high-LTV borrowers receive similar interest rates and loan amounts) and repayment behavior appears unchanged. This finding emerges both on average (columns [1] to [3]) and within-lender (columns [4] to [6]) suggesting that leakages of this variety are not occurring in this context.

4. **Validity of arrears results in Table II.** We conduct two placebo tests (unreported). First, we approximate exempt households by focusing on current homeowners with mortgages that are refinanced (as opposed to purchases). Second, we examine an alternate salient threshold: LTV just below 100. We use households borrowing after with LTV between 95 and 100 and compare suitably matched households borrowing unconstrained before. In both cases, there is no effect of the regulation on arrears, both on average and between the income subgroups. This minimizes the concern that repayment performance is improving among all households over time. It also addresses the concern that because we have a shorter performance window post-policy, our framework could mechanically generate better performance for the lower LTV loans.

5. **Validity of resilience results in Table III.**

   (a) We obtain qualitatively similar results (unreported) when we instead consider changes households' average hourly wage (total income divided by hours worked) rather than income. We identify households in the bottom quartile of wage growth (between the end of year t and the end of year $t+1$) as experiencing an adverse income outcome. Arguably, growth in hourly wages is harder to adjust than income growth (e.g., by adjusting hours worked).

   (b) In a placebo analysis (unreported), we examine this consumption-liquidity response among households borrowing away from the 106-LTV threshold. Given data constraints, we consider households borrowing with an LTV in the 80 to 100 range. We then compare households borrowing after versus before at similar LTV ratios (i.e., similar mortgage leverage), and matched in terms of the usual financial, demographic, and location variables. Contrasting the results of this section, we find no differential effect of the introduction of the LTV limit on households' consumption-liquidity response to income loss.

Table IA.I: Variable definitions

This appendix presents the definitions for the variables used throughout the paper. In the source column, "CBS," "ED," "K," "RDW," and "SC" stand for Statistics Netherlands (CBS), European Datawarehouse, Land Registry (Kadaster), Netherlands Vehicle Authority, 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	CBS
Home Value	Transaction price of house	K
Mortgage Payment	Reduction in mortgage amount plus interest expense	CBS
Income	Pre-tax household labor income	CBS
Liquid Assets	Deposits and bank savings	CBS
Wealth	Total assets	CBS
Net Wealth	Total assets minus total debt	CBS
Student Debt	Indicator on if any adult member of household carries any student debt	CBS
Number of Kids	Number of kids	CBS
max(Age)	Maximum age of household members	CBS
min(Age)	Minimum age of household members	CBS
Single Parent	Indicator on if household includes single parent	CBS
Immigrant	Indicator on if any adult member of household is foreign born	CBS
Num. Cars	Number of cars registered to household	RDW
Car Weight	Total weight of cars registered to household	RDW
Payment/Income	Mortgage payment divided by household income	CBS
Mortgage Debt/Income	Mortgage amount divided by household income	CBS
Total Debt/Income	Total debt divided by household income	CBS
Payment Arrears	Indicator on if mortgage has payment arrears	ED, SC

Table IA.II: Summary statistics

This table provides summary statistics for samples of Dutch households used in the empirical analysis from August 2010 until July 2012. The unit of observation is a household. The sample is shown either in the period one year before (i.e., the last tax filing before) the rule change and one year after (i.e., the first tax filing after). The new LTV limit was introduced on August 1, 2011. Columns [1] and [2] show statistics for the population of renting households in the year prior to the rule change. The remaining columns summarize the data for samples of first-time homebuyers. Columns [7] and [8] matches each household buying at the limit (i.e., LTV equals 105 or 106) after the rule change to a household buying unconstrained before the rule change. The samples of buyers are restricted to LTV ratios between 80 and 120. All variables are defined in Appendix Table IA.I.

