The Mortgage Piggybank: Building Wealth through Amortization*

Asaf Bernstein†

Peter Koudijs‡

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Abstract

Mortgage amortization schedules are among the largest savings plans in the world (e.g. U.S. households contribute hundreds of billions of dollars annually to these "mortgage piggy banks"). However, little is known about their effects on wealth accumulation. Ex-ante, effects are unclear. It depends on the fungibility of home equity and other savings, and households' willingness to adjust consumption or leisure. Empirically, effects are difficult to identify since amortization and other savings choices are typically co-determined. We overcome this challenge by utilizing a 2013 Dutch reform that increased amortization requirements for new mortgages. Using detailed administrative data, we compare savings decisions for home-buyers right before or after the reform. We use plausibly exogenous variation in the timing of home purchase coming from life-events (e.g. birth of a child) to address selection concerns. Dynamic difference-in-difference estimates support the parallel trend assumption. We find that marginal wealth-building from amortization (MWA) is substantial. Remarkably, households leave other savings untouched and cut consumption and leisure instead, implying a near 1-for-1 rise in net-worth. Results hold at least five years out when additional amortization-induced home equity is larger than other savings. The effect is ubiquitous, holding for unconstrained households, who could easily offset additional amortization, and movers, suggesting a broad applicability of our results. Consistent with a simple model, we do find that estimates are lower for households who appear less financially sophisticated or less willing to adjust short-term consumption. Overall, our results highlight the critical importance of mortgage amortization for household wealth-building and macroprudential policies.

JEL Classifications: G4, G5, G19, G21, G51, J3, R2

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†(corresponding author) University of Colorado at Boulder & NBER; asaf.bernstein@colorado.edu

‡ Erasmus University Rotterdam; koudijs@eur.ese.nl

Mortgage amortization schedules are recurring debt repayment plans that promote building savings in the form of home equity over time - akin to a "mortgage piggy bank". Amortization amounts are substantial at both the household and macroeconomic level. For example, U.S. households contribute hundreds of billions of dollars to these plans annually. This is of the same magnitude as pension contributions, the other major form of illiquid savings.[1] Despite the importance of mortgage amortization, we know surprisingly little about its effect on wealth building. Theoretically, effects are uncertain. In standard models, households would be expected to partially undo amortization via home equity withdrawals or by saving less in other accounts. Empirically, our knowledge is limited. This is likely driven by identification challenges: the decision about how much to save and the choice to purchase a house with a particular mortgage contract are jointly determined.

In this paper, we overcome these challenges by combining a plausibly exogenous increase in mortgage amortization for Dutch home-buyers around a macroprudential mortgage reform in January 2013 with detailed administrative household level data. We provide the first causal evidence of estimates of the Marginal Wealth-building from Amortization (MWA), a key parameter that has critical economic and policy implications. Prior to the reform, first-time home buyers (FTHBs) typically borrowed half of the mortgage sum interest-only. Afterwards, the vast majority borrowed the full amount through a standard fully amortizing mortgage. This caused a substantial rise in mortgage amortization. Most homeowners were "compliers" both before and after the reform, meaning that our results apply to the broader population. We look at all FTHBs who bought around the end of 2012 and beginning of 2013 and compare their wealth accumulation over the *same* later years (2014 to 2017). Differences in wealth accumulation are smooth and flat for cohorts buying before the reform, then jump up suddenly and persistently for cohorts buying after. The average MWA is close to one (0.9-1.0). Each dollar of additional amortization leads to nearly one dollar of additional wealth building. This is accomplished by cutting expenditures (around 2/3), and increasing labor supply (around 1/3). There is little evidence that households cut other forms of savings.

Our results are unlikely to be driven by selection. Dutch payment-to-income limits before the reform were already calculated as if a mortgage was fully amortizing. This means that the reform did not change households' ability to purchase a home. There is also no evidence that the probability of home purchase fell after the reform. There is no bunching in housing transactions right before the reform, or a drop thereafter, and households did not become less likely to purchase a home in response to "life-events" (changes in the number of household members such as the birth of a child). Cohorts buying after the reform

[1] In 2016, there were $10.3 trillion in U.S. residential mortgages (FRED) with 2.5% of principal scheduled to be amortized and 2.8% actually repaid in 2016 (CoreLogic), equating to $250-300 billion in savings via mortgage amortization. By comparison, there were around $398 billion in 401(k) pension contributions reported to the Department of Labor in 2016 (including both employee and employer contributions).

also did not purchase cheaper homes. Further, there is no evidence of systematic differences in observables for households buying right before or after the reform, or for differential pre-trends. Finally, we obtain similar MWA estimates when using the timing of life-events to instrument for the timing of home purchase.

Our findings are also unlikely to be driven by households who are at a corner solution that do not want to build wealth. We obtain similar estimates for households who save regularly, build-up extra home equity, and contribute to their pensions on top of the standard employer-sponsored programs. We do find smaller MWA estimates for households who reveal a preference for short-run consumption, and who appear more sophisticated. This suggests that the high full sample MWA estimate is driven by households who are willing to adjust short-run consumption to maintain their preferred level of savings or who do not fully internalize that additional amortization increases expected long-run income.

Our results continue to hold five years after the reform (when our data ends). At that point, the additional amortization-induced wealth accumulation exceeds the average household's stock of liquid assets. Further, we find no evidence that households undo the additional amortization when given the opportunity during a move, even when doing so would be relatively cheap and easy. This suggests that the impact on wealth accumulation is likely to be substantial, even in the long run.

More generally, our results suggest that amortization is a first-order driver of aggregate household wealth-building and, if access to housing is unequal, wealth inequality.[2] All else equal, a linear aggregation of our partial equilibrium estimates implies that in the absence of mortgage amortization, U.S. homeowners would save \$1.25-1.5 trillion less over the next five years. Our results also point to the importance of alternative mortgage products (and refinancing rules) and standardized mortgage maturities that affect the rate of amortization and that differ widely across countries.[3] Further, our findings have important policy implications. Freezing mortgage amortization or implementing payment moratoria, as many countries have done in response to Covid-19, appear even more effective in stimulating consumption than implied by standard models. Freezing amortization in the U.S. for a period of two years is equivalent to all TARP (Trouble Asset Relief Program) payments in the four years following the Great Recession. Our partial equilibrium estimates also have implications for broader macroprudential policies that change mortgage rules since they pin down the wealth effects of amortization *conditional* on being able to buy a home. If an increase in amortization does tighten payment-to-income requirements, households could end up buying smaller homes or no home at all. Our estimates suggests that this would have larger implications for wealth accumulation than perhaps expected. Finally, our results stand in contrast to classic models of consumer

[2] For example, over the last half-century, black households in the U.S. have been 20-30 percentage points less likely to be homeowners (U.S. census IPUMS various years).

[3] The average mortgage maturity over the past forty years in Sweden was 45 years, but in Germany was only 15 (hofinet.org).

behavior. Households appear to treat amortization-driven home equity as infungible with other forms of wealth, and do not alter savings plans in response to changes in amortization.

We position our findings in the literature in Section V. Our results imply that households' consumption and labor supply strongly respond to decreases in current disposable income, but not to increases in long-run illiquid savings. These are plausible given prior work in the literature, suggesting potentially linked fundamental mechanisms, but not obvious *ex-ante* given different estimates' context-specific nature. Further, our effects appear longer-lived than those in connected work on other illiquid savings interventions, in particular pension contribution nudges. This likely comes from the relatively high costs to undo mortgage amortization. While any parameter estimated in one country is subject to that country's institutional and cultural context, there are a number of reasons to believe that our results extend to other countries as well. In particular, prior work has established that Dutch consumption responses to income or wealth shocks are broadly in line with other countries. Also, the high MWA estimate we find is ubiquitous across different wealth and age groups. This suggests that our estimates provide an important and useful benchmark for amortization effects in other contexts as well.

The rest of the paper is structure as follows. Section I provides institutional background. Section II introduces a simple conceptual framework and formally defines the MWA, lays out its economic drivers, and links the variables of interest to what we can observe in the data. It also lays out our empirical design. Section III describes the data in more detail. Section IV has our results. We first present our baseline MWA estimate, and then discuss possible selection, households' labor response, heterogeneity, and further robustness. Section V links our MWA estimate to other estimates in the literature (in particular the marginal propensity to consume out of housing related shocks), and discusses the wider applicability of our results. Section VI concludes. Additional results are in the Online Appendix.

I. Background

I.A. Mortgage Environment in the Netherlands

Traditionally, Dutch mortgage rules were relatively loose. The country has strong recourse laws and full mortgage interest deductibility (MID), which stimulated mortgage borrowing with large interest-only (IO) components, and loan-to-value (LTV) ratios well in excess of 100%. Starting in 2001, the Dutch government began to restrict mortgage origination rules. Before the 2013 reform, the following rules were in place for mortgages to be eligible for MID and national mortgage insurance programs.[4] Maturity was limited to 30 years, and the IO component capped at 50%. The payment-to-income ratio was calculated using a fixed formula set by the Dutch National Institute for Budget Information (NIBUD) assuming that

[4] National mortgage insurance is available for mortgages with a sum below some cutoff, €320k during 2012-2013.

the entire mortgage was amortizing following an annuity schedule (even if it was not). The LTV was capped at 106% and set to drop by 1 percentage point a year to reach 100.

Before the 2013 reform, households typically borrowed up to the IO cap. The majority of mortgages originated were 50% IO and 50% amortizing. We verify this with data from the Mortgage Data Network (HDN).[5] In most cases, amortization worked through linked savings accounts. These accounts exactly replicate amortizing mortgages but have the same MID as IO mortgages. Homeowners deposit a monthly sum equal to a regular amortization amount into a savings account that has the same interest rate as the mortgage. The accumulated savings are used to fully repay the mortgage at maturity, while the interest payments on the linked savings cover (part of) the mortgage interest payments, which are fully tax deductible. Homeowners are not allowed to access the linked savings during the duration of the mortgage. Returns on savings are not taxed.

Dutch households tend to borrow up to the allowable regulatory limits (van Bekkum et al. 2019). For first-time home buyers (FTHBs) in 2012 and 2013, more than 40% of mortgage offers were within 5 percentage points of the regulatory LTV limit and around 20% were exactly at the limit. Dutch mortgage lenders typically do not consider expected future income growth when determining who to lend to.[6]

Dutch homeowners can extract or utilize home equity in three different ways. First, they can refinance the entire mortgage whereby they can cash out. Second, many households pass a mortgage deed at origination for an amount larger than the actual mortgage sum they get from the bank. This allows them to extract home equity through a second lien quite easily and cheaply as they avoid additional notary fees. Mortgage increases have to satisfy the relevant LTV and PTI constraints. Households lose the MID over the extracted home equity, unless it is used for remodels. Finally, because of the strong recourse laws, banks take the value of home equity into account when extending non-mortgage credit (i.e. consumer loans).

I.B. The 2013 Dutch reform

To promote "financial stability", the Dutch government tightened mortgage rules for home purchases contracted from January 1st, 2013 onwards. FTHBs were required to have a fully amortizing 30-year

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[5] HDN covers around 75% of mortgage offers between January 2009 and December 2014. The dataset contains detailed information on loan characteristics including the size of the mortgage and mortgage contract type. We verify that during 2011 and 2012, 98.1% of new originations were at least partially amortizing and conditional on being partially amortizing 70.8% were within 10pps of 50% amortizing.

[6] "The mortgage lender will, when determining the borrowing capacity of a mortgage applicant, consider their *current fixed* and *permanent* income. (…) (italics added for emphasis)." *Gedragdscode Hypothecaire Financieringen* (Code of Conduct for Mortgage Loans), 2011, article 6.3. For self-employed individuals, the borrowing capacity was based on the average income over the last three years. Rules remained the same around the reform; see *Tijdelijke Regeling Hypothecair Krediet* (Temporary Regulation Mortgage Credit), 2012, article 2.

mortgage in order to be eligible for MID and national mortgage insurance.[7] This excluded linked savings accounts. Non-conforming mortgages lost MID on the full mortgage balance, making them very costly. Existing homeowners buying a new home fell under the new rules for any mortgage increases, but were grandfathered under the old rules for their existing mortgages (but only for the remaining maturity).

The reform was first proposed in April 2012. For most of the year, it remained uncertain whether it would pass and, if so, in what form. On August 31st, 2012, ABN AMRO, one of the largest banks and mortgage lenders in the Netherlands, noted that "[t]he future concerning the measures is far from certain, since it is a very hot political issue. The election results on 12 September 2012 are crucial in this respect and could change the situation drastically.''[8] In the end, the reform was approved on November 20th, 2012.

Figure 1 shows that in the beginning of 2012, less than 5% of offers were for standard fully amortizing mortgages, while in the beginning of 2013 almost 95% were. IO mortgages (and linked savings accounts) virtually disappeared. The percentage of the mortgage balance expected to be repaid increased dramatically. The data suggests that households undid little-to-none of the treatment of the reform via differential voluntary repayment or home equity withdrawals. We compare expected versus actual mortgage repayment over 2015 for FTHBs buying before or after the reform. Based on information provided by HDN on mortgage offers, we expect that those buying after should have repaid an additional 1.5% of the mortgage sum. This is matched almost exactly by observed mortgage repayments in Statistics Netherlands (CBS).

The 2013 reform has four key characteristics. First, almost all FTHBs were compliers both before and after the reform. This implies that our estimates likely apply to the broader population, rather than a particular subset of households who endogenously choose IO mortgages (as would be the case in many settings like the U.S.). Second, while the reform clearly increased monthly amortization payments, it did *not* mechanically alter regulatory maximum PTI limits. Even prior to the reform, NIBUD would compute PTI limits as if the mortgage was a standard fully amortizing 30-year fixed rate loan, regardless of the actual mortgage type or terms. This means than we can isolate the effect of additional amortization from changes in households' regulatory ability to afford a given mortgage. This has important implications for the interpretation of our results, which we discuss in more detail in Section V.E. Third, mortgages were already partially amortizing, and had been for some time prior to the reform. This means we can contribute effects to increases in amortization, not to the lack of familiarity with amortization itself. Finally, the Netherlands came out of a recession in 2013, with both GDP and employment rates bottoming out. This means that

[7] Parliamentary document 33405-29. *Wijziging van de Wet inkomstenbelasting 2001 en enige andere wetten in verband met de herziening van de fiscale behandeling van de eigen woning (Wet herziening fiscale behandeling eigen woning)* (Law change income tax with respect to the fiscal treatment of residential property).

[8] "Covered Bonds in the Netherlands", ABN Amro (September 2012).

house prices and mortgage rates were largely flat around the reform (further details are in Section I.A of the Online Appendix).

I.C. The Dutch pension system

How households respond to the 2013 reform could, at least in theory, depend on their pension entitlements. The Dutch pension system consists of three pillars: (1) social security benefits (which are the same for everyone), (2) employer-sponsored pension programs (predominantly defined-benefit), and (3) voluntary pension accounts. Employer-sponsored programs are primarily negotiated through collective labor agreements. If offered, participation is mandatory. In sectors without collective labor agreements, employers are not legally required to offer a program and around 10% of people do not participate (Knoef et al. 2017). There is substantial variation in contribution rates (as a function of income) across different programs. This varies predominantly at the level of sectors or large firms (Bosch et al. 2019). Voluntary pension contributions are only possible if total contributions, including those to employer-sponsored programs, remain below a statutory cap. A substantial fraction of households make voluntarily contributions (around 9.1% in our sample). The median contribution in 2016 (conditional on contributing) was €900. People are not allowed to access pension entitlements before retirement; this includes voluntary accounts.

The high incidence of voluntary contributions indicates that many people have an interest in saving more for the long-run. This is supported by anecdotal evidence. Starting in 1990s, the Dutch government has restricted the amounts Dutch households can save in tax-exempt pension accounts (with recent restrictions in 2011 and 2015). They have done so exactly because many households were contributing at the limit, and this hurt short-term tax revenues.[9] That Dutch households are willing to save for the long term is supported by other research. Kuhn et al. (2011) show that households decide to save a large part of an exogenous shock to their wealth, while Kárpáti (2022) shows that that people meaningfully increase their liquid savings after they learn that they have a longer life expectancy.

II. Theoretical model and empirical predictions

II.A. Stylized theoretical model

We use a stylized model to better understand what the MWA exactly captures, and how it is pinned down by economic primitives. There are two periods, and the household faces the following intertemporal budget constraints (IBCs):

$$C_1 + S_1^L = Y_1 - S_1^N \tag{1}$$

$$C_2 = Y_2 + R(S_1^L + S_1^N) \tag{2}$$

[9] *Wet verlaging maximumopbouw- en premiepercentages pensioen en maximering pensioengevend inkomen (33.610); Memorie van toelichting (TK, 3)*, April 15, 2013.

where Y_1 and Y_2 are endowments that are exogenously given and C_1 and C_2 are consumption in the form of both spending and leisure. The household saves into liquid and illiquid savings, S_1^L and S_1^N respectively, where the former is a choice variable and the latter is beyond the household's control. For simplicity, we assume that the return on these savings, R, is the same for both. The household's discount rate is given by $\delta = 1/R$. We assume that households enjoy certain liquidity benefits from holding S_1^L, which can be both rational and behavioral, that they do not get from S_1^N. In particular, we assume that the household has the following preferences:

$$U = u(C_1) + \delta u(C_2) + v(S_1^L) \tag{3}$$

where $u(\ )$ and $v(\ )$ are standard concave utility functions with $f'(x) > 0$, $f''(x) < 0$, and $\lim_{x \downarrow 0} f'(x) = \infty$.

Taking the first order condition (FOC) with respect to S_1^L, we get that

$$u'(C_1) - v'(S_1^L) = u'(C_2) \tag{4}$$

In equilibrium, the household will allocate its resources between period 1 and 2 consumption, taking into account that S_1^L provides additional liquidity benefits. An increase in amortization increases a household's illiquid savings, S_1^N. If the rest of S_1^N is beyond the household's control, the household needs to adjust its equilibrium C_1 or S_1^L. The fungibility between liquid and illiquid savings is given by $F = \partial S_1^L / \partial S_1^N < 0$ and the marginal wealth building from amortization by $MWA = 1 + \partial S_1^L / \partial S_1^N$. If the household only adjusts liquid savings, then $F = -1$ and $MWA = 0$. If it only adjusts period 1 consumption, then $F = 0$ and $MWA = 1$.

Lemma 1. The MWA equals the difference between the (short term) marginal propensities to consume (MPC) out of the net-present value (NPV) of short and long-term income shocks.

Forcing the household to accumulate more S_1^N is equivalent to a negative income shock in period 1 and the present value of an equivalent positive income shock in period 2. The MWA captures to what degree households adjust their current liquid savings S_1^L, and thereby their consumption C_1, to the NPV of these two shocks. Formally,

$$\begin{aligned} MWA &= 1 + \frac{\partial S_1^L}{\partial S_1^N} = 1 - \frac{\partial S_1^L}{\partial Y_1} + \frac{\partial S_1^L}{\partial Y_2/R} \\ &= \frac{\partial C_1}{\partial Y_1} - \frac{\partial C_1}{\partial Y_2/R} = MPC_1^{ST} - MPC_1^{LT} \end{aligned} \tag{5}$$

where Y_2/R is discounted period 2 income. The MWA will be large if the MPC_1^{ST} is large and the household does not find it too costly to adjust C_1 in response to a contemporaneous income shock (and is able to do so). Also, the MWA is large if the MPC_1^{LT} is small and the household is unwilling to deviate from a given savings plan; for example, when an exogenous increase in Y_2 does not lead to a smaller S_1^L.

The next two lemmas highlight two predictions from the model that we can test empirically:

Lemma 2. The MWA depends on the relative concavity of the household's utility from consumption and liquid savings at the equilibrium allocation. In particular, holding all other concavities constant, the MWA is decreasing in $-u''(C_1)$.

Applying implicit differentiation to the FOC, it is straightforward to show that:

$$MWA = 1 + \frac{\partial S_1^L}{\partial S_1^N} = \frac{-v''(S_1^L)}{-u''(C_1) - v''(S_1^L) - Ru''(C_2)} \in (0,1) \tag{6}$$

The household will be more willing to adjust C_1 if this leads to only a small change in marginal utility $u'(C_1)$. In part, this is traded off against changes in $v'(S_1^L)$, the direct marginal utility a household gets from holding S_1^L. The MWA will be large if $-u''(C_1)$ is small and $-v''(S_1^L)$ is large; that is, if adjusting short term consumption is less costly than adjusting liquid savings.

Lemma 3. The MWA is decreasing in the household's level of financial sophistication.

We define sophistication, captured by parameter $\alpha \in [0,1]$, as the extent to which households internalize that an increase in S_1^N will make them better off in period 2. We rewrite period 2's IBC as

$$C_2 = Y_2 + R(S_1^L + \alpha S_1^N) \tag{7}$$

and the expression for the MWA from equation (6) as

$$MWA = \frac{-v''(S_1^L) - (1-\alpha)Ru''(C_2)}{-u''(C_1) - v''(S_1^L) - Ru''(C_2)} \in (0,1) \tag{8}$$

This shows that less sophisticated households (with a lower α) see less substitutability between S_1^L and S_1^N and have a higher MWA.

Financially constrained households, who have little S_1^L to begin with, might only have the option to adjust C_1 in response to an increase in S_1^N. This includes households who are subject to hyperbolic discounting or dynamic inconsistency problems. Further, households with substantial pension entitlements may not be interested in accumulating more long-term savings. If their S_1^L is intended for short term consumption only, it might be unresponsive to changes in S_1^N. In our analysis, we focus on subgroups of households who either have substantial savings, suggesting they are unlikely to be constrained, or households who make voluntary pension contributions or who have smaller pension entitlements. We expect that these groups have a meaningful demand for long term savings and are not at a "savings corner", so that their MWA is not mechanically high.

II.B. Taking the model to the data

To map the model to the actual data, we write a households' IBC as

$$C_t + S_t^L + S_t^N = Y_t + R_t^L S_{t-1}^L + R_t^N S_{t-1}^N \tag{9}$$

where C_t is consumption, S_t^L and S_t^N are liquid and illiquid savings, Y_t is after-tax labor income, and R_t^L and R_t^N are after-tax returns on liquid and illiquid savings. Liquid savings S_t^L include bank deposits, stocks, bonds and voluntary pensions, net of non-mortgage debt (we refer to this as Δ*net-liquid savings*$^{+}$ in the empirical analysis).[10] Illiquid savings S_t^N are home equity (the value of the house minus the mortgage balance) and mandatory pensions. We can decompose illiquid savings into the mortgage balance, M_t, and a residual, $\tilde{S}_t^N$: $S_t^N = \tilde{S}_t^N - M_t$. The mortgage interest rate net of taxes is given by R_t^M. Residual $\tilde{S}_t^N$ is largely beyond a household's control in our setting. We assume that changes in $\tilde{S}_t^N$ do not differ systematically between those buying before and after the reform.[11]

We are interested in changes in the stock of total savings: $\Delta S_t = \Delta S_t^L + \Delta S_t^N$. In the empirical analysis, we compare households buying right before and after the reform, the latter denoted with a prime:

$$\begin{aligned} \Delta S_t' - \Delta S_t &= \left(\Delta S_t^{L'} - \Delta S_t^L\right) + \left(\Delta S_t^{N'} - \Delta S_t^N\right) \\ &\approx \left(\Delta S_t^{L'} - \Delta S_t^L\right) - \left(\Delta M_t' - \Delta M_t\right) \end{aligned} \tag{10}$$

where $\left(\Delta \tilde{S}_t^{N\prime} - \Delta \tilde{S}_t^N\right) \approx 0$. The Dutch administrative data provide detailed and complete information about the stock of financial assets, voluntary pension contributions, non-mortgage liabilities, and the mortgage balance from which we can precisely calculate ΔS_t^L and ΔM_t. We calculate the MWA as

$$MWA \equiv 1 + \frac{\Delta S_t^{L'} - \Delta S_t^L}{\Delta M_t - \Delta M_t'} = \frac{\Delta S_t' - \Delta S_t}{\Delta M_t - \Delta M_t'} \tag{11}$$

which captures the degree to which households adjust their liquid and total savings in response to increased amortization.

II.C. Empirical design

As a first step, we compare economic outcomes for cohorts of FTHBs who bought around the reform by month of closing:

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[10] We define "liquid savings" as savings that are built from discretionary contributions that can be adjusted at a relatively low cost (unlike home equity or mandatory pension contributions). Voluntary pension savings cannot be withdrawn before retirement, but contributions are fully under the household's control.

[11] Mandatory pension contributions (as a proportion of income) are the same for everyone in the same sector or (large) corporation. Keeping hours worked constant, we do not expect this to vary systematically between those buying before or after the reform. House price appreciation is primarily driven by market movements. Absent geographical clustering, this will not lead to systematic differences. Appreciation might also differ due to additions. In Table 3, Column 6, we verify that house price appreciation (calculated from tax appraisals which are updated annually) is the same for those buying before or after the reform.

$$\Delta Y_{Jan-Dec\ 2015,i} = \sum \beta_{C}^{\Delta Y} \times 1_{C,i} + \eta_i \tag{12}$$

where ΔY is either ΔM (mortgage repayment), ΔS^L (change in liquid savings), or ΔS (change in total savings or wealth accumulation), all measured between January and December 2015. In each regression, the only independent variable is the cohort $1_{C,i}$: the month a household closed on their house. The omitted cohort is February 2013. The reform was binding for those buying (going under contract) starting January 1st, 2013. Given that the closing period typically takes at least two months, the reform started to affect households closing from March 2013 onwards. Applying equation (11), we can approximate the MWA as

$$\widehat{MWA} = \frac{\beta_{Post}^{\Delta S} - \beta_{Pre}^{\Delta S}}{\beta_{Post}^{\Delta M} - \beta_{Pre}^{\Delta M}} \tag{13}$$

We then estimate the MWA more formally using an intent-to-treat design, with the closing date as an instrument for mortgage repayment. In particular, we estimate the following first stage:

$$\Delta M_{Jan-Dec\ 2015,i} = \delta\ 1_{Post,i} + \lambda_r + X_i'\beta + \eta_i \tag{14}$$

where $1_{Post,i}$ is a dummy variable equal to 1 if a household i closed on their house from March 2013 onwards, λ_r are location fixed effects, and X_i are household controls in the years prior to home purchase (e.g. 2010 household gross income). If the reform increased mortgage repayment, we would expect δ to be positive and highly statistically significant. The second stage estimates the effect of the predicted mortgage amortization from equation (14) on total savings (we run this with two-stage least squares to obtain the correct standard errors):

$$\Delta S_{Jan-Dec\ 2015,i} = \gamma \widehat{\Delta M}_{Jan-Dec\ 2015,i} + \lambda_r + X_i'\beta + u_i \tag{15}$$

with $\widehat{MWA} = \gamma$.

One concern is that the timing of closing may be correlated with households' preferred wealth accumulation, which would bias our estimates. To address this selection issue, we restrict the sample to FTHBs who had a "life-event" during 2012-2013 (changes in family structure, such as the birth of a child, death of a family member, divorce, child moving out, etc.). For this sub-group, the timing of the first home purchase is likely driven by the life-event rather than by a strategic response to the reform. Life-events are a strong predictor of closings and we use the timing of the life-event instead of the actual closing to determine whether a household is treated or not. We re-estimate equation (12) where cohort $1_{C,i}$ is now the quarter in which a household had a life-event.[12] Further, we run the 2SLS specification from equations (14) and (15) with the month of the life-event as an instrument for mortgage repayment.

[12] We use the quarter rather than the month to preserve statistical power.

Another concern is that households buying before or after the reform are on different saving trajectories. We investigate differential pre-trends in a difference-in-difference framework. In particular, we estimate the following regressions:

$$Y_{t,i} = \sum_t \beta_t^Y \times \tau_t \times 1_{Post,i} + \sum_t \delta_t^Y \times \tau_t + \eta_i \quad (16)$$

where $Y_{t,i}$ is either $P_i - M_{t,i}$ (the level of home equity), $S_{t,i}^L$ (the level of liquid savings), or $S_{t,i}$ (the level of total savings or net-worth) and subscript $t \in \{2007, 2017\}$ is the calendar year. The omitted year is 2012 and τ_t are year fixed effects. We include FTHBs buying right around the reform (Jan-Feb 2013 vs March-April 2013) and $1_{Post,i}$ is a dummy variable equal to 1 if a household i closed on their house from March 1st, 2013 onwards. Coefficients β_t^Y capture how differences between FTHBs purchasing just before or after the reform evolve over time. When calculating differences in home equity we assume that there is no differential house price appreciation for the two groups (we verify this assumption in Table 3, Column 6). If coefficients β_t^Y are zero before 2012 and significantly positive afterwards, this would support the causal interpretation of our estimates.

II.D. Decomposing wealth accumulation

Additional wealth accumulation needs to be paid for. We can rewrite the IBC in equation (9) as

$$\Delta S_t = Y_t - C_t + r_t^L S_{t-1}^L - r_t^M M_{t-1} \quad (17)$$

with $r = R - 1$. Comparing households buying right before and after the reform, we have that:

$$\Delta S_t' - \Delta S_t = \underbrace{(Y_t' - Y_t)}_{a} + \underbrace{\left(r_t^{L'} S_{t-1}^{L'} - r_t^L S_{t-1}^L\right)}_{b} - \underbrace{\left(r_t^{M'} M_{t-1}' - r_t^M M_{t-1}\right)}_{c} + \underbrace{(C_t' - C_t)}_{d} \quad (18)$$

Differences in the change of the stock of total savings can come from differences in (a) after-tax labor income, (b) returns on liquid savings, (c) mortgage interest payments, and (d) consumption.

We have complete administrative data on (a) labor income, so we can precisely observe $Y_t' - Y_t$. In the empirical analysis, we estimate equations (12) and (16) for hours worked to analyze households' labor market response to the reform, and equations (14) and (15) for gross labor income to quantify how much of the additional wealth accumulation comes from working more.

We can decompose (b) into differences in returns and differences in the stock of liquid savings:

$$r_t^{L'} S_{t-1}^{L'} - r_t^L S_{t-1}^L = \left(r_t^{L'} - r_t^L\right) S_{t-1}^L + r_t^{L'} \left(S_{t-1}^{L'} - S_{t-1}^L\right) \quad (19)$$

The first term, $(r_t^{L'} - r_t^L) S_{t-1}^L$, will differ if those buying before or after have different portfolios and investment returns. On average, 8% of households own any stocks and bonds as part of their liquid savings. There is no information on capital gains, but we can approximate this by looking at the change in the value of stocks and bonds from year to year. Further, we observe the total interest and dividends they earn. We check whether these differ between households buying before or after the reform. If that is not the case, it is unlikely that actual capital gains will differ systematically. The second term, $r_t^{L\prime}(S_{t-1}^{L\prime} - S_{t-1}^{L})$, will differ if households change their liquid savings in response to the reform, or had different initial levels. We investigate both in the data.

Similarly, we can decompose (c) into differences in mortgage interest rates and differences in the mortgage balance:

$$r_t^{M\prime} M_{t-1}^{\prime} - r_t^{M} M_{t-1} = \left(r_t^{M\prime} - r_t^{M}\right) M_{t-1} + r_t^{M\prime}(M_{t-1}^{\prime} - M_{t-1}) \tag{20}$$

The first term, $\left(r_t^{M\prime} - r_t^{M}\right) M_{t-1}$, will be close to zero for those buying around the reform as they pay virtually the same mortgage interest rate (Section I.A of the Online Appendix has details). The second term, $r_t^{M\prime}(M_{t-1}^{\prime} - M_{t-1})$ will differ if households change their mortgage amount in response to the reform, which we check in the data. Further, households buying after the reform are forced to amortize more and will see a larger decline in their $M_{t-1}^{\prime}$. This effect starts out small, but grows over time.

As for (d), consumption, we only observe car purchases directly. However, assuming that (b) is close to zero (to be verified empirically), differences in consumption can be imputed and are given by:

$$C_t^{\prime} - C_t \approx (\Delta S_t^{\prime} - \Delta S_t) - (Y_t^{\prime} - Y_t) + r_t^{M\prime}(M_{t-1}^{\prime} - M_{t-1}) \tag{21}$$

which we can calculate in the data.

III. Data description

III.A. Data and Sample

Our analysis takes advantage of highly detailed administrative datasets collected by the Dutch Statistics Office (CBS), featuring individual-level information on every person living in the Netherlands from 2006 to 2017. Housing transactions come from the deeds registry, with several months between the signing of the purchases contract and the deed transfer. We obtain household size and composition from the household spell registry. This information is accurate and up-to-date and allows us to pin down the timing of "life-events", which we define as instances where the number of household members changes. Further social and demographic characteristics come from the civil register (administered by local municipalities). Household balance sheet information, used to calculate the Dutch wealth tax, comes from the Dutch tax authorities and is verified by financial institutions. Household income statements also come from the tax authorities and include voluntary pension contributions as well as income from interest and dividends. Information on non-mortgage liabilities comes from the national credit registry. Data on house appreciation comes from municipal records which update appraised house values each year, based on both changing market conditions and additions. Finally, hours worked come from the social security administration (2010-2016), car registrations from the road traffic administration, and total pension entitlements from the responsible pension funds (for 2016 and 2017). Section II.A of the Online Appendix has a detailed overview of the exact databases we use.

For the empirical analysis, we apply two data filters. A household's wealth or mortgage balance cannot change by more than €100k and its mortgage balance not by more than 30% in a given year. Further, we winsorize all variables at the 1% level. This ensures that outliers do not drive our results. In Online Appendix III.C we show that results are robust to dropping the two filters.

We focus our analysis on all 94,094 people in the Netherlands who bought their first home financed with any kind of mortgage between April 2012 and December 2013. Taking February 2013 as the omitted month, this means we look at 10 months before and after the reform. We then examine their outcomes in the years around the house purchase. Table 1, Panel A provides simple summary statistics on these households. In line with the overall population of homeowners, mortgage liabilities are by far the largest component of average household debt. For our group the median mortgage balance is €183k, with median total debt at €189k. The median LTV is about 105%, which reflect the strict recourse laws enforced in the Netherlands. As first-time home buyers, households in our sample tend to be fairly young, with a median age of 36 years for the oldest household member, and have a fairly high income (which is why they are able to buy a house) with a median household gross income in 2014 of about €54k. For the figures applying the cohort-based analysis from equation (12), we use this full sample; for the formal regression analysis in equations (14) and (15) and the diff-in-diff analysis in equation (16), we use a restricted set of cohorts.

III.B. Savings

1. Mortgage debt repayment (amortization)

Outstanding mortgage liabilities are based on tax records filed by households and verified by banks. For households buying after the reform, we have complete data on the yearly mortgage balance from which we calculate repayment. For households buying before, we miss detailed information about the linked savings accounts discussed in Section I.A. As we noted before, most households had a mortgage that was 50% IO and 50% amortizing, typically through a linked savings account. Therefore, if we observe a mortgage without a year-over-year change in its balance, we assume that the mortgage is 50% amortizing through a linked account. We impute the amortization the household effectively made, assuming an annuity mortgage with an interest rate of 4.50% (the average rate in 2012-3).[13] If we observe a mortgage with a year-over-change in its principal, we assume this mortgage did not have a linked savings account and calculate mortgage repayment directly from the change in balance.

[13] In our robustness checks, we show that results are virtually the same when we change these assumptions. As we noted previously, households were unable to access linked savings before the end of the mortgage.

2. Liquid savings

Liquid savings are all bank deposits (checking and savings accounts) and financial instruments like stocks and bonds. We observe the market value of these savings at year-end (also based on bank-verified administrative tax records). Table 1, Panel A shows that the median household has €8.5k in liquid savings in 2014, with the 25th percentile at €2.8k. At the median, liquid savings increase by €0.3k (Δ*liquid savings*). This remains the same when we include voluntary pension contributions (Δ*liquid savings*$^+$), though the 75th percentile moves from 3.3k to 3.4k. When we take changes in non-mortgage debt into account (Δ*net-liquid savings*$^+$), median savings increase by €0.6k. This last measure corresponds to the change in liquid savings in the model. Voluntary pension contributions come from administrative tax records (we only observe the flow, not the stock). Non-mortgage liabilities come from the national credit registry.

For our sample of FTHBs, the reform means an additional amortization of around €2k per year.[14] The majority of households would have a sufficient stock of liquid savings to cover the additional amortization without having to cut consumption or increase working hours. However, for households at the lower end of the savings distribution, this is not the case. Therefore, we also run the analysis focusing on households with more substantial savings. One group we consider consists of households who have at least €10,000 in liquid assets as of the end of 2015, or who accumulate at least €3,000 in additional liquid assets over 2015. Summary statistics are in Table 1, Panel B. This group has median liquid assets of around €21k, making the additional amortization after the reform a fairly small portion of their liquid savings. At the median (and mean), this group also adds an additional €3k in liquid savings in a given year, which suggests they are not at a corner solution where they do not want to save more. Also, they are able to offset the additional amortization without having to cut the level of their current savings.

For additional analyses, we also consider the sample of all home buyers, including those who already own a home. For completeness, summary statistics for this group are in Table 1, Panel C. For this group, median liquid savings are €12k, and, at the median, liquid assets increase by €0.2k.

Annual changes in liquid savings can be substantial. The within household year-over-year standard deviation in liquid savings is about €14k between 2006 and 2017 (not reported). In 2014 this was about €9k. In Online Appendix II.B, we show that this variation appears to be driven by changes in households' economic conditions, confirming that the tax authorities provide real time information about liquid savings. Kárpáti (2022) confirms, within the same database, that our measure of savings, *liquid savings*, falls when individuals suddenly learn they have lower life expectancy.

[14] This is approximately what we would expect in our sample for the average difference in amortization between a 50% and 100% amortizing mortgage in the first few years of the mortgage at an interest rate of 4.5%

3. Total savings (wealth accumulation)

When considering changes in total savings (wealth accumulation), we consider changes in the mortgage balance (amortization) and changes in liquid savings including voluntary pension contributions, net of non-mortgage liabilities (*net-liquid savings*$^+$). For our baseline estimates we ignore changes in house prices, employer-sponsored pension entitlements, and the discounted value of future labor income. The first two are largely beyond the households' control and are unlikely to differ systematically between households buying before and after the reform.[15] For additional analysis, we do include changes in appraised house values, which are updated annually to reflect market price movements and additions. We cannot include employer-sponsored pension entitlements in the analysis, as this information is only available in 2016 and 2017. We analyze changes in labor income separately.