Sample:	Population		LTV=105/6		Any LTV		Any LTV (matched)	
Timing:	Before		After		Before		Before	
Restrictions:	Renters		First-time buyers		First-time buyers		First-time buyers	
	N	Mean	*N*	Mean	*N*	Mean	*N*	Mean
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Income	406,981	37,572	1,308	53,638	10,479	58,468	1,308	53,031
Liquid Assets	406,981	8,236	1,308	9,082	10,479	16,677	1,308	8,578
Total Wealth	406,981	8,803	1,308	10,122	10,479	23,465	1,308	9,669
Student Debt	406,981	0.20	1,308	0.33	10,479	0.31	1,308	0.33
Number of Kids	406,981	0.33	1,308	0.08	10,479	0.25	1,308	0.08
max(Age)	406,981	32.74	1,308	29.58	10,479	30.72	1,308	29.33
min(Age)	406,981	25.84	1,308	27.04	10,479	24.40	1,308	26.87
Single Parent	406,981	0.14	1,308	0.01	10,479	0.06	1,308	0.01
Immigrant	406,981	0.20	1,308	0.10	10,479	0.14	1,308	0.10

Table IA:III: 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. *Affected* households borrow at the LTV limit (i.e., LTV equals 105 or 106) in the after period (i.e., from August 2011 until the end of the sample). Each affected household is matched to a household buying unconstrained before the rule change. N households are matched with replacement based on the characteristics shown in Appendix Table IA.II in the year prior to the home purchase. All variables are defined in Appendix Table IA.I. Standard errors are clustered by month-of-purchase. *, **, and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Dependent variable: *log(Liquid Assets)*			
Year relative to home purchase:	t	$t+1$	$t+2$
	[1]	[2]	[3]
Affected	–0.328***	–0.139***	0.081*
	(0.075)	(0.031)	(0.043)
Δ_{i-j} matching vars.	Y	Y	Y
N	1,283	1,278	1,019

Table IA.IV: Effects of LTV limit by household income

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

Dependent variable:	*LTV*	*log(Mortgage Amount)*	*log(Home Value)*	*log(Mortgage Payment)*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel A: Low income group								
Affected	–0.049***	–0.099***	–0.068***	–0.114***	–0.010**	–0.420***	–0.414***	–0.349***
	(0.002)	(0.007)	(0.008)	(0.018)	(0.004)	(0.023)	(0.024)	(0.078)
N	1,308	1,292	1,304	1,290	1,226	1,218	1,224	1,262
Panel B: High income group								
Affected	–0.046***	–0.096***	–0.050***	–0.067***	–0.012***	–0.339***	–0.343***	–0.312***
	(0.002)	(0.006)	(0.007)	(0.018)	(0.003)	(0.030)	(0.031)	(0.084)
N	1,308	1,292	1,312	1,306	1,362	1,398	1,392	1,304
Δ_{i-j} matching vars.	Y	Y	Y	Y	Y	Y	Y	Y

Table IA.V: Effects of the LTV limit in "hot" and "cold" housing markets

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 in "hot" and "cold" housing markets. We partition housing markets (two-digit postcodes) and identify hot markets as those with: (a) above-median transaction volume (based on total number of home sales); (b) above-median house prices (based on average price per home sale); (c) among the ten largest cities based on population. The unit of observation in each regression is a household. The sample includes homeowners that purchase houses from August 2010 until July 2012. The sample is restricted to LTV ratios between 80 and 120. Affected households borrow at the LTV limit (i.e., LTV equals 105 or 106) in the after period (i.e., from August 2011 until the end of the sample). Each affected household is matched to a household buying unconstrained before the rule change. N households are matched with replacement based on the characteristics shown in Appendix Table IA.II in the year prior to the home purchase. All variables are defined in Appendix Table IA.I. Standard errors are clustered by month-of-purchase. *, **, and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Panel A: Hot based on above-median transaction volume (based on total number of home sales)

Dependent variable:	*LTV*	*log(Mortgage Amount)*	*log(Home Value)*	*log(Mortgage Payment)*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Affected×*Hot Market*	0.002	–0.008	–0.008	0.001	0.004	–0.014	–0.029	–0.018
	(0.002)	(0.007)	(0.007)	(0.022)	(0.004)	(0.040)	(0.043)	(0.053)
Affected	–0.050***	–0.091***	–0.053***	–0.089***	–0.011***	–0.354***	–0.345***	–0.318***
	(0.001)	(0.006)	(0.006)	(0.016)	(0.003)	(0.029)	(0.033)	(0.088)
Δ_{i-j} matching vars.	Y	Y	Y	Y	Y	Y	Y	Y
N	1,308	1,308	1,308	1,248	1,308	1,308	1,308	1,283

Panel B: Hot based on above-median house prices (based on average price per home sale)

Dependent variable:	*LTV*	*log(Mortgage Amount)*	*log(Home Value)*	*log(Mortgage Payment)*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Affected×*Hot Market*	0.006***	–0.014	–0.014	–0.015	–0.003	–0.034	–0.029	–0.002
	(0.002)	(0.011)	(0.014)	(0.022)	(0.004)	(0.053)	(0.056)	(0.046)
Affected	–0.052***	–0.088***	–0.049***	–0.079***	–0.007**	–0.343***	–0.344***	–0.327***
	(0.001)	(0.007)	(0.007)	(0.017)	(0.003)	(0.040)	(0.039)	(0.074)
Δ_{i-j} matching vars.	Y	Y	Y	Y	Y	Y	Y	Y
N	1,308	1,308	1,308	1,248	1,308	1,308	1,308	1,283

Panel C: Hot based on among ten largest cities based on population

Dependent variable:	*LTV*	*log(Mortgage Amount)*	*log(Home Value)*	*log(Mortgage Payment)*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Affected×*Hot Market*	0.005**	–0.011	–0.015	–0.024	–0.001	–0.034	–0.055	–0.011
	(0.002)	(0.013)	(0.014)	(0.035)	(0.006)	(0.062)	(0.065)	(0.074)
Affected	–0.050***	–0.092***	–0.052***	–0.080***	–0.009***	–0.351***	–0.343***	–0.325***
	(0.001)	(0.006)	(0.006)	(0.016)	(0.003)	(0.028)	(0.029)	(0.087)
Δ_{i-j} matching vars.	Y	Y	Y	Y	Y	Y	Y	Y
N	1,308	1,308	1,308	1,248	1,308	1,308	1,308	1,283