III.C. Income and consumption

1. Hours Worked and Labor Income

If households do not fully adjust the additional amortization by decreasing other forms of savings or consumption, they need a way to pay for it. One option is to increase labor income. Through the social security administration, we can perfectly track the number of hours worked for all household members, as well as labor income. This allows us to differentiate between increased labor input and higher wages.

2. Return on savings

Another way by which households could pay for additional amortization is to pursue higher returns on their savings. The Dutch tax authorities provide information on income from interest and dividends. We do not have information about capital gains; we only observe the value of financial assets at year-end. We can use differences in interest and dividends received to infer whether households buying before or after the reform systematically experienced different capital gains.

3. Consumption and car expenditures

Finally, households can pay for the additional amortization by consuming less. As is typical with administrative datasets, we do not observe this directly. Under the assumption that households buying around the reform have a similar stock and flow of liquid savings, and have similar returns, we can use equation (21) to back out changes in overall consumption. We can also use car expenditures as proxy. We cross-reference Dutch car registrations with household income data to filter out households who got a company lease (which is not uncommon in the Netherlands). We infer car values from car characteristics.

[15] Further, since house prices are the discounted present value of future rental rates (the cost of living somewhere), house price changes may not reflect changes in wealth.

IV. Results

IV.A. Mortgage amortization and wealth accumulation

Figure 2, Panel A presents estimates of equation (12). We present the amortization and flow of savings in 2015 for first time home buyers (FTHBs) by purchase-month-cohort. The reform affects households going under contract starting January 1st, 2013. It takes at least two months to close. Households in the March 2013 cohort are the first to be affected by the reform, followed by an increasing fraction of households in later cohorts. We restrict the figure to closings from April 2012 to December 2013, with ten pre- and post-reform cohorts and February 2013 the omitted cohort. We do not include any controls.

The figure shows that amortization is largely the same for cohorts closing between April 2012 and February 2013. Consistent with our expectations, average mortgage repayment increases with about €2k in 2015 for later cohorts. This is equivalent to about 25% of the median stock of liquid savings (cf. Table 1). The particular shape of the line, flat with a sudden increase in amortization for cohorts closing after February 2013, suggests that cohort-trends are not driving our results. The figure also shows that households buying before or after the reform largely have the same flow of liquid savings (*net-liquid savings+*). The line is largely flat and does not have a sudden change for cohorts closing after February 2013. As a result, households buying after the reform accumulate around €2k more wealth over 2015. Applying equation (13), this means that the MWA is close to one.

Table 2 formalizes this analysis following the 2SLS procedure outlined in equations (14) and (15). This table includes the subset of ~42k FTHBs who closed between October 2012 and September 2013, excluding the March and April 2013 cohorts for whom it is ambiguous whether they went under contract before or after the reform. Columns (1) and (2) confirm that cohorts buying after the reform saw additional amortization and wealth accumulation of around €2k. The IV estimate in column (3) estimates the MWA close to one, with the 95% confidence interval between 0.88 and 1.10. Column (4) shows that the difference in the flow of *net-liquid savings*$^+$ is close to zero and statistically insignificant. Columns (5) and (6) look at wealth accumulation between the end of 2013 and 2017 (as far as our data runs).[16] Column (5) shows additional wealth accumulation of around €8k over these four years. Column (6) confirms that the MWA is close to one when we consider this longer period. Finally, Column (7) considers households buying close to the reform (closing in January to April 2013). This sub-sample is not affected by end-of-year effects coming from the moment of closing and, more generally provides the best like-with-like comparison available. Again, the MWA estimate is large and close to one.

[16] We omit the post-reform cohorts of August and September 2013 for this analysis. These cohorts buy their home sufficiently close to year-end such that the level of liquid savings at the end of 2013 is temporarily depressed, which artificially increases the change in liquid savings between 2013 and 2017, leading to a bias in the MWA.

In sum, the full sample MWA estimate is close to one. Circling back to Lemmas 2 and 3, this is consistent with most households acting as if they face a low cost of adjusting short term consumption relative to adjusting their liquid savings and/or not fully realizing that additional amortization increases long term wealth. In Section IV.D, we construct a number of proxies to see if the MWA varies in these two dimensions to evaluate whether these are indeed the reasons that the full sample MWA is close to one.

Section III of the Online Appendix further evaluates the robustness of these results. We include appraised house price appreciation in our measure of wealth accumulation and also estimate the MWA using the levels (rather than changes) of home equity and the mortgage balance. Further, we estimate the MWA under different amortization assumptions for mortgages extended before the reform, and alternative sample filters. The MWA estimate remains robustly close to one.

IV.B. Selection concerns and pre-trends

They key identifying assumption of our analysis is that households buying (just) before or after the reform are otherwise comparable. One potential key concern is that households who are most averse to accumulating savings might strategically self-select into buying before the reform. If so, the flow of liquid savings for those buying before the reform would have been artificially low, leading to an upward bias in the MWA estimate. The late passage of the reform in November 2012 suggests that strategic timing of home purchase would have been difficult. This is confirmed by Figure 2, Panel B, which shows that there is no bunching of closings right before the reform.[17]

To further address selection concerns, we compare those buying right before or after the reform in a host of observable dimensions, including house values, pre-reform savings, income and income growth. Results are in Section IV of the Online Appendix. We find no substantial differences between those buying right before or after, implying selection is not a major concern. Section I.A of the Online Appendix shows that mortgage interest rates and LTVs do not differ between the two groups, suggesting that banks also saw no substantial change in the pool of home buyers. The only difference we find is in post-reform income growth. We explore this separately in the next section and show that this is entirely driven by an increase in hours worked, likely caused by the reform itself.

Next, we instrument the timing of home purchase with "life-events", instances where the number of household members changes. These events strongly predict the timing of home purchase, but are unlikely to be timed strategically in response to the reform. We restrict the sample to households who had a life-

[17] The spike in transactions in June of 2012 is driven by concerns about an increase in the transaction tax for new house purchases (which never materialized). This stands in stark contrast to the lack of any spike or dip around the 2013 reform, suggesting households do sometimes respond to changes in mortgage rules, but clearly did not appear to do so for this reform.

event in 2012 or 2013 and also purchased a house for the first time. Figure 3 replicates the purchase-cohort analysis of equation (12), but now we define the cohort based on the timing of the life-event instead of the actual purchase date. Because of a smaller sample size, and limited power, we present results using quarter rather than month cohorts. Results are comparable to Figure 2, Panel A. Amortization and wealth accumulation are the same for cohorts having a life-event before the reform, but are economically and statistically larger for cohorts having a later life-event. Table 3 provides the IV estimates from equations (14) and (15). Columns (1) through (3) first show that those having a life-event before or after the reform have similar observable characteristics in 2010, confirming that the timing of life-events is not correlated with initial income or savings. Columns (4) and (5) confirm that households with a life-event after the reform have both higher amortization and wealth accumulation in 2015, while Column (6) confirms that they do not see differential house price changes. They key IV estimate is in Column (7), which shows that the estimated MWA is 0.888 and includes one in its 95% confidence interval. This suggests that our baseline estimate for the MWA in Table 2 is unlikely to be upward biased due to selection.

One potential concern is that the municipalities only register changes in the number of household members after home purchase. Due to the strict administrative rules this is unlikely, but we still explore this concern just in case. Column (8) limits the sample to life-events taking place in a different month than home purchase and still finds a MWA of 0.957.

Another concern is that selection could affect the extensive margin, with fewer people being able to afford a home after the reform. Figure 2, Panel B shows that the level of home transactions is stable around the reform. Further, the fact the households buying before or after look similar on observables, suggests that (substantial) changes on the extensive margin are unlikely. Column (9) tests whether households having a life-event before or after the reform differ in the likelihood of owning a home in 2016. There is no difference either economically or statistically. Section IV of Online Appendix performs this analysis cohort-by-cohort and shows that there is also no difference for households buying right around the reform.

Another way to test our key identification assumption is by looking at whether households buying before or after the reform are on the same initial saving trajectories. Figure 4 provides estimates of the diff-in-diff equation (16). We focus on households buying right around the reform (Jan-Feb 2013 vs March-April 2013) and estimate differences between those buying before and after. The figure shows that, before the reform, there are no systematic differences in liquid savings and net-wealth (liquid savings minus non-mortgage liabilities). After the reform, liquid assets remain the same for both groups, but those buying after progressively obtain more home equity due to increased amortization. This leads to a differential increase in net-wealth (home equity and liquid savings minus non-mortgage liabilities). The shape of the wealth accumulation line, flat before the reform and suddenly increasing after, supports the causal interpretation of our MWA estimate.

IV.C. Labor, investment and consumption response

Our results so far indicate that households buying after the reform did not change their liquid savings. The intertemporal budget constraint from equation (17) indicates that the additional amortization must be paid for by higher labor income, a higher net income on savings, or a reduction in consumption.

1. Labor response

Figure 5 presents the purchase-cohort analysis from equation (12) for hours worked. On the left vertical axis we plot the additional amortization amount (the same as in Figure 2, Panel A). On the right axis we plot the change in hours worked between 2012 and 2015. The figure shows that the two variables move in lock-step. There are no differences among households buying before the reform, but those buying after work around 100 hours more per year. The particular shape of the line, flat with a sudden increase in hours worked for cohorts closing after February 2013, suggests that cohort-trends are not driving our results.

Table 4, Panel A looks at this more formally, again restricting the analysis to households who closed between October 2012 and September 2013, excluding the March and April 2013 cohorts. Columns (1) and (2) show that those closing after the reform started to work around 80 hours more per year, corresponding to around a 5% increase. As a result, they earn more. Columns (3) and (4) verify that (an increase in) hours worked are associated with higher income, while column (5) shows that those buying after the reform indeed earn more. Column (6) indicates that this is largely driven by an increase in hours worked; the remaining increase in income is economically small and statistically insignificant. This suggests that those buying after the reform do not see a differential increase in wages per hour. Section V.A in the Online Appendix documents there is also an effect on the number of household members that work. However, Column (7) shows that this only explains a small part of the increase in income. Overall, our results suggest that household members who do not work full time increase their hours worked if they buy after the reform. This fits with the culture in Netherlands, where one partner often works part-time.

Table 4, Panel B evaluates how much of the increased amortization is covered by higher labor income. We estimate this using the IV approach from equations (14) and (15). We use the relevant marginal tax rate (42%) to map our gross-income estimates into net-income. Estimates are in columns (1) and (2), where the latter includes additional controls and municipal fixed effects. Around $0.65 \times (1 - 0.42) = 38\%$ of the increased amortization is paid for by higher labor income. Columns (3) and (4) verify that this is driven by higher income in 2015, not lower income in 2012. If we take the estimate from column (4), $0.59 \times (1 - 0.42) = 34\%$ of the increased amortization is paid for by higher income. Controls are not available for all households, leading to a drop in observations from column (1) to (2). For good measure, we estimate the MWA for this sample in column (5), which is again close to one.

There is a concern that households who expect to increase their hours worked are more likely to buy after the reform. This could bias our MWA estimates if those households are on a different savings trajectory. To alleviate this concern, columns (6) and (7) focus on households where all adult members are already working full time. As expected, column (6) indicates that these households do not see a significant increase in their income between 2012 and 2015. Nevertheless, column (7) shows that they still have an MWA close to one, suggesting such selection concerns are unlikely to bias our estimates.

To alleviate any remaining selection concerns, we use a difference-in-difference analysis to evaluate pre-trends. We estimate equation (16) for households buying right before or after the reform. Compared to Figure 4, we extend the after-reform group to households buying up to December 2013. The change in hours worked is noisy (with many zeros) and we need more observations to increase statistical power.[18] Estimates are in Figure 6. Unlike Figure 4, data is only available for 2010-2016. The figure shows that, before the reform, there are no systematic differences in hours worked, suggesting the two groups are not on differential trends. After 2012, differences start to emerge. The adjustment is gradual, likely because not all employers can accommodate more hours instantaneously. Section I of the Online Appendix shows that Dutch unemployment rates were falling from 2014 onwards, so it is possible that this could have played some role in the observed labor supply response to this shock. The effect levels off in 2015.

In sum, the evidence indicates that households buying after the reform differentially increased their hours worked. Additional earnings cover about 38% of the increased amortization. There is no evidence that households buying after the reform were on different trends and expected to increase their hours worked. When we restrict the sample to households whose adult members worked full time before, we find a similar MWA close to one, suggesting that any (possible) selection does not bias our estimates.

2. Income from savings

Next, we explore whether households buying after the reform obtained higher income from their savings. We first consider liquid savings. Following equation (19), income may differ due to higher returns or a higher stock of savings. In Section V.B of the Online Appendix, we evaluate differences in returns. Households with higher amortization may have put more of their savings into assets with higher expected returns, such as stocks. Only about 8% of our sample invested in stocks and bonds. This does not differ between those buying before or after the reform. The same holds for the amount invested. We observe dividends and interest paid, and we approximate capital gains by the change in value of stocks and bonds from year-to-year. There are differences in neither, suggesting that households did not experience differential returns on their portfolios. Further, the results in Section IV of the Online Appendix (Figure

[18] Since we take hours worked in 2012 as baseline, we cannot extend the pre-group to households buying in 2012.

A.5 in particular) indicate that there are also no differences in the accumulation of liquid savings, either before or after the reform. Together, this suggests that income from liquid savings is unlikely to differ.

In terms of illiquid savings, the discussion in Section II.B and equation (20) indicate that differences will arise in the form of lower interest payments due to higher amortization. Holding the interest rate fixed, an amortizing mortgage has constant payments. As the amortization amount increases over time, interest payments decline. Part of the additional amortization faced by households buying after the reform is therefore covered by lower interest payments. This effect grows over time. Section V.B of the Online Appendix indicates that this covers around 8% of the additional amortization in 2015.

3. Consumption

Given the previous discussion, we can back out households' consumption response using equation (21). As of 2015, households cover around 38% of the additional amortization with higher labor income, and 8% with lower interest payments. The remaining 54% must have come from a reduction in consumption. There is no direct data on consumption, though we do observe car purchases. In Section V.C of the Online Appendix, we show that households buying after reform start buying cheaper cars. This is consistent with households reducing overall consumption and covers 11% of the additional amortization.

IV.D. Heterogeneity

In this subsection, we investigate heterogeneity in our baseline estimates. First, we evaluate whether our result that the MWA is close to one is driven by households being at a savings corners. Second, we test Lemmas 2 and 3 from Section II.A. to better understand the intuition behind our findings. Finally, we discuss what models of economic behavior might be consistent with our empirical results.

1. Demand for long term savings

If households are financially constrained, or have no interest in accumulating savings for the long term, they will not have any liquid savings they can reduce to compensate for the additional amortization required after the reform. As a result, their MWA will be mechanically close to one. To evaluate whether this is driving our results, we identify groups of households who are likely to be unconstrained, or who appear interested in saving for the long run, and test whether these groups have a lower MWA.

Table 5, Panel A considers households who appear unconstrained. Columns (1) and (2) look at households whose 2014 LTVs and LTI at origination were relatively low and far away from the regulatory constraint. Given that these households did not borrow as much as they could suggests that they are not constrained. Nevertheless, we find large $MWAs$ for these groups that are close to one. This also suggests that Dutch households having high LTVs cannot explain a high MWA. Column (3) considers households who, in 2015, had at least a stock of €10k in liquid savings and a flow of €3k, suggesting they had enough money to pay for the additional amortization. Again, we find a MWA close to one. Next, we consider households who had a stock of liquid assets of at least of €10k in 2011. Columns (4) and (5) show that these households had substantial liquid savings in 2011 and 2015. Nevertheless, as column (6) indicates, their MWA is close to one. Finally, column (7) considers households investing in stocks and bonds who, by revealed preference, likely have sufficient liquid savings to cover short term spending goals and invest some of their liquid savings for the long run. This group also has a MWA close to one, providing further evidence that our MWA estimate is not mechanically close to one.

Table 5, Panel B shifts the focus to households with an apparent interest in accumulating long term savings. Anecdotally, Dutch households appear to have strong demand for long term savings. Columns (1) and (2) consider households who, at some point between 2011 and 2017, made voluntary pension contributions. These household clearly express and interest in accumulating long term savings, and likely have the financial means to compensate some of the additional amortization. Nevertheless, we find a large MWA for this group that is close to one. Columns (3) through (6) consider households with limited predicted pension payouts, either below €500 or €1000 per month, with columns (5) and (6) restricted to households who are actively contributing to an employer-based mandatory plan. These households have a pension gap and we would expect that at least some of their liquid savings are intended for the long run. However, this group also has MWA close to one.

Finally, we look at a sample of households who move and resell their house before the end of 2016 ("movers"). Constrained households might want to undo the additional amortization through home equity withdrawal. Though possible (cf. Section I.A), this might be costly. Such costs are lower during a move when a household has to refinance anyway. In our sample, we observe around 1.8k households who resell their house between 2013 and 2016. Table 6 explores this sub-sample in greater detail. Columns (1) and (2) show that those buying their first house after the reform still have a mortgage balance that is about 6% lower at the end of 2017 than those buying before. Column (3) confirms that home equity withdrawal when buying the second home is indeed small and insignificant. Further, columns (4) and (5) shows that the level of liquid assets is not higher at the end of 2017. Finally, column (6) shows that the MWA (as measured in 2016) is not lower for movers.

In sum, our high MWA estimate is likely not driven by households at a savings corner. Households who appear unconstrained, or interested in accumulating long-run savings, have a MWA close to one. Further, households do not appear to extract home equity even when costs are low.

2. Cost to adjusting short term consumption or liquid savings and sophistication

What then, is the economic explanation for a full-sample MWA close to one? In the stylized model of Section II.A., the MWA depends on the concavities of households' utility from consumption and liquid savings, as well as financial sophistication. In particular, the MWA can be high if it is less costly for households to adjust short term consumption than to adjust liquid savings, and if households do not fully internalize that amortization increases future wealth. To evaluate the validity of these hypotheses, we construct a number of proxies to test if the MWA varies in these dimensions.

Households' costs to adjust short term consumption or liquid savings are not directly observable. We construct two proxies for the cost of adjusting short term consumption that are revealed by economic behavior. There is no apparent proxy for the cost of adjusting liquid savings, but our tests are still informative as long as the two costs are not (perfectly) correlated. First, we look at households who put little money down when buying a house. These are households who reveal a preference to consume in the short term rather than lock-in more housing consumption in the future. Second, we look at households who pay a low mortgage interest rate. Households can fix the interest rate for different lengths of time. The shorter the period, the lower the rate. Households with lower rates reveal a preference to consume in the short term rather than lock in more consumption certainty in the future.[19]

We approximate households' (financial) sophistication by looking at the highest level of education someone in the household followed, which we observe for everybody in the sample. We define individuals to be "highly" educated if they followed a theoretical rather than a practical degree (information about the exact degree is unavailable). If sophisticated households are more aware of amortization's positive effect on long-term wealth, we would expect that the MWA is lower for this group.

A number of caveats are in order. First, because the MWA estimate for the entire sample is close to 1 (and a $MWA \gg 1$ seems unnatural), it is unlikely that there is a large group with $MWA < 1$. That means that we have limited statistical power to find heterogeneity in the MWA. Further, households with a high cost of adjusting short term consumption may have little liquid savings to begin with, meaning that their MWA cannot be too low, further limiting power to detect differences. Second, the cost of adjusting short term consumption is measured through observed behavior. We verify that this behavior was not affected by the reform itself to rule out selection concerns, but the underlying source of variation is not necessarily exogenous. Third, our proxies might pick up the cost of adjusting short term consumption and sophistication jointly. Households who put little money down maximize the interest rate deduction and the amount of cheap collateralized credit they can obtain. Those with a lower mortgage rate might have shopped around for better rates. Both could indicate higher sophistication. Further, households with a higher education face a steeper upward lifetime income profile and could be more averse to reduce short term consumption.[20]

[19] We do not observe the length of the fixed interest period, and we cannot look at this directly. Taking the full interest rate menu that mortgage borrowers faced in January 2013, fixed-interest period dummies, originator-product dummies and LTV buckets have a partial R^2 of 0.79, 0.11 and 0.10, respectively. Source: hypotheekrente.nl as of 01/05/2013.

[20] According to cross-sectional data from CBS for 2016, among individuals with a high education level, those aged 45-49 have a 82% higher income than those aged 20-24. For individuals with a practical degree the difference is only 34%. Source: https://hetgeldcollege.nl/gemiddelde-salaris-per-leeftijd/

Results are in Table 7. We first focus on the group of people who put down between 0 and 2% on their home. Columns (1) estimates the MWA to be 0.699 for this group when looking at 2015 amortization and wealth building. Column (2) shows that the difference with the rest of the sample is statistically significant, while column (3) shows that the effect is quantitatively similar for the full period between 2013 and 2017. Column (4) verifies that the likelihood that households put down between 0 and 2% is the same before and after the reform, alleviating selection concerns. Column (5) restricts the sample to people who saw a large enough increase in their house price that they had substantial home equity in the years after purchase. This does not materially change the MWA, indicating it is the choice of how much money to put down, not the level of home equity *per se*, that drives this source of heterogeneity.

Next, we turn to differences in the mortgage rate and the level of education. Column (6) shows that people who paid a mortgage interest rate below the 25th percentile in a given cohort month had an MWA of 0.799. The difference with the rest of the sample is statistically significant. Column (7) differentiates people by education. Borrowers with a higher level of education have an MWA of 0.900, which is statistically significantly different from the rest of the sample. Column (8) verifies that the likelihood of being highly educated is the same before and after the reform. Section VI in the Online Appendix presents a specification curve where we experiment with different cut-offs for how much money people put down, and different periods over which we measure amortization and wealth building. Further, we combine different sample selection criteria. Our results are robust to definition and specification choice.

In sum, though there are a number of caveats, empirical findings are consistent with our stylized model. Households with a higher cost to adjust short term consumption and a higher education level seem to have a MWA less than one. This suggests that our full-sample estimate of an MWA close to one is driven by households' willingness to reduce short term consumption in order to keep liquid savings at some target, and households not fully internalizing that higher amortization leads to higher long-term wealth.

3. Link to Specific Models of Economic Behavior

In this subsection, we discuss a number of (behavioral) economic models that map into our stylized model and that could explain at least part of our empirical results.

In our simple model, the MWA is high when adjusting consumption leads to a smaller decline in marginal utility than adjusting liquid savings. This could be rationally driven by a liquidity wedge between liquid savings and home equity. States of the world in which households want to tap into home equity (e.g. after a job loss), could be situations where house prices and income have fallen such that LTV and PTI constraint bind and additional mortgage credit is expensive or inaccessible (DeFusco et al. 2020a). Further, extracting home equity involves non-trivial cost and time (Campbell et al. 2021). As a result, households might be reluctant to adjust their liquid savings and rather incur the costs of adjusting short-run consumption. However, while this predicts a MWA bigger than zero, it does not predict it to be close to one. Further, this explanation is contradicted by a number of other results. First, households appear willing to cut consumption today in order to avoid any increased risk of doing so in the future, even though they are able and willing to alter labor supply today and have chosen their current level of precautionary savings as a buffer against shocks. Second, Figure 2, Panel B provides no evidence of bunching before the reform. If households are concerned about the liquidity wedge, we would expect them to buy earlier. Finally, households with a larger stock of savings presumably intend some of that for long-term purposes (e.g. retirement) that are fungible with home equity. Still, these households still have a MWA close to one.

Adding "temptation" to this framework might go a long way in explaining our findings (e.g. Kovacs and Moran 2020; Kovacs et al. 2021; Attanasio et al. 2021). Suppose that one group of households will be tempted to consume their liquid savings at some point. Realizing their lack of commitment, they transform all savings designated for the long term into illiquid assets. Only savings meant for the short term remain liquid. Additional amortization means building up more illiquid savings. Since these are a poor substitute for liquid savings, this group might prefer to reduce current consumption and leisure rather than liquid savings (MWA close to one). Another group suffers less from temptation. They transform less of their savings into illiquid assets. In our data, these would be the type of households who put less money down at origination. Liquid assets are partly meant for the long term, making them a better substitute for additional amortization. Therefore, when forced to amortize more, this group might prefer to adjust liquid savings rather than consumption and leisure (MWA less than one). That said, a temptation model might also predict that households purchasing after the reform put down less money at home purchase as higher future amortization already locks in more long-term savings. This is not what we find in the data, though this might be driven by imperfect fungibility between down payments and amortization.

Another explanation is bounded rationality, which is supported by the finding that more sophisticated households have a lower MWA. Households might follow a simple saving heuristic, which, in many countries, are provided by official agencies. The Dutch NIBUD has offered an online tool since 2008 and, consistent with advice in other countries, their "optimal" level of liquid savings depends on household size, income and house value, but does not take amortization into account. Similarly, amortization could act as a "default" setting that nudges people to accumulate more wealth (Thaler and Sunstein 2009). Further, the Dutch reform may have changed households' beliefs about optimal savings, which would explain the lack of bunching in housing transactions before the reform. Alternatively, households may have separate mental accounts for mortgage repayment and liquid savings (Camanho and Fernandes 2018, Argyle et al. 2019). In the extreme, they might treat the former as bills rather than savings, although this appears inconsistent with a lack of bunching. They could also consider such savings as bequests only as soon as they are accumulated. Finally, households might overestimate their need for liquidity in the future, even if they appear unconstrained (D'Acunto et al. 2020, Olafsson and Pagel 2017, Aydin 2022). Though explanations based on bounded rationality can be made consistent with our findings, none of them have the inherent feature that households who reveal a higher cost of reducing short term consumption have a lower MWA.

In sum, the existing literature provides a number of possible explanations for finding a MWA close to one, though no single explanation seems be to be able to explain the full set of our empirical findings all at once. Most likely, our results driven by a combination of factors.

IV.E. Further extensions and robustness

1. Long run effects

Section VII of the Online Appendix uses the available evidence to evaluate how long the effects of additional amortization on wealth building might persist. We find an MWA close to one even if we look at later years for which there is available data (up to 2017), or if we look at older homebuyers, including those older than 50. This suggests that effects might be persistent. Next, we compare aggregate statistics between the Netherlands and the U.S. which suggest that mortgage repayment typically follows the amortization schedule, with little evidence for mortgage repayment from other sources or large home equity withdrawals. This is further suggestive evidence that increasing amortization requirements can have large effects on life-time wealth accumulation. Finally, the evidence from Table 6 shows that movers who resell their house do not extract home equity. This suggests that in the future, few households may undo the additional amortization when they move.

2. The state of the economy

The Dutch reform was passed right after a severe recession. This raises the question whether the state of the macro-economy matters for our MWA estimates. There is substantial geographic variation in the severity of the recession and subsequent recovery. Using this variation, Section I.B of the Online Appendix shows that the MWA does not depend on local economic conditions, in particular house price changes or unemployment. This suggests that the state of the economy is not of first order importance for the MWA.

3. Effects of the reform on mortgage interest deductibility (MID)

By increasing amortization, the reform reduced the net present value (NPV) of future MID and households' life-time wealth. Section VIII of the Online Appendix suggests that this effect is quantitatively small because most of the MID loss accrues further in the future. Further, the section provides additional tests suggesting this has no significant effect on our MWA estimates. In particular, we use a delta-in-delta estimation strategy, leveraging the fact that amortizing increases over time for an annuity mortgage. This gives us a MWA close to one as well. We also consider non-first time home buyers who were also (partially) affected by the reform depending on the remaining maturity of their mortgage. We show that responses are similar for those with a remaining maturity of more or less than 10 years, suggesting that the lower NPV of MID is not of first order importance for our MWA estimate.

V. Discussion: Positioning our Findings in the Literature

V.A. How does our MWA estimate fit in the existing empirical literature?

There is no direct equivalent of our MWA estimate in the literature. That said, equation (5) shows that the MWA is equivalent to the difference in frequently estimated marginal propensities to consume out of short and long-term income shocks (this is what amortization changes simultaneously in opposing directions). In our simplified setup, consumption includes spending as well as leisure, while existing work typically defines it as spending alone. Therefore, we need to compare the MWA to existing estimates of both the marginal propensities to consume (MPC) as well as earn (MPE) out of income shocks. For example, in response to a positive income shock, we would expect a household to spend more ($MPC > 0$) and work and therefore earn less ($MPE < 0$). We can rewrite equation (5) as

$$\begin{aligned} MWA &= MPCL_1^{ST} - MPCL_1^{LT} \\ &= \left(MPC_1^{ST} - MPE_1^{ST}\right) - \left(MPC_1^{LT} - MPE_1^{LT}\right) \end{aligned} \tag{22}$$

where $MPCL_1^{ST/LT}$ is the marginal propensity to enjoy *current* consumption (*C)* and leisure (L) out of short or long-term income shocks. Similarly, $MPC_1^{ST/LT}$ and $MPE_1^{ST/LT}$ are the marginal propensities to consume or earn out of short or long-term shocks, again in the *current* period. As such, the MWA can be interpreted as an estimate of the wedge between $MPCL$s out of short and long-term shocks.

There is a wide range of $MPCs$ and $MPEs$ out of short and long-term shocks in the literature that all appear highly context specific. This means that existing MPC and MPE estimates do not directly imply an accurate MWA estimate. Nonetheless, comparing results does give some sense of where our estimates are likely to fit in the existing literature and what that says about the amortization-specific context.

Even in a single study of spending out of tax rebates, MPC_1^{ST} estimates range from 0.5 to 0.9 (Parker et al. 2013). MPE_1^{ST} estimates can be high in absolute terms, but also quite variable depending on the context. For lotteries, the estimate lies around -0.11 (Imbens et al. 2001), while for lost social security payments, estimates range from -0.7 to -1.0 or even lower than that (Deshpande 2016). For housing specifically, Di Maggio et al. (2017) estimate an MPC_1^{ST} of 0.8 from increases in mortgage payments coming from interest rate resets in the U.S., while Zator (2020) finds an MPE_1^{ST} of about -0.35 for mortgage interest rate increases in Poland. These are of course an imperfect proxy for changes in amortization. For example, Di Maggio et al. (2017) also show that interest rate resets directly affect incentives to refinance in a fixed rate environment, leading to effects on spending not just caused by short-term income shocks.

That said, MPC and MPE estimates from recurring (as opposed to one-time) income shocks tend to be at the higher end and could certainly be consistent with a $MPCL_1^{ST}$ close to one.

There is also a substantial range in MPC_1^{LT} estimates, though they tend to be much lower than for the MPC_1^{ST}. Historical estimates out of changes in housing wealth range from 0 to 0.11 (Mian et al. 2013; Kaplan et al. 2016; Pistaferri 2016; Aladangady 2017; Guren et al. 2021). Looking at a period similar to ours (2012-2018), JMPCI (2020) arrive at a MPC_1^{LT} estimate close to zero (0 to 0.016) even among seemingly unconstrained households with ample access to liquidity or with significant home equity. While there is certainly variation in findings, it appears that the vast majority of changes in housing wealth are saved. Though the context is different, this is consistent with evidence that increases in employer pension contributions in Denmark are not offset through other savings, even among richer households (Chetty et al. 2014). There are fewer studies estimating the MPE_1^{LT}. Bernstein's (2021) estimates, also based on house price changes, are close to zero. Together, these findings suggest a $MPCL_1^{LT}$ close to zero.

In sum, some implied estimates of the $MPCL_1^{ST}$ are close to one, while the $MPCL_1^{LT}$ appears close to zero. This means that a MWA close to one is plausible ex-post, though not obvious ex ante. Again, context matters. The nature of shocks in our work and the existing literature is different. In particular, households might perceive increases in home equity due to amortization or house price increases differently. The former might be less salient than the latter. Also, a house price change does not require any change in spending or labor supply, while higher amortization does. Further, increased amortization implies predictable future wealth changes, whereas future house prices are more uncertain. The fact that estimates are (roughly) in line with one another, however, may suggest a more fundamental mechanism that links home equity to savings decisions. Finally, our estimates are more precise than what could be inferred from the existing literature. We can, for example, confidentially reject estimates of less than 0.5 across virtually all specifications. This implies a significant wedge between marginal propensities to consume out of short and long-term shocks that is not necessarily obvious from the existing literature.

V.B. How does our MWA estimate compare to other types of illiquid wealth contributions (pensions)?

Nudges, such as some default contribution level, have been shown to importantly influence pension contributions (Madrian and Shea 2001). Increases in contributions do not appear to be offset in the short-term by changes in liabilities (Beshears et al. 2022), but do appear to be reversed within just a few years (Choukhmane 2019, Wang et al. 2022). This is different from our finding that the MWA remains high even five years out. One possible reason is that it is relatively inexpensive to undo default settings in pension contributions, while there are substantial costs to extracting home equity. Also, as we discuss in Section IV.D.3, our results might be partially due to mental accounting with households viewing additional amortization as a bill, or at least as some non-standard account. These unique features suggest that the "mortgage piggy bank" should be considered a distinct mechanism that causes people to save more. Further, our results suggest that changes to amortization rules appear to have longer-term effects on wealth accumulation than interventions such as pension contribution nudges.

The literature has also considered how illiquidity itself could contribute to socially optimal savings plans. Deviating from full rationality, Beshears et al. (2020) argue that in a model with taste shocks and present-bias, the optimal plan includes an account with early liquidation costs, which (if our effects persist) could be like the amortization-induced home equity that we document in this paper.

V.C. How do our findings contribute to the literature on mortgage choice?

There is a large literature on how liquidity constraints, financial sophistication, savings preferences, and future income expectations affect a household's choice to get an alternative mortgage product (AMP), such as an interest-only (IO) or adjustable rate mortgage, or to refinance into such products (e.g., Mian and Sufi 2009; Cocco 2013; Cox et al. 2015; Kuchler 2015; Adelino et al. 2016; Hertzberg et al. 2018; Larsen et al. 2018; Bäckman and Khorunzhina 2020). Such choices can radically alter households' effective amortization schedule. Our finding that the MWA is close to 1 across many different groups of households suggests that mortgage choice will have substantial effects on wealth accumulation. As suggested by Section IV.D.2, this is even true if those opting for AMPs or refinancing have a higher cost of reducing short term consumption or are more financially sophisticated, though expected effects will be smaller.

Though important, the existing literature has not been able to draw this conclusion yet. Mortgage choice is likely endogenous and co-determined with other savings decisions. Households choosing IO mortgages or AMPs have been shown to differ systematically and substantially even prior to home purchase (Cocco 2013; Cox et al. 2015; Kuchler 2015). Related work has looked at mortgage run-offs (moments when mortgages are fully repaid) to provide quasi-experimental evidence on effects of the complete removal of mortgage payments on household behavior (Coulibaly and Li 2006; Scholnik 2013; Andersen et al. 2022). These studies have not estimated the MWA, likely because selection concerns make this challenging.[21] Just like the choice of mortgage type is potentially endogenous, responses prior to run-offs indicate that the choice of when to finally repay a mortgage is as well (Andersen et al. 2022). Even if these challenges could be overcome, results would complement (rather than compete with) our current findings: whereas we measure the wealth effects of amortization during a household's first formative years, mortgage

[21] Run-offs only provide a quasi-exogenous change in amortization if they were determined (far) in the past, in particular if borrowers chose to remain on the same amortization schedule during the life of mortgage, without pre-paying or refinancing. However, such borrowers are rare and likely unrepresentative. By revealed preferences, they take their amortization schedule as given and adjust consumption and other savings around it. Their response to a mortgage run-off is likely to be different from those who did choose to pre-pay or refinance. Alternatively, one could look at the anticipated run-off based on the "glide-path" of a household's mortgage repayment. However, that timing is a choice, potentially reflecting pre-payments that are jointly determined with future consumption, savings, and labor income decisions. For example, households may time the run-off to coincide with the desired moment to work fewer hours or anticipated changes in spending patterns and savings goals.

run-offs provide insights much later in life-cycle, arguably around a watershed moment when individuals start thinking about moving, semi-retiring, etc.

V.D. To what degree can our findings be applied to other settings outside of the Netherlands?

While any parameter estimated in one country is subject to that country's institutional and cultural context, there are a number of reasons to believe that our results extend to other countries as well. First, others have used the Netherlands to study consumption, labor, and savings decisions as well (Kuhn et al 2011), and the estimated Dutch MPC_1^{ST} lines up well with estimates from other countries including the U.S. (e.g. Johnson et al. 2006; Agarwal et al. 2007; Parker et al. 2013; Kueng 2018; Olafsson and Pagel 2019; Jørring 2020; Ganong and Noel 2020). One might expect that this also holds for the MWA as this parameter is tied (though not identical) to the MPC_1^{ST}. If anything, the Dutch MPC_1^{ST} estimate is on the low end, suggesting that the Dutch MWA estimate is a lower bound (cf. Section II.A). Second, the Netherlands has a highly educated population (in terms of tertiary education ranked 8th in the OECD, OECD 2022). This suggests it is unlikely that the people we examine are atypically prone to making (financial) mistakes. Third, while the U.S. and the Netherlands have comparable rates of home ownership, older Dutch households are much more likely to still have a mortgage (Section VII of the Online Appendix). This suggests that people may (broadly) follow the standard amortization rules (fully amortizing in the U.S. vs partly interest-only in the Netherlands for pre-2013 mortgages), and that changing amortization rules can have comparable effects in different countries. Fourth, the Netherlands share many similarities with other Northern European countries in terms of their home equity usage and social safety nets which are consistent with a broader applicability of our findings. Even in settings like the U.S., with weaker safety nets and lower costs of home equity withdrawal, effects are plausibly similar since we find that the MWA is largely unchanged at the time of moving (when withdrawals costs are low). Finally, a high MWA appears ubiquitous across groups with different levels of (liquid) savings and future projected pension payouts (Table 5), and age (Section VII of the Online Appendix), suggesting that our results are likely to apply across different sets of people.

V.E. To what degree can our estimates be applied to evaluate macroprudential policies?