Table IA.VI: Further robustness checks

This table shows robustness tests for 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. Unless indicated otherwise, *Affected* households borrow at the LTV limit (i.e., LTV equals 105 or 106) in the after period (i.e., from August 2011 until the end of the sample). Each affected household is matched to a household buying unconstrained before the rule change. N households are matched with replacement based on the characteristics shown in Appendix Table IA.II in the year prior to the home purchase. Panels A and B consider alternative timing for the rule change. Panel A excludes homes purchased in the period between announcement and implementation. Panel B instead uses the rule announcement date to partition homes purchased before and after the rule change. Panel C expands the set of affected households to those borrowing with an LTV between 100 and 106. Panel D matches households only on the basis of financial information (lagged income, wealth, and liquid assets). Panel E repeats the analysis for a set of exempted "underwater movers"—households that are current homeowners, moving house, and permitted to violate the LTV limit. All variables are defined in Appendix Table IA.I. Standard errors are clustered by month-of-purchase. *, **, and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Dependent variable:	*LTV*	*log(Mortgage Amount)*	*log(Home Value)*	*log(Mortgage Payment)*	*Payment /Income*	*Mortgage Debt /Income*	*Total Debt /Income*	*log(Liquid Assets)*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Panel A: Alternative timing (exc. Apr.-Jun.)								
Affected	–0.052***	–0.116***	–0.075***	–0.077***	–0.006*	–0.447***	–0.439***	–0.361***
	(0.001)	(0.006)	(0.005)	(0.017)	(0.003)	(0.022)	(0.025)	(0.079)
N	1,288	1,288	1,288	1,223	1,288	1,288	1,288	1,269
Panel B: Alternative timing (announcement date)								
Affected	–0.052***	—0.112***	–0.070***	–0.079***	–0.008**	–0.428***	–0.423***	–0.348***
	(0.001)	(0.005)	(0.005)	(0.016)	(0.003)	(0.020)	(0.025)	(0.074)
N	1,341	1,341	1,341	1,275	1,341	1,341	1,341	1,322
Panel C: Alternative affected group (100≤LTV≤106)								
Affected	–0.054***	–0.052***	–0.008**	–0.038**	0.002	–0.208***	–0.203***	–0.226***
	(0.001)	(0.003)	(0.003)	(0.014)	(0.002)	(0.014)	(0.019)	(0.055)
N	2,968	2,968	2,968	2,791	2,968	2,968	2,968	2,924
Panel D: No matching on family characteristics								
Affected	–0.051***	–0.100***	–0.060***	–0.104***	–0.010***	–0.383***	–0.376***	–0.367***
	(0.001)	(0.006)	(0.005)	(0.015)	(0.002)	(0.019)	(0.028)	(0.055)
N	1,511	1,512	1,511	1,405	1,511	1,511	1,511	1,483
Panel E: Falsification (underwater prior home buyers)								
Exempted	0.003	0.013	0.005	–0.003	0.000	–0.032	–0.007	–0.144
	(0.003)	(0.017)	(0.015)	(0.029)	(0.005)	(0.060)	(0.061)	(0.102)
N	277	277	277	261	277	277	277	271
Δ_{i-j} matching vars.	Y	Y	Y	Y	Y	Y	Y	Y

Table IA.VII: Non-affected households' mortgage loan conditions and repayment behavior

This table shows the effects of the LTV limit on households' mortgage loan conditions and repayment behavior around the implementation of the LTV limit. The "non-affected" households in this analysis are those borrowing in excess of the LTV limit after the reform comes into effect. The unit of observation in each regression is a mortgage. The sample includes mortgages originated for purchase by employed individuals between August 2010 until July 2012. Included mortgages must be used to purchase a home (as opposed to, say, refinance) and the borrower is a salaried employee (as opposed to a pensioner, student, self-employed individual, etc.). N mortgages are matched with replacement to a nearest-neighbor using at-origination family income, age, as well as an exact match on postal code. *Interest Rate* and *Loan Amount* are the interest rate in percent terms and loan amount (in euros) at the time of origination. *Payment Arrears* is an indicator variable equal to one if a loan enters payment arrears and zero otherwise. *Non-Affected* is an indicator equal to one from August 2011 until the end of the sample, and zero otherwise. Non-affected households borrow in excess of the LTV limit (i.e., LTV strictly greater than 106) in the after period (i.e., from August 2011 until the end of the sample). All variables are defined in Appendix Table IA.I. Standard errors are clustered by month-of-purchase. *, **, and *** indicate statistical significance at the 10% level, 5% level, and 1% level, respectively.

Panel A: No match on lender identity			
Sample: $LTV>106$			
Dependent variable:	*Interest Rate*	*log(Loan Amount)*	*Payment Arrears*
	[1]	[2]	[3]
Non-Affected	0.017	0.009	0.004
	(0.041)	(0.010)	(0.004)
Δ_{i-j} matching vars.	Y	Y	Y
Match on lender	N	N	N
N	3,834	3,834	3,834

Panel B: Exact match on lender identity			
Sample: $LTV>106$			
Dependent variable:	*Interest Rate*	*log(Loan Amount)*	*Payment Arrears*
	[1]	[2]	[3]
Non-Affected	–0.011	–0.012	0.002
	(0.051)	(0.018)	(0.006)
Δ_{i-j} matching vars.	Y	Y	Y
Match on lender	Y	Y	Y
N	3,174	3,174	3,174