Our MWA estimate is based on a shock that changes amortization, but that leaves the regulatory PTI constraint unchanged. Also, we do not find that our shock changes households' decision if, or when, to purchase a home and for what price. As such, we can observe the direct effects of amortization on wealth accumulation without confounding influences from (regulatory-induced) changes in demand. This means that we can directly apply our estimates to settings that only feature changes in amortization. For example, all households with an amortizing mortgage experience an increase in amortization over the life of the mortgage and our findings suggests that this increases wealth accumulation over the life-cycle. As said, mortgage choice and the possibility to refinance a mortgage could delay mortgage repayment, and thereby reduce wealth accumulation. Further, our estimates have direct implications for macroprudential policies during recessions (Piskorski and Seru 2018; Ganong and Noel 2020). In particular, policies that encourage contracts with countercyclical amortization (Guren et al. 2021; Campbell et al. 2021; Kovacs and Moran 2020) are likely to have an even bigger impact than implied by standard models. Our results also suggest that additional amortization in good times does not reduce households' liquid savings. Together with the fact that households do not seem to undo the additional amortization (at least not during the first five years of the reform), this suggests that households' financial stability may improve. The caveat here is that this is achieved through a reduction in consumption and leisure which could make households less flexible in dealing with future shocks.

Our estimates are still applicable (though less directly so) to broader macroprudential policies that also have important (regulatory) demand effects, such as an increase in amortization requirements that does tighten the PTI constraint (e.g., Best et al. 2018; Svensson 2019, 2020; DeFusco et al. 2020b, Bäckman and van Santen 2020). First, our results pin down one important parameter a policy maker requires to evaluate the likely impact of such a policy - the wealth effect of amortization for the group of people who still purchase a house (scaled down by the possible reduction in the size of their mortgage). Second, our high MWA estimate suggests that demand effects have implications for wealth accumulation that are larger than perhaps anticipated. Macroprudential policies that limit leverage and homeownership may exclude sub-groups of households (e.g., Charles and Hurst 2002; Krivo and Kaufman 2004) who then not only miss out on homeownership, but also on the wealth accumulation effects of an amortizing mortgage. This is critical since households appear to use home equity as a primary form of savings, with real estate accounting for over 70% of U.S. households assets (Campbell 2006; Poterba et al. 2013).

VI. Conclusion

We provide the first empirical evidence on the effects of mortgage amortization on wealth accumulation by using detailed individual-level administrative data and variation in the timing of purchase by home buyers around a 2013 reform in the Netherlands. We find that even five years later there is no observable change in non-mortgage savings, leading to a substantial rise in net worth. The effects occur suddenly, and only for cohorts who are exposed to the reform. We find no evidence of bunching and results are unchanged using the timing of life-events (e.g. birth of a child) as an instrument for buying before vs. after the reform. The rise in wealth accumulation is achieved through an increase in labor supply and reduction in expenditures. Our findings hold looking at households with substantial liquid assets and across a broad age range, suggesting our results hold for the general population, and not just for non-savers and the young. They are, however, lower for people who appear to prefer short term consumption over long term housing consumption and consumption certainty, and for people who appear more sophisticated.

Aggregate mortgage amortization is economically large, in fact similar in size to pension contributions, so the finding of a substantial effect of amortization on wealth building has important implications. Ex-ante macroprudential polices aimed at building up home equity through amortization may not significantly reduce household liquidity. Ex-post macroprudential policies that reduce principal repayments during recessions are likely to have larger effects than in standard models. Our results also suggest that homeownership is a critical driver of household wealth building when coupled with an amortizing mortgage.

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Figures and Tables

Figure 1. The Percentages of New Mortgages that are Fully Amortizing, 2012-2013

This figure shows the percentage of new mortgage offers in the Netherlands that are fully amortizing by offer date in each month from 2011 to 2014. The red dashed line indicates the implementation of the 2013 reform examined in this paper that discouraged the use of interest-only loans.

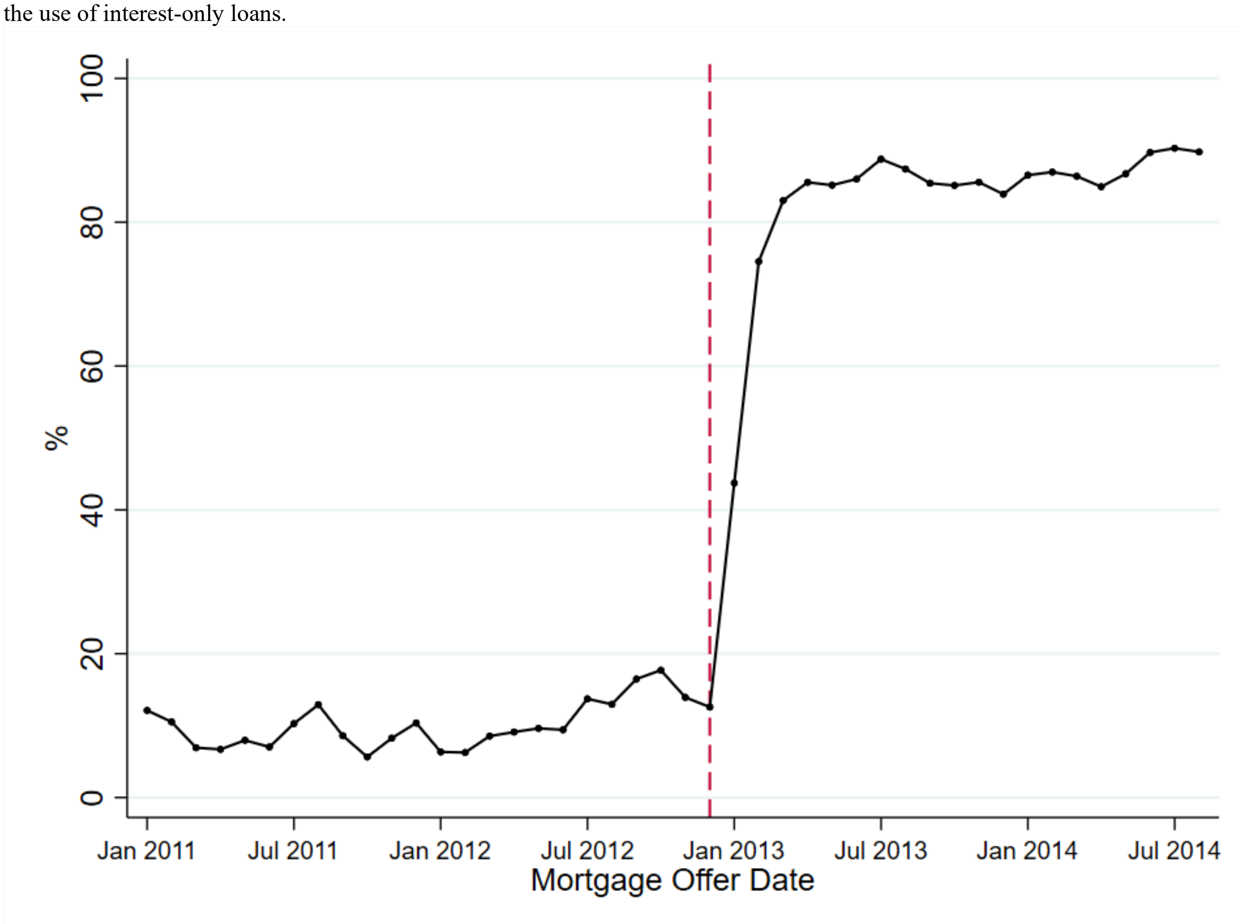
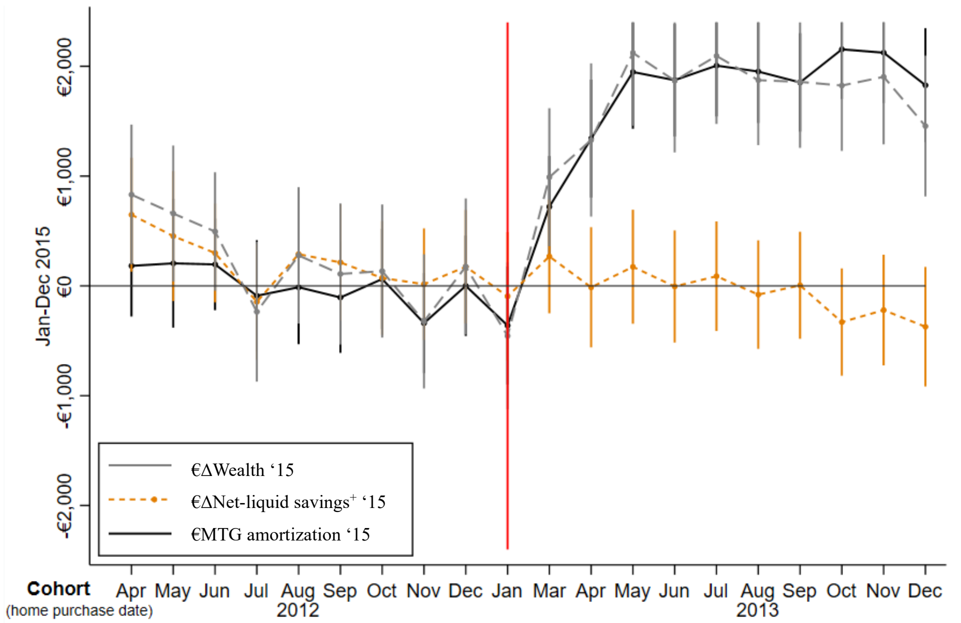


Figure 2. Cohort Estimates (Baseline)

Panel A shows the effect of mortgage amortization on wealth accumulation using variation in the timing of the closing of home purchase for first-time home buyers around the 2013 reform, following equation (12). The sample includes all first-time home buyers in the Netherlands who closed on their home between April 2012 and December 2013. We regress mortgage repayment (solid black line), wealth accumulation (gray dashed line), and *net-liquid savings*$^{+}$ (yellow dotted line), all from January to December 2015, on categorical dummy variables for each cohort (month of closing on the house) that are on the x-axis. We include no other control variables. The omitted cohort is February 2012 and each dot gives the estimate for the relative effect each month. Around each dot, we plot 95% confidence intervals that are based on standard errors clustered at the household level. Panel B shows the number of underlying home closings for our sample. The spike of home purchases in June 2012 reflects the expected increase in in the real estate transaction tax (that never materialized).

Panel A: Amortization and Wealth Accumulation in 2015



Panel B: Timing of home closings, April 2012 – December 2013

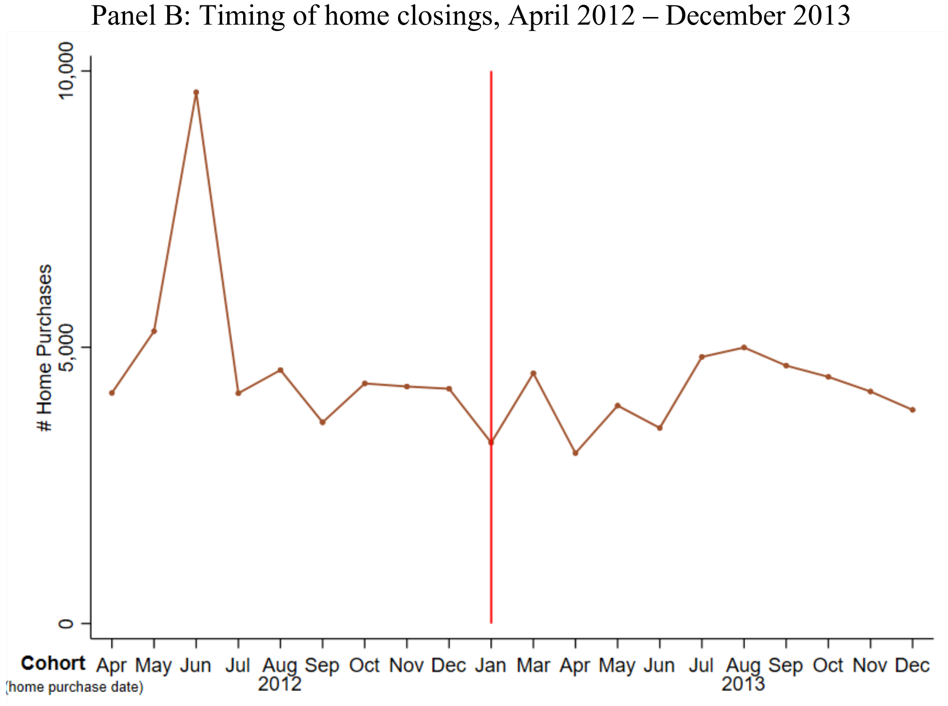


Figure 3. Amortization and Wealth Accumulation: Cohort Estimates (Life-events)

This figure shows the effect of mortgage amortization on wealth accumulation using the timing of a “life-event” as an instrument for the timing of the closing of home purchase around the 2013 reform. The sample includes all first-time home buyers in the Netherlands who closed on their home from Q1 2012 to Q4 2013 and who experienced a life-event during this period. Life-events are defined as quarters with changes in the number of members of a household (e.g. birth of a child). Following equation (12), we regress mortgage repayment (black) and wealth accumulation (gray), both from January to December 2015, on categorical dummy variables for each life-event cohort (quarter of a life-event). We include no other control variables. The omitted quarters are Q4 2012 and Q1 2013 and each dot gives the estimate for the relative effect each quarter. Around each dot, we plot 95% confidence intervals that are based on standard errors clustered at the household level.

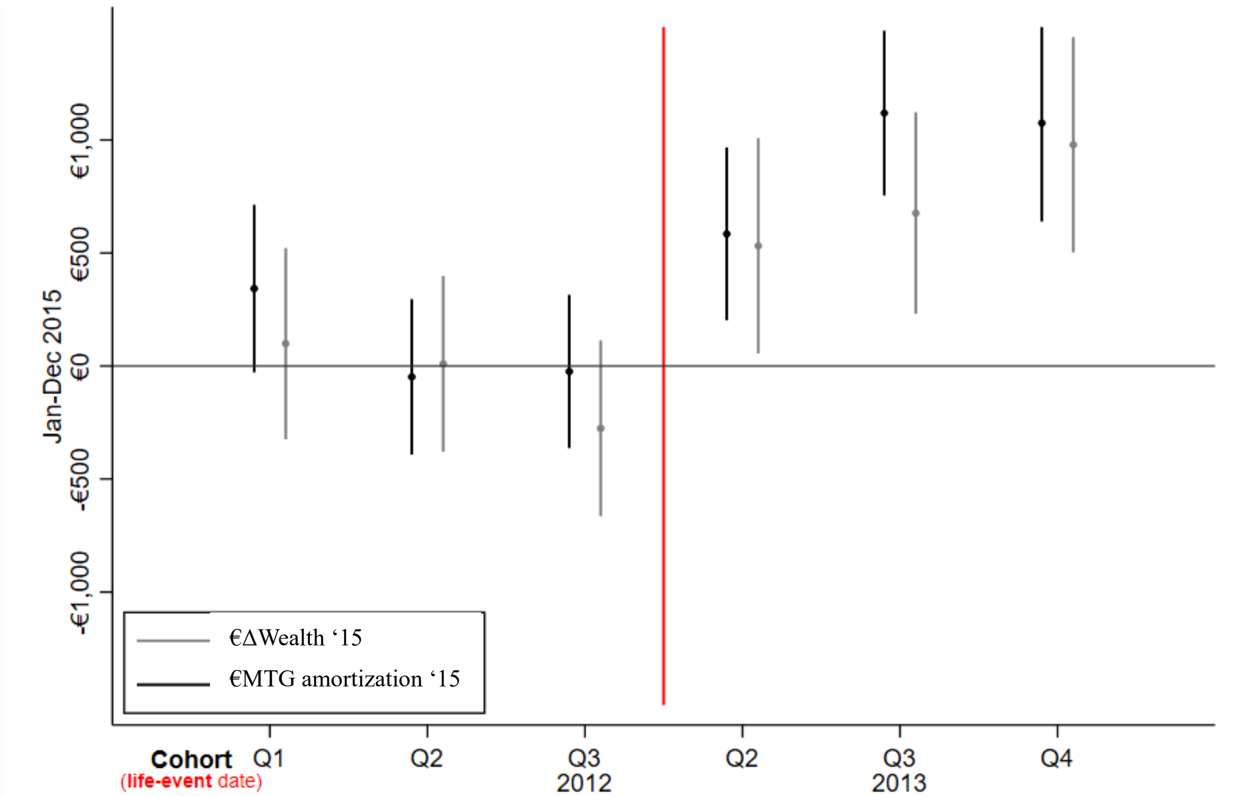


Figure 4. Amortization and Wealth Accumulation: Diff-in-Diff Estimates

The figure gives the difference-in-difference estimates from equation (16) to study differential (pre-)trends. The outcome variables (home equity, *liquid savings*, and total savings or net-worth) are in levels. The estimates show differences between first-time home buyers buying just before (Jan-Feb 2013) or after (March-April 2013) the reform (i.e. the "narrow window definition of Table 2, column (7)), and show how these differences evolve over time (from 2007 to 2017). The omitted year is 2012. Differences in home equity are zero before the reform by design, as the households have not purchased a house yet. When calculating differences in home equity, we assume that there is no differential house price appreciation for those buying before or after the reform (we verify this assumption in Section III.A of the Online Appendix). Before buying a house, a household's net-worth is simply all liquid savings minus total (non-mortgage) liabilities.

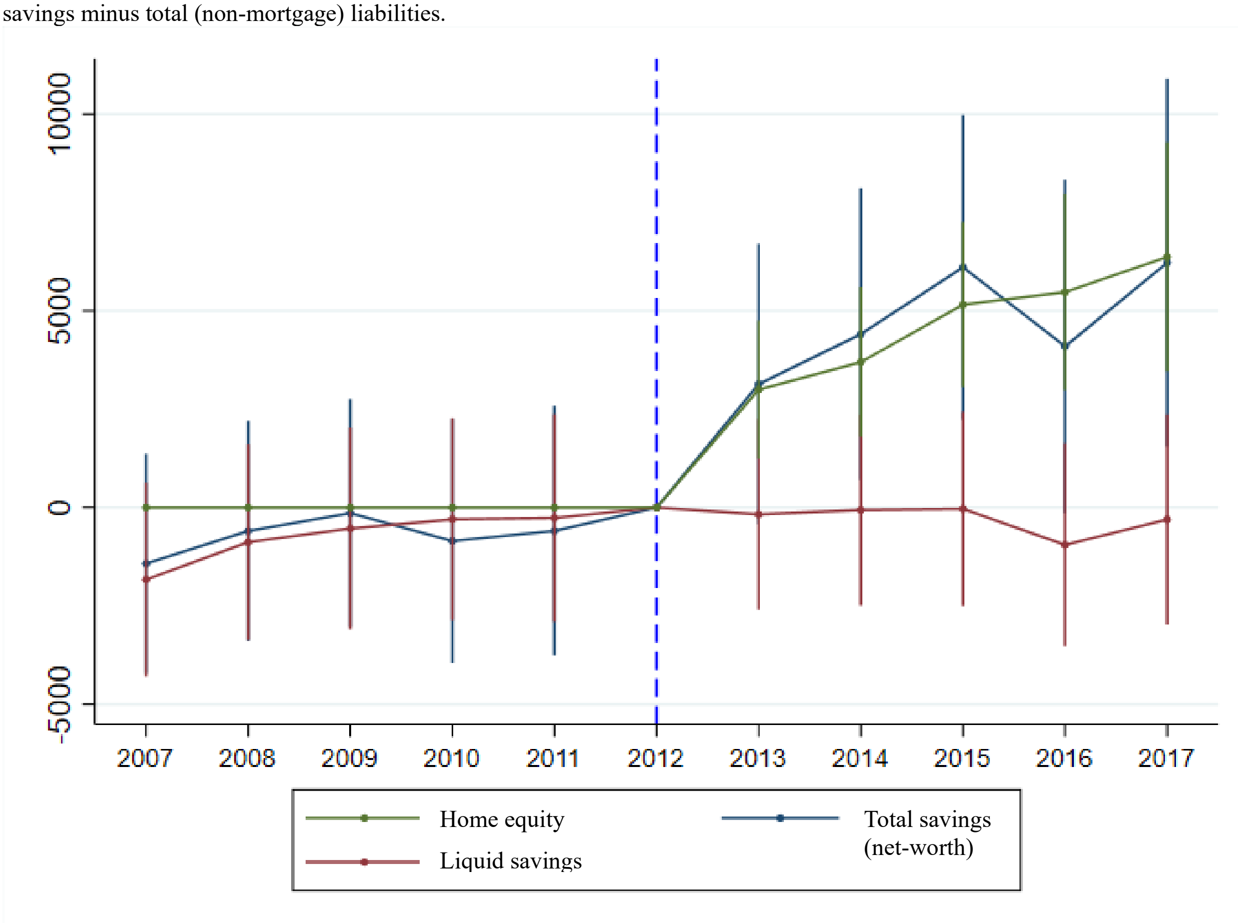


Figure 5. Amortization and Hours Worked: Cohort Estimates

The figure replicates the purchase-month-cohort analysis of equation (12) in Figure 2, but rather than showing liquid and total savings, it shows how mortgage repayment in 2015 (solid black line) co-moves with the change in household hours worked between 2012 and 2015 (solid orange line) for first-time home buyers purchasing around the reform. Again, the omitted cohort is February 2012 and each dot gives the estimate for the relative effect each month. Around each dot, we plot 95% confidence intervals that are based on standard errors clustered at the household level.

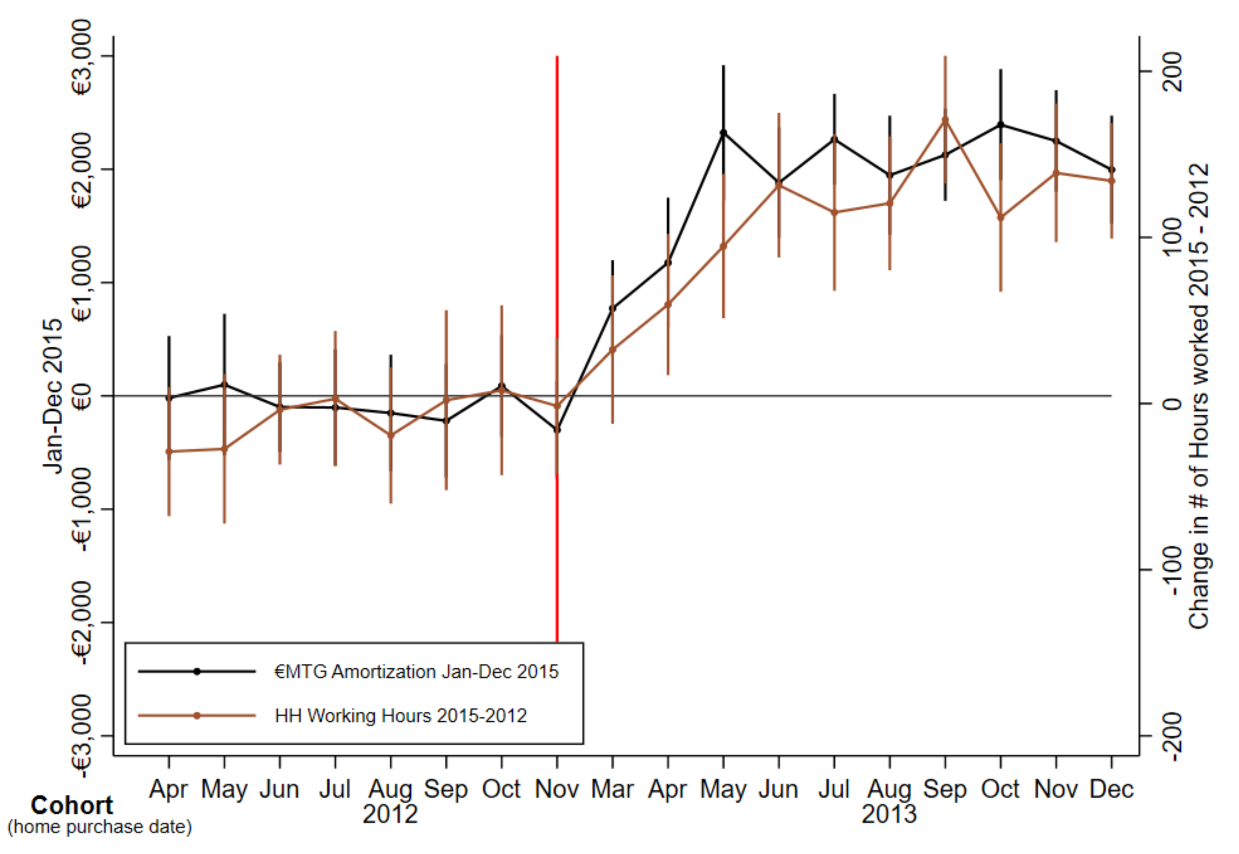


Figure 6. Hours Worked: Diff-in-Diff Estimates

This figure provides diff-in-diff estimates from equation (16) for the number of household hours worked. The estimates show differences between first-time home buyers buying just before (Jan-Feb 2013) or after (March-Dec 2013) the reform, and show how these differences move over time (from 2007 to 2016, when this data ends). Compared to Figure 4, we extend the after-reform group to households buying up to December 2013. The change in hours worked is noisy (with many zeros) and we need more observations to increase statistical power (since we take hours worked in 2012 as baseline, we cannot extend the pre-group to households buying in 2012).

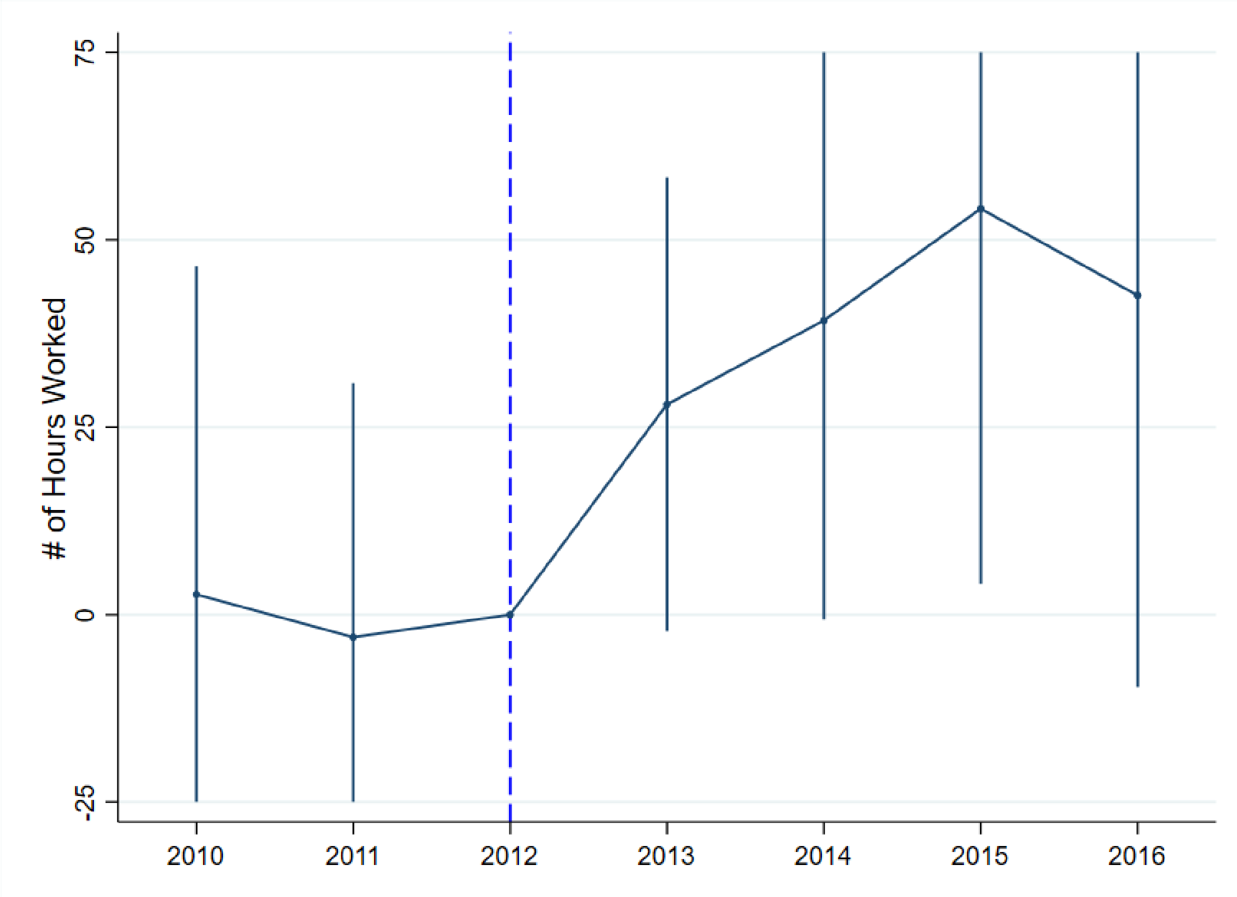


Table 1. Summary statistics

The table presents summary statistics for 2014 from the CBS administrative datasets for Dutch households buying a home financed with a mortgage between April 2012 and December 2013 (taking February 2012 as the omitted month, this is 10 months before and after the reform). We apply the two data filters and winsorization procedure described in Section III.A. Panel A is for first-time home buyers only. The table includes the population of all buyers in the Netherlands who we can identify as having no house or mortgage prior to these years, but do afterwards. We use this sample in Figure 2. In subsequent figures and tables, we use sub-samples of households who bought a house closer to the 2013 reform. Throughout the paper and below we distinguish three measures of savings. *Liquid savings* = all bank deposits (checking + savings + other) + stocks + bonds + other marketable securities. Δ*Liquid savings*$^{+}$ = Δ*Liquid savings* + voluntary pension contributions. Δ*Net-liquid savings*$^{+}$ = Δ*Liquid savings*$^{+}$ - ΔNon-mortgage liabilities. We can measure *Liquid savings* in both levels and flows. We do not have a measure of the level of voluntary pension assets, only the yearly flow, so we can only measure *Liquid savings*$^{+}$ and *Net-liquid savings*$^{+}$ in yearly flows. Panel B gives information for the subset of home buyers in Panel A who also have at least €10,000 as of the end of 2015 or accumulate at least €3,000 in additional *Liquid savings* over 2015. This corresponds to the sub-group in Table 5, Panel A, columns (1) through (3) but includes all home buyers, not those right around the reform. Panel C includes all home buyers, including those who have owned homes before.

Panel A. All First-time Home Buyers (N=94,094)					
	Mean	Median	Stdev	25th	75th
Mtg LTV Year-end '14	1.02	1.05	0.16	1.01	1.09
Mtg Balance Year-end '14 (€)	196k	183k	74k	148k	228k
Total Liabilities Year-end '14 (€)	205k	189k	90k	152k	236k
Income Year-end '14 (€)	70k	65k	35k	46k	87k
Liquid savings Year-end '14 (€)	19k	8.5k	31.6k	2.8k	22.6k
ΔLiquid savings '14-15 (€)	1.3k	0.3k	7.5k	-1.2k	3.3k
ΔLiquid savings^{+} '14-15 (€)	1.3k	0.3k	7.5k	-1.2k	3.4k
ΔNet-liquid savings^{+} '14-15 (€)	1.5k	0.6k	8.2k	-1.5k	4.1k
Panel B. "Savers" First-time Home Buyers (N=49,458)					
Liquid savings Year-end '14 (€)	32.1k	20.9k	38.9k	11.4k	38.6k
ΔLiquid savings '14-15 (€)	3.4k	3.1k	9.2k	-0.5k	7.1k
Panel C. All Home Buyers (N=253,831)					
Liquid savings Year-end '14 (€)	27.2k	12.4k	44.5k	4.1k	31k
ΔLiquid savings '14-15 (€)	1.4k	0.2k	10.7k	-1.5k	3.9k

Table 2. Mortgage Amortization and Wealth Accumulation

This table shows estimates of the intent-to-treat design laid out by equations (14) and (15). Columns (1) through (4) consider all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013 (for whom it is ambiguous whether they purchased before or after the reform). Relative to the sample we use in Figure 2, this is a smaller group buying closer to the reform. As dependent variable, we consider changes in the mortgage balance, total savings (wealth accumulation) or liquid savings (*net-liquid savings*$^+$) from January to December 2015. Coefficient *Post* captures whether households purchased after the reform (closing May 1st 2013 or later). Column (1) present the first stage estimates, where we predict the additional mortgage repayment of households purchasing after the reform (*Post)*. Column (2) presents the reduced form, where we estimate the additional wealth accumulation for this group. Column (3) has the formal two stage least square estimate, where we instrument the amount repaid with whether a household purchased after the reform. The estimated coefficient equals the MWA, measuring how much additional wealth is built out of an additional euro of mortgage amortization. Column (4) applies the same intent-to-treat design for the change in liquid savings (*ΔNet-liquid savings*$^+$), measuring how much liquid wealth is reduced (or increased) in response to an additional euro of mortgage amortization. Columns (5) and (6) consider changes between December 2013 and 2017. We omit the post-reform cohorts of August and September 2013 because the closing it sufficiently close to year-end that the level of liquid savings at the end of 2013 is temporarily depressed, which artificially increases the change in liquid savings between 2013 and 2017, leading to a bias in the MWA. Column (7) again considers changes in 2015, but zooms in on households closing just around the reform, in either January/February or March/April 2013. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	1st Stage	RF	IV	IV	RF	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	MTG Repaid ‘15	ΔWealth ‘15	ΔWealth ‘15	ΔNet-liquid savings$^+$ ‘15	MTG Repaid ’13-‘17	ΔWealth ’13-‘17	ΔWealth ‘15
Post	2,045.0***	2,038.2***			8,230.0***		
	(19.22)	(14.47)			(27.51)		
MTG Repaid			0.997***	-0.00329		0.926***	1.173***
			[0.88,1.10]	[-0.12,0.11]		[0.82,1.03]	[0.81,1.54]
			(17.74)	(-0.06)		(17.23)	(6.31)
IV	-	-	Post	Post	-	Post	Post
Narrow window	-	-	-	-	-	-	Y
F-Stat	-	-	369.3	369.3	-	756.8	37.4
Obs	42,468	42,468	42,468	42,468	25,001	25,001	15,223
Adj. R^2	0.020	0.011	0.330	0.001	0.072	0.317	0.261

Table 3. Mortgage Amortization and Wealth Accumulation – Life-events

This table uses the timing of a "life-event" as instrument for additional amortization induced by the 2013 reform. Life-events are defined to be months with changes in the number of members of a household (e.g. birth of a child). With the exception of Column (9), the sample includes all first-time home buyers who closed on their home in 2012 or 2013 and who experienced a life-event between November 2012 and September 2013. Relative to the sample we use in Figure 3, this is a smaller group buying closer to the reform. *Post(life event)* captures households having a life-event on March 1, 2013 or later. Columns (1) through (3) provide summary statistics as of 2010 for those having a life-event before or after the reform. Columns (4) and (5) provide estimates of the first stage and reduced form, while Column (6) looks at the change in house price between 2013 and 2015. Columns (7) and (8) use two stage least squares to estimate the *MWA*, where the latter excluded cases where households close in the same month as the life-event. The latter also excludes households with a life-event in March because some of those went under contract before the reform, reducing the power of the first stage and possibly creating statistical bias. Column (9) has the full sample of all households experiencing a life-event (not just those buying a home) that did not own a home at the end of 2011. The dependent variable is a dummy variable equal to 1 if the household owns real estate by December 2016. We regress this on a dummy variable equal to 1 if the life event occurred after the reform. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	Covariate Balance Tests			1st Stage	RF	RF	IV	IV	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	HH Income '10	ΔNet-liquid savings+ '10	ΔWealth '10	MTG Repaid '15	ΔWealth '15	%ΔHome Value '15-'13	ΔWealth '15	ΔWealth '15	Have Real Estate, '16
MTG Repaid '15							0.888***	0.957***	
							[0.45,1.32]	[0.24,1.67]	
							(4.03)	(2.62)	
Post(life event)	-249.5	-57.89	383.1	792.8***	704.3***	0.000026			0.0011
	(-0.36)	(-0.17)	(0.32)	(4.60)	(3.20)	(0.02)			(0.50)
Life-Event Buyer	Y	Y	Y	Y	Y	Y	Y	Y	Y
IV	-	-	-	-	-	-	Post(life)	Post(life)	-
Life!=Move Date	-	-	-	-	-	-	N	Y	-
F-Stat	-	-	-	-	-	-	42.3	15.4	-
Obs	16,581	16,559	16,559	16,581	16,581	16,581	16,581	11,363	317,552
Adj. R^2	0.000	0.000	0.000	0.003	0.001	0.000	0.355	0.357	0.000

Table 4. Labor and income response

This table shows how households' labor supply differs between those buying before or after the reform. The sample includes all first-time home buyers who closed on their home between October 2012 and February 2013 and May to September 2013 (*Post*). Panel A focuses on direct responses, while Panel B considers to what degree additional labor supply pays for additional amortization. Columns (1) and (2) in Panel A consider the change in hours worked between 2012 and 2015. Columns (3) and (4) look at the general relation between hours worked and income, or changes therein. Columns (5) through (7) consider changes in income between 2012 and 2015 and relates this to the timing of closing, adding the change in hours worked or the change in the number of earners in a household as possible mediating variables. Panel B implements the intent-to-treat design from equations (14) and (15), using whether a household purchased after the reform (closing May 1st 2013 or later). Columns (1) and (2) consider the change in income between 2015 and 2012 as dependent variable. Columns (3) and (4) decompose the effect by looking at the level of income in 2012 and 2015 separately. Columns (2) through (4) add municipal fixed effects and the natural log of household income and the level of liquid assets, both as of 2010, as covariates. This information is available for around 90% all households in the sample, and Column (5) re-estimates the MWA for this set. Next, we restrict the sample to households where all members aged 25 or older worked full-time (35 hours or more) in 2011. Column (6) looks at the change in income between 2012 and 2015, while column (7) estimates the MWA for this group. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

Panel A. Labor and Income Response

	RF (1) ΔHours worked '15-'12	RF (2) Ln ΔHours worked '15-'12	OLS (3) Income '12	OLS (4) ΔIncome '15-'12	RF (5) ΔIncome '15-'12	RF (6) ΔIncome '15-'12	RF (7) ΔIncome '15-'12
Post	86.12***	0.0492***			1,270.1***	364.2	1,033.1***
	(8.35)	(3.22)			(5.08)	(1.59)	(4.28)
Hours Worked '12			15.64***				
			(74.63)				
ΔHours worked '15-'12				10.54***		10.52***	
				(40.79)		(40.62)	
Δ#Earners '15-'12							9,916.6***
							(24.20)
Obs	42,468	42,468	42,468	42,468	42,468	42,468	42,468
Adj. R2	0.004	0.000	0.310	0.175	0.001	0.175	0.066

Panel B. Income Response – IV estimates

	IV (1) ΔIncome '15-'12	IV (2) ΔIncome '15-'12	IV (3) Income '12	IV (4) Income '15	IV (5) ΔWealth '15	IV (6) ΔIncome '15-'12	IV (7) ΔWealth '15
MTG Repaid '15	0.621***	0.665***	-0.0747	0.590***	0.987***	0.217	0.931***
	[0.38,0.87]	[0.37,0.96]	[-0.28,0.13]	[0.23,0.95]	[0.84,1.13]	[-0.09,0.53]	[0.77,1.10]
	(4.97)	(4.43)	(-0.72)	(3.22)	(13.43)	(1.38)	(11.03)
Muni FE	N	Y	Y	Y	Y	N	N
Add. Cntrls	N	Y	Y	Y	Y	N	N
Full-time '11	-	-	-	-	-	Y	Y
IV	Post	Post	Post	Post	Post	Post	Post
F-Stat	369.3	143.6	143.6	143.6	143.6	161.2	161.2
Obs	42,468	38,877	38,877	38,877	38,877	14,026	14,026
Adj. R2	-0.047	-0.048	0.046	0.034	0.313	0.001	0.332

Table 5. MWA for Households with a Demand for Long Term Savings

This table estimates the MWA for households who likely have a demand for long term savings. Panel A considers households who appear unconstrained and who, in all likelihood, can and want to save for the long term. Columns (1) through (3) consider three definitions under which household appear unconstrained *after* the reform (at origination, in 2014, or 2015): (1) having a mortgage with a loan-to-value ratio below 0.9, (2) a loan-to-income ratio less than four, or (3) having a stock of liquid savings of more than €10k and a flow or more than €3k. Next, we consider households who appear unconstrained *before* the reform (in 2011) and had at least €10k in liquid savings. Columns (4) and (5) present the average level of liquid savings in 2011 and 2015, and column (6) the MWA for this group. Column (7) estimates the MWA for households owning any stocks and bonds in 2011. Panel B considers households with an apparent interest in accumulating long term savings. Columns (1) and (2) look at households who, at some point between 2011 and 2017, made voluntary pension contributions and estimate the MWA over 2015 or 2013-2017. Columns (3) through (6) estimate the MWA over 2013-2017 for households with limited predicted pension payouts, either below €500 or €1000 per month, with columns (5) and (6) restricted to households who are actively contributing to an employer-based mandatory plan. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

Panel A. Substantial liquid savings

	IV	IV	IV	OLS	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ΔWealth '15	ΔWealth '15	ΔWealth '15	Liq. Sav. '11	Liq. Sav. '15	ΔWealth '15	ΔWealth '15
MTG Repaid '15	1.358***	0.959***	1.008***			0.967***	1.100***
	[0.95,1.77]	[0.82,1.10]	[0.86,1.16]			[0.79,1.14]	[0.58,1.62]
	(6.48)	(13.77)	(12.96)			(10.79)	(4.17)
Liq. Sav.'11>10k				43,445***	26,486***		
				(96.66)	(81.06)		
LTV '14	<0.9	-	-	-	-	-	-
LTI at orig	-	<4	-	-	-	-	-
Liquid Sav. '15	-	-	>10k|>3k	-	-	-	-
Liquid Sav. '11	-	-	-	-	-	>10k	-
Stocks/bonds '11	-	-	-	-	-	-	Y
IV	Post	Post	Post	-	-	Post	Post
F-Stat	32.5	265.4	223.0	N/A	N/A	350.3	41.8
Obs	5,762	27,569	22,005	42,468	42,468	17,268	4,007
Adj. R^2	0.202	0.328	0.252	0.243	0.173	0.302	0.267

Panel B. Making Voluntary Pension Contributions or Limited Predicted Pension Payout

	(1)	(2)	(3)	(4)	(5)	(6)
	ΔWealth '15	ΔWealth '13-17	ΔWealth '13-17	ΔWealth '13-17	ΔWealth '13-17	ΔWealth '13-17
MTG Repaid	1.210***	1.270***	0.976***	0.918***	0.995***	0.938***
	[0.56,1.86]	[0.919,1.621]	[0.82,1.14]	[0.66,1.18]	[0.81,1.18]	[0.58,1.230]
	(3.66)	(7.10)	(12.00)	(6.93)	(10.76)	(5.09)
Vol. Pen. '11-17	Y	Y	-	-	-	-
Pred. Mo. Payout	-	-	<1000 euro	<500 euro	<1000 euro	<500 euro
Mean " "	-	-	431	169	534	207
≥1 Active account	-	-	-	-	Y	Y
IV	Post	Post	Post	Post	Post	Post
F-Stat	10.3	33.0	219.7	95.8	177.9	46.9
Obs	3,878	3,143	10,857	6,072	7,919	3,290
Adj. R^2	0.239	0.492	0.435	0.389	0.427	0.401

Table 6. Movers Sample

The table examines the sub-sample who bought their first home between October 2012 and September 2013 (excluding March and April 2013) and then sold their home and moved by December of 2016. Columns (1) and (2) consider whether those first buying after the reform have a lower mortgage balance at the end of 2017, when our sample ends. Column (3) looks at whether this group extracted more home equity during the move. Columns (4) and (5) consider the stock of liquid assets at the end of 2017. Column (6) tests whether the MWA is lower for movers than for the rest of the sample. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	OLS (1) MTG Bal '17	OLS (2) MTG Bal '17 (ln)	OLS (3) Home Equity Extraction at Sale	OLS (4) Liquid Assets '17	OLS (5) Liquid Assets '17 (ln)	IV (6) Chg Wealth '16
Post	-15,507**	-0.0630**	-1,303.0	-1,056.9	-0.0702	
	(-2.04)	(-2.00)	(-0.25)	(-0.40)	(-0.58)	
MTG Repaid '16						0.946*** [0.80,1.10] (12.34)
MTG Repaid '16 x Mover						0.305 (1.16)
Move '13-16	Y	Y	Y	Y	Y	-
IV	-	-	-	-	-	Post
Obs	1,768	1,689	1,799	1,768	1,768	39,312
R^2	0.003	0.004	0.000	0.000	0.000	0.401

Table 7. Heterogeneity analysis

This table explores heterogeneity in our sample, motivated by the stylized model of Section II.A. We first consider households who put little money down when they purchased their home, where "little money down" is defined as a down payment between 0 and 2% of the purchase price. Columns (1) through (3) estimate this group's MWA over 2015, or 2013-2017, and test whether it is significantly lower than the rest of the sample. Column (4) includes a dummy for high house price appreciation (i.e. the assessed value of a house increased by at least 10% between 2013 and 2017) as additional covariate. Column (5) tests whether the likelihood of putting little money down changed due to the reform (which would be indicative of differential selection). Column (6) considers households who, in a given month, contracted a mortgage interest rate in the lowest quartile, indicative of a short maturity or, possibly, successful rate shopping. Column (7) considers households where at least one member has followed a theoretical ("high") rather than a practical degree. Column (8) tests whether the likelihood of having a higher education changes after the reform (which, again, would be indicative of selection). Estimates in columns (2) through (4) and (6) to (7) include the heterogeneity variable independently as well (in addition to its interaction, which is reported in the table). Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	(1) ΔWealth '15	(2) ΔWealth '15	(3) ΔWealth '13-17	(4) ΔWealth '13-17	(5) Little Money Down	(6) ΔWealth '15	(7) ΔWealth '15	(8) Hi Educ
MTG Repaid	0.699***	1.072***	0.980***	0.969***		1.085***	1.135***	
	[0.45,0.95]	[0.95,1.20]	[0.86,1.10]	[0.84,1.10]		[0.96,1.21]	[0.96,1.31]	
	(5.56)	(16.76)	(15.58)	(14.87)		(17.05)	(12.67)	
MTG Repaid		-0.373***	-0.284**	-0.288**				
x Little Money Down		(-2.64)	(-2.25)	(-2.22)				
Post					-0.0053			0.0057
					(-0.82)			(0.94)
MTG Repaid						-0.372***		
x Lo MTG Rate						(-2.84)		
MTG Repaid							-0.235**	
x Hi Educ							(-2.42)	
Vol Money Down (%)	0-2%	0-2%	0-2%	0-2%	0-2%	-	-	-
Hi ΔHouse Price	-	-	-	Y	-	-	-	-
Incl. hetero var.	-	Y	Y	Y	-	Y	Y	-
IV	Post	Post	Post	Post	-	Post	Post	-
F-Stat	107.2	190.2	266.1	244.3	-	194.8	163.5	.
Mean Dep Var					0.2894			0.2894
Obs	12,292	42,468	25,001	22,717	42,468	42,468	35,929	35,929
R-sq	0.293	0.315	0.314	0.310	0.000	0.314	0.340	0.000

Online Appendix

Table of Contents

I. Macro-economic developments, 2012-2017

I.A. Overview

An advantage of studying the Dutch 2013 reform is that there were no other dramatic changes in macroeconomic and mortgage conditions around 2013. Figure A.1 shows that Dutch GDP and employment bottomed our right after the reform and started to grow afterwards (after first falling due to the Global Financial and Euro crises). Figure A.2 shows that house prices bottomed out in early 2013 and increased afterwards. The figure also shows that average mortgage interest rates were smooth around the reform even though increased amortization implies shorter duration. This likely reflects the fact that default risk is limited (because of the strict recourse laws), and that fixed rate periods are typically short (85% of homeowners had rates that became floating within the first 10 years). Figure A.3 shows that average origination LTVs and LTIs also varied smoothly around the reform. These results suggest that mortgage supply did not change in response to the reform.

Figure A.1 Dutch GDP and Employment

This figures plots quarterly nominal GDP (solid line) and employment (grey dashed line) between 2007 and 2017. GDP is normalized to 100 in the first quarter of 2013. Source: CBS.

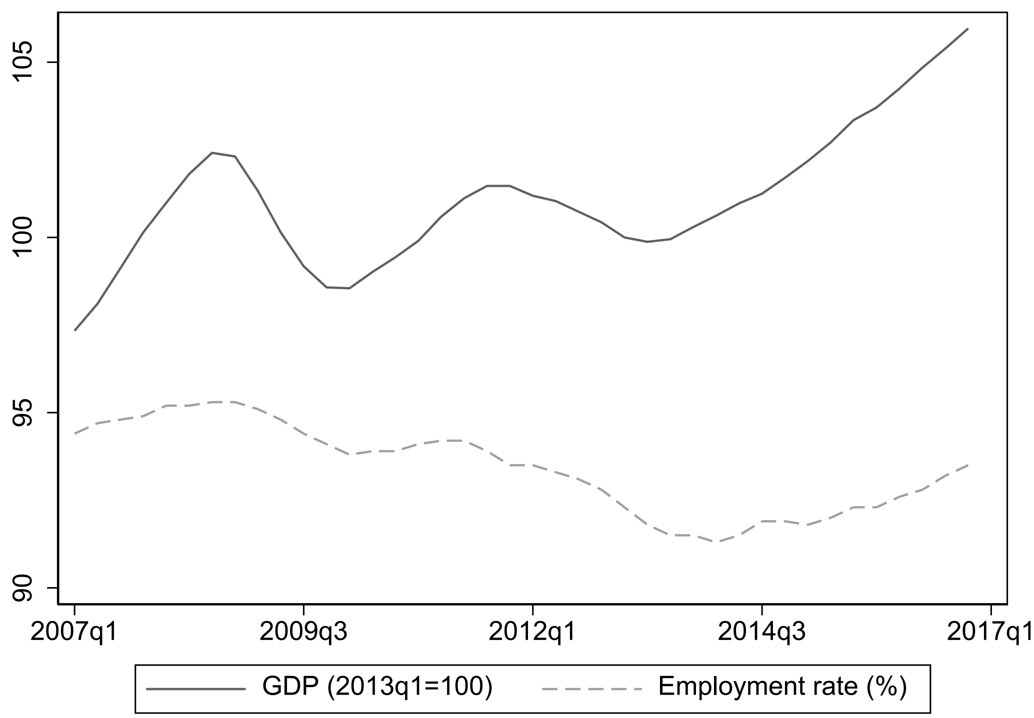


Figure A.2 Dutch house prices and mortgage interest rates

This figure present aggregate Dutch housing trends around the January 2013 reform. House prices (black line) are normalized to be 100 in 2005 and plotted on the left y-axis. Average residential mortgage interest rates (gray line) are plotted on the right y-axis. All data come from aggregate statistics publicly available from aggregate CBS data.

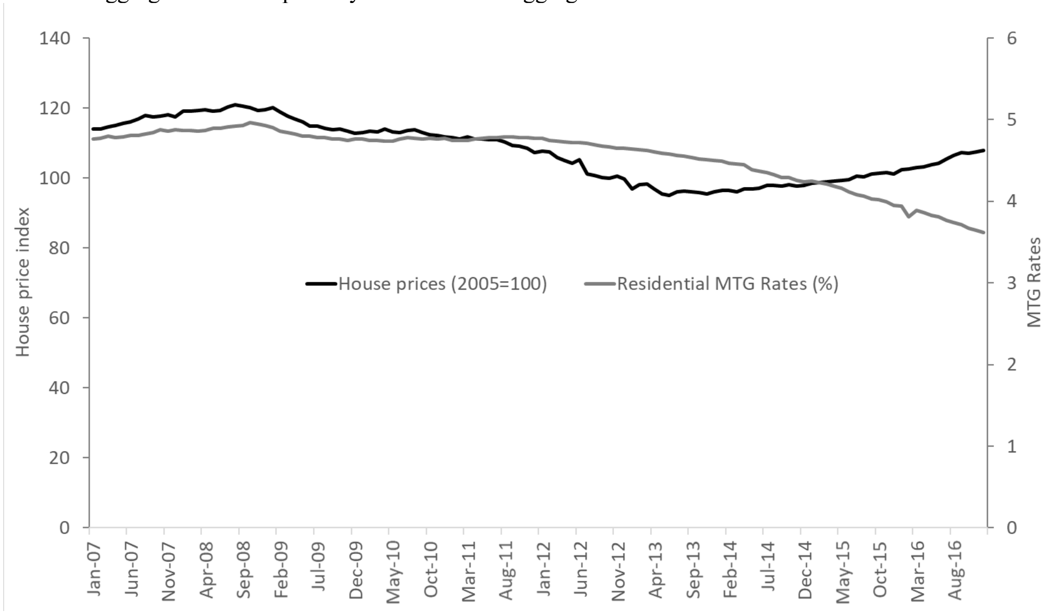


Figure A.3 Origination LTV and LTI (mean) by Mortgage Offer Date for FTHBs, 2012-2013

This figure depicts the mean origination loan-to-value (LTV) and loan-to-income (LTI) of mortgage offers for first-time homebuyers in 2012 and 2013 by mortgage offer dates. Data come from HDN and cover about 3/4s of mortgage offers. The sample includes all mortgages offered to first-time homebuyers, for those aged 30 and older, where the mortgage product type is at least partially known. The new mortgage reform affected mortgages originated after December 2012 (vertical red dashed line).

I.B. The MWA and macro-economic conditions

In Table A.1, we explore the degree to which the MWA depends on local economic conditions using use geographic variation in the depth of the recession and speed of the subsequent recovery after 2013. Columns (1) and (2) check whether the MWA is different in municipalities that saw different house price changes between 2011 and 2013 or 2013 and 2017. This is not the case. Column (3) also considers house price changes between 2013 and 2017 but focuses at the property level. Again, the MWA is virtually unchanged. Finally, column (4) considers the municipality unemployment level (in 2016, the earliest year for which this is available) and finds no effect on the MWA.

Table A.1 the MWA and local economic conditions

This table tests whether the MWA differs with economic conditions. We estimate the MWA using the IV strategy following equations (14) and (15), where we instrument mortgage repayment in a given year with *Post*, a dummy for home closings on May 1st or later. We include interactions between the instrumented mortgage repayment and several measures of local economic conditions (all in the form of Z-scores). Column (1) includes all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013, and estimates the MWA over 2015, looking at the interaction effect with municipal house prices changes between 2011 and 2013. Columns (2) through (4) estimate the MWA over the full period between 2013 and 2017, where, as in Table 2, columns (5) and (6), we omit the post-reform cohorts of August and September 2013. Column (2) looks at the interaction effect with contemporaneous municipal house prices changes between 2013 and 2017, column (3) at the individual's property house price change (based on annually updated official appraisals), and column (4) at the change in the municipal unemployment rate between 2015 and 2016 (when this information is first available). Estimates include the heterogeneity variable independently as well (in addition to its interaction, which is reported in the table). Below the coefficient estimates, we report t-statistics based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	IV (1) ΔWealth '15	IV (2) ΔWealth '13-17	IV (3) ΔWealth '13-17	IV (4) ΔWealth '13-17
MTG Repaid	0.996***	0.926***	0.919***	0.928***
	(17.68)	(17.32)	(16.94)	(17.21)
MTG Repaid	-0.0637			
x ΔMuni Houseprice, '11-'13 (Z)	(-1.04)			
MTG Repaid		0.053		
x ΔMuni Houseprice, '13-'17 (Z)		(0.92)		
MTG Repaid			-0.077	
x ΔProp Houseprice (Z) , '13-'17 (Z)			(-0.62)	
MTG Repaid				-0.061
x ΔMuni Unemp., '16-15 (Z)				(-1.25)
IV	Post	Post	Post	Post
Includes heterogeneity variable	Y	Y	Y	Y
F-Stat	184.6	755.1	654.6	631.9
Obs	42,468	25,001	25,001	25,001
R^2	0.327	0.322	0.318	0.317

II. Data: sources and reliability

II.A. Data sources

All data comes from the Dutch Statistics Office (CBS). Table A.2 maps the types of data we use into their respective sources.

Table A.2 Data sources

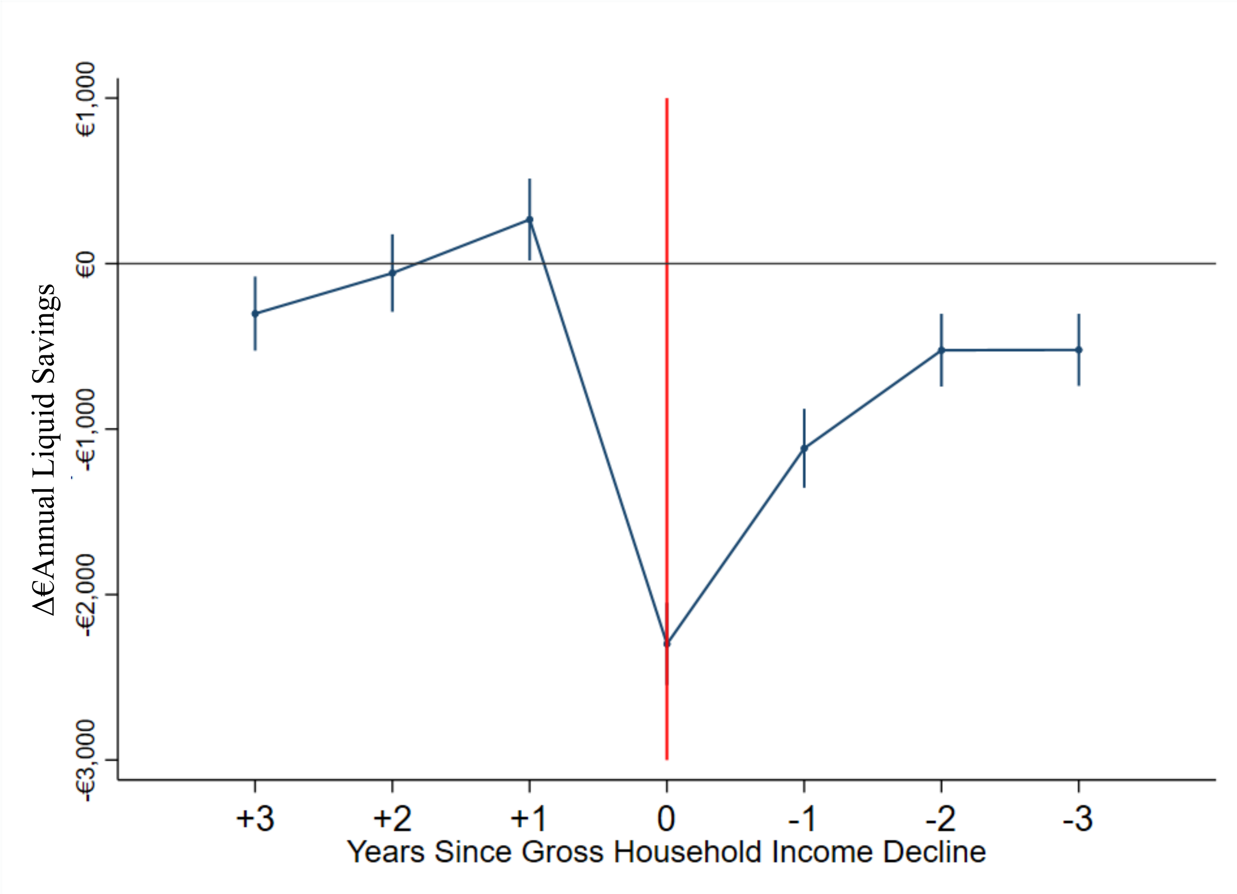
Information type	Name dataset in CBS micro data
Car registrations	*Rdwnpacttab*
Existing purchase dwellings registry	*Bestaande Koopwoningen*
Family structure	*Huishoudensbus*
Hours worked and other job characteristics	*Spolisbus*
Household balance sheets	*Integraal Vermogen*
Household income statements	*Inpatab*
Socio-demographic characteristics	*Persoontab*
Spells for individual addresses	*Adresbus*
Total (voluntary and mandatory) pension entitlements	*Pensaanspraakoptab*

II.B. Reliability

The data on liquid savings provided by CBS seem accurate and up-to-date. In Figure A4, we plot yearly changes in liquid savings in the years around a decline in gross household income, after including household and time fixed effects. As expected, there is a substantial reduction in the year of the income decline as households use their liquid savings as a buffer.

Figure A.4 Sensitivity of liquid savings to income declines

This figure looks at the sensitivity of liquid savings to income declines. We compute the yearly change in liquid savings for the full sample of first-time home buyers from Table 1, Panel A for all years between 2006 and 2016. We determine years with declines in gross household income. We plot the coefficients from a regression of the change in liquid savings on dummy variables for the years before or after the decline in income. We include household and year fixed effects. Vertical lines give 95% confidence intervals that are based on standard errors clustered at the household level.



III. Robustness: alternative variable creation and sample selection

III.A. Alternative wealth measures

Table A.3 uses different wealth measures. Column (1) includes house price changes (as observed in annually updated appraisals) in our wealth accumulation measure. Next, we consider the levels of net worth and home equity, rather than changes, where home equity is calculated using the appraised house price. Column (2) considers the actual date of closing, while Column (3) considers "life-events". All three columns give an estimate of the MWA close to one.

Table A.3 Alternative wealth measures

This table calculate the *MWA* using alternative wealth measures. Column (1) adds house price appreciation (observed in annually updated appraisal reports) to our measure of wealth accumulation. Columns (2) and (3) look at the levels of home equity (calculated with appraised house prices) and the mortgage balance. Columns (1) and (2) include all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013. *Post(buy)* captures households closing on a home on May 1, 2013 or later. Column (3) includes all first-time home buyers who closed on their home in 2012 or 2013 and who experienced a life-event between November 2012 and September 2013. *Post(life event)* captures households having a life-event on March 1, 2013 or later. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	IV (1) ΔWealth '15	IV (2) Net Worth '15	IV (3) Net Worth '15
MTG Repaid '15	1.226***		
	[0.968,1.484]		
	(9.32)		
Home Equity '15		0.971***	0.990***
		[0.884,1.058]	[0.625,1.354]
		(21.94)	(5.33)
Life-Event Buyers	N/A	N/A	Y
Period	'15	'15	'15
IV	Post(buy)	Post(buy)	Post(life)
Include house price	Y	Y	Y
F-Stat	369.3	472.5	27.0
Obs	42,468	42,468	16,581
R^2	0.057	0.664	0.659

III.B. Alternative amortization assumptions

The partial amortization of mortgages extended before the reform happened through linked savings accounts for which we miss detailed information. We assume that if a mortgage has no year-over-year change in its balance, it is 50% amortizing through a linked account and we impute the amortization the assuming an annuity mortgage with an interest rate of 4.50% (the average rate in 2012-3). Table A.4 explores wo what extent our estimates are sensitive to these assumptions. Columns (1) through (5) use different assumption on how much pre-reform mortgage amortize, ranging from 0 to 70%. Columns (6) and (7) use different mortgage rates. The table shows that this has virtually no effect on our MWA estimates.

Table A.4 Alternative amortization assumptions

This table experiments with alternative amortization assumptions for mortgages extended before the reform. Columns (1) through (5) consider a different percentage of the mortgage that is amortizing. Columns (6) and (7) consider different mortgage interest rates. The table includes all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013, with *Post* capturing households closing on a home on May 1, 2013 or later. The MWA is estimated using the 2SLS approach from equations (14) and (15). Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	(1) ΔWealth '15	(2) ΔWealth '15	(3) ΔWealth '15	(4) ΔWealth '15	(5) ΔWealth '15	(6) ΔWealth '15	(7) ΔWealth '15
MTG Repaid '15	0.995***	0.995***	0.995***	0.995***	0.996***	0.995***	0.995***
	[0.93,1.07]	[0.91,1.08]	[0.90,1.09]	[0.88,1.12]	[0.86,1.13]	[0.90,1.09]	[0.87,1.12]
	(28.00)	(22.07)	(20.10)	(16.15)	(14.18)	(20.24)	(15.62)
Amortization	0	30	40	60	70	50	50
Mortgage rate	N/A	4.5	4.5	4.5	4.5	6	3
IV	Post(buy)	Post(buy)	Post(buy)	Post(buy)	Post(buy)	Post(buy)	Post(buy)
F-Stat	1,420.8	902.3	750.9	485.8	373.6	761.2	454.4
Obs	42,468	42,468	42,468	42,468	42,468	42,468	42,468
R^2	0.347	0.337	0.335	0.332	0.332	0.335	0.332

III.C. Alternative samples

To arrive at our baseline estimates, we apply a number of data filters. In particular, we require that a household's wealth cannot change by more than €100k and its mortgage balance not by more than 30% in a given year. Table A.5 explores the sensitivity of our estimates to these two filters. Columns (1) and (2) include households that saw a large change in their wealth or mortgage balance, while Column (3) applies no filters at all. The table indicates that this has no significant quantitative effect on our estimates. The MWA is large and close to one.

Table A.5 Alternative samples

The table explores the effect of our data filters, in particular the dropping of households with large changes in their wealth or mortgage balance. The table includes all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013, with *Post* capturing households closing on a home on May 1, 2013 or later. The MWA is estimated using the 2SLS approach from equations (14) and (15). Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	IV (1) ΔWealth '15	IV (2) ΔWealth '15	IV (3) ΔWealth '15
MTG Repaid '15	1.051^{***} [0.917,1.185] (15.38)	0.983^{***} [0.862,1.105] (15.86)	1.000^{***} [0.920,1.080] (24.52)
Include large ΔWealth	Y	-	Y
Include large MTG Repaid	-	Y	Y
Include all	-	-	Y
IV	Post	Post	Post
F-Stat	229.9	143.3	35.1
Obs	42,666	44,555	113,231
R-sq	0.241	0.448	0.944

IV. Covariate balance

In this sub-section we evaluate whether households buying before or after the reform were balanced. First, we look at a number of observable characteristics. Second, we look at the extensive margin, that is whether the probability of buying a house was different after the reform.

Figure A.5 explores whether households buying before or after the reform look balanced on a number of observable characteristics using the purchase-month-cohort approach from equation (12). Apart from looking at differences by purchase-cohort, the figure also looks at differences between first-time home buyers (FTHBs) and non-FTHBs. Panel A. looks at mortgage amortization. This mirrors Figure 2, Panel A and shows that non-FTHBs were less affected by the reform. Panel B looks at the home purchase prices, which is flat around the reform. There is no evidence that the reform caused households to buy cheaper houses. Further, FTHBs and non-FTHBs are on similar trajectories. Panels C and D look at liquid savings, either in logs or scaled by the mortgage balance. Both are flat around the reform and FTHBs and non-FTHBs are on the same trajectories. The same holds for the level of liquid assets in 2010 (in logs) in Panel E. Panel F looks at gross income in 2010 (in logs). Here a downtrend is visible. Cohorts buying after the reform have lower income. However, there is no discontinuous difference around the reform, and there is a similar trend (though smaller) for non-FTHBs, suggesting this pattern is not driven by selection. Further, if anything, we would expect households with a higher level of income in 2010 to save more after 2012. This would artificially lower, not increase, the MWA. Panels (G) and (H) look at the change in liquid savings and income between 2010 and 2011. Both are flat around the reform and FTHBs and non-FTHBs are on the same trajectories. In sum, households buying before and after the reform, especially if they buy right around it, look balanced on observables.

Figure A.6 looks at the extensive margin. Table 3, Column (9) tests whether households having a life-event before or after the reform differ in the likelihood of owning a home in 2016. There is no difference, neither economically or statistically. Figure A.6 performs this analysis cohort-by-cohort and shows that there is also no difference for households buying right around the reform.

Figure A.5 Covariate balance

This figure performs the same purchase-month cohort analysis from equation (12) for large set of observables, for both first-time home buyers (FTHBs) and non-FTHBs. Confidence intervals are based on standard errors clustered at the household level.

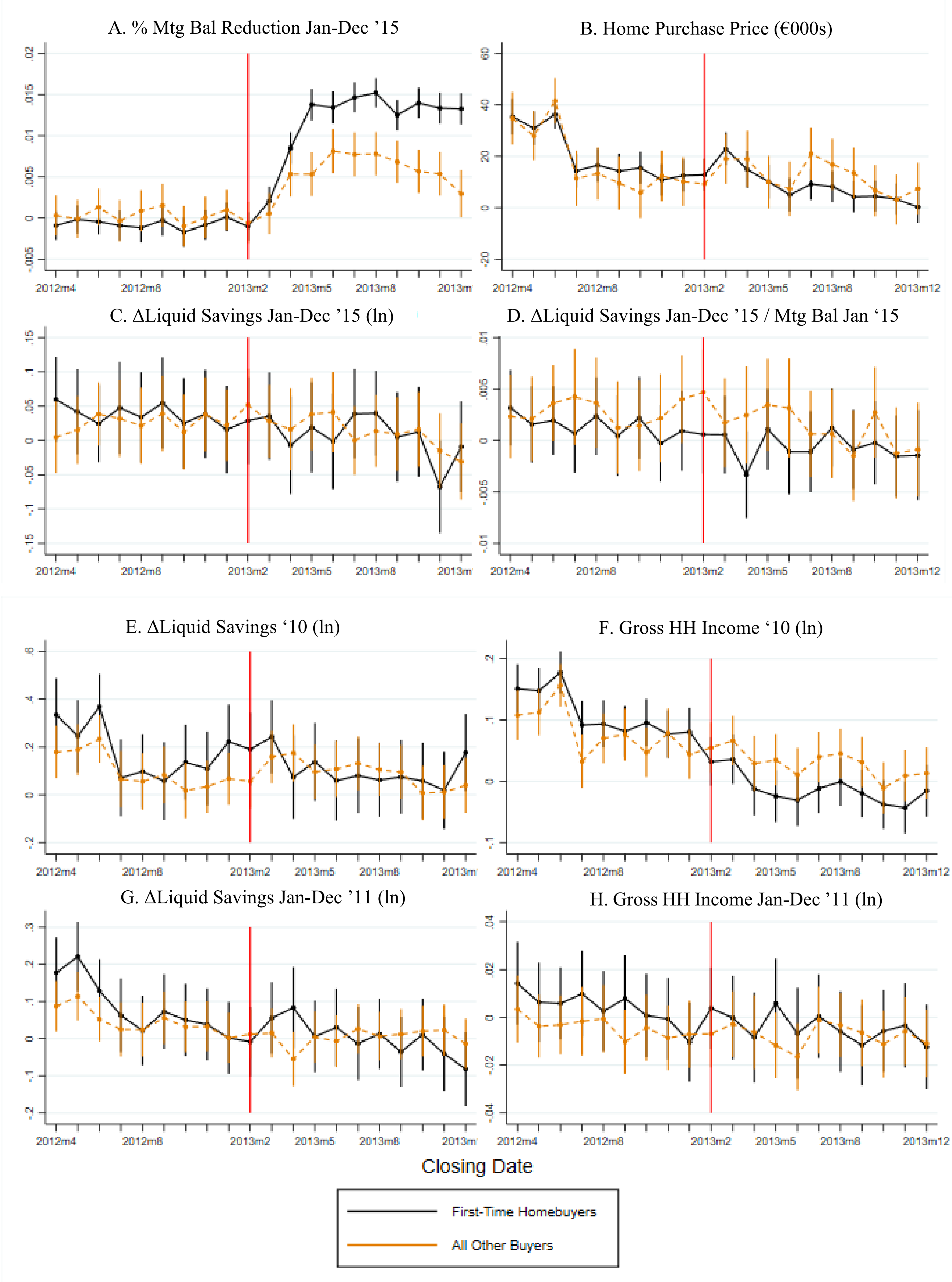
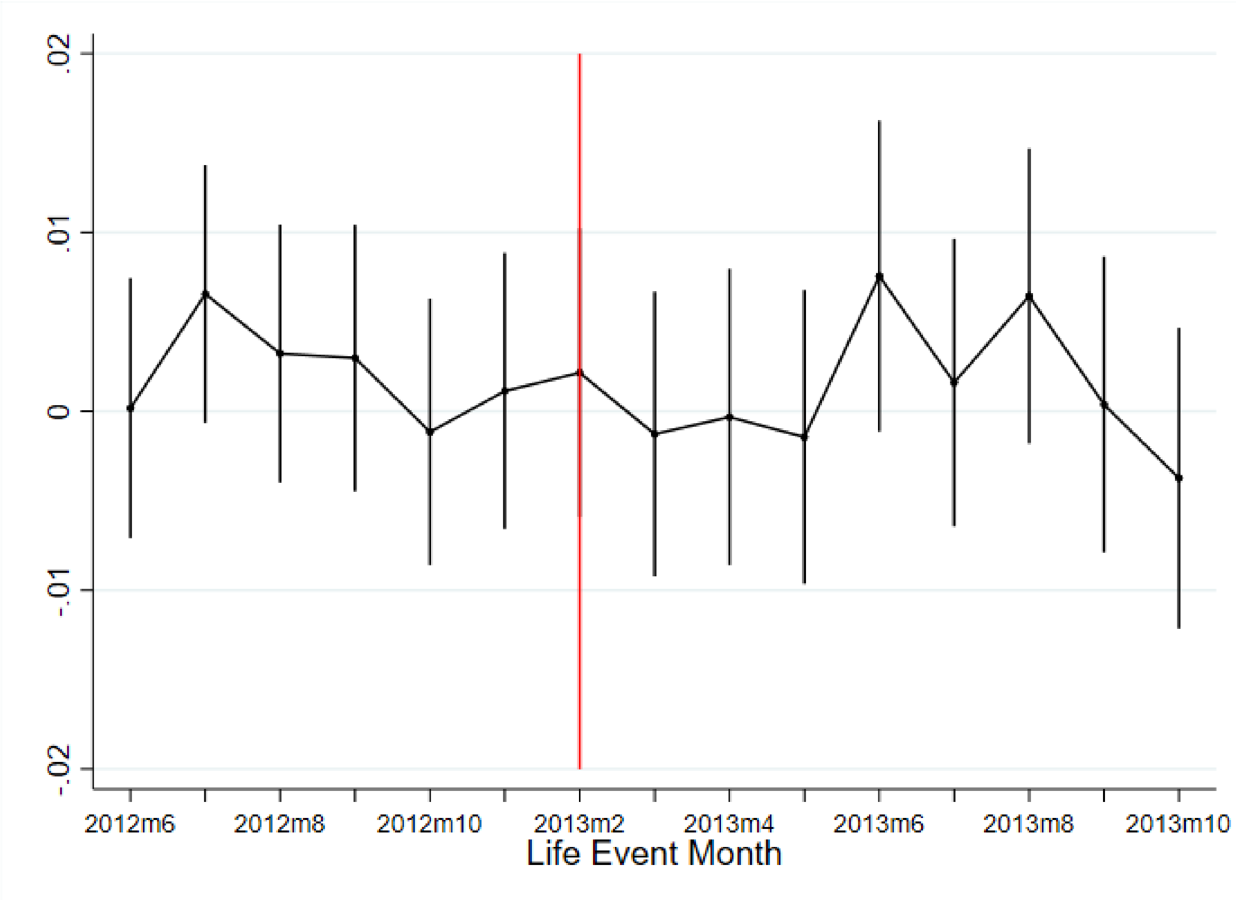


Figure A.6 Probability of Home-ownership by 2016 for all households with a life-event in 2012-2013

This figure explores the effect of life-events on the probability of owning a house by 2016. We look at all households with a life-event between 2012 and 2013 who do not who a home at the end of 2011. We do not require them to become a first-time homebuyer during this period. Of the households in this sample, 16.9% own a home at the end of 2016. We regress a dummy variable equal to 1 if they own any real estate by the end of 2016 on the month of the life-event. Life-events are defined to be months with changes in the number of members of a household (e.g. birth of a child). The vertical lines show 95% confidence intervals which are based on standard errors clustered at the household level



V. Additional evidence on the labor, investment and consumption response

V.A. Labor-supply: extensive margin

Table A.6 explores whether households buying after the reform differentially changed their labor supply on the extensive margin compared those buying before. Column (1) shows that the average number of earners per household, 1.28 in our data, goes up by 0.024 for those buying after. This increases to 0.030, with an average of 1.69 in our data, when we restrict the sample to households with at least two working-age adults (defined as age 25 to 65). Column (3) shows that the fraction of single earner households, 0.27 in this sub-sample, declines by 0.022. This increases to 0.15 when we only consider households who see a change in their single earner status.

Table A.6 Labor Supply: # of HH Earners

This table looks at the effect of the reform on the number of earners within a household. Columns (1) and (2) have the change in the number of earners in a household between 2012 and 2015 as dependent variable, Columns (3) and (4) a dummy for whether a household has a single earner. An "earner" is somebody working more than 10 hours per week. Column (1) includes all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013. *Post* captures households closing on a home on May 1st, 2013 or later. Columns (2) and (3) limit the sample to households with at least two members in working age (defined as 25-65), Column (4) to the subset of households who see a change in their single earner status. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	(1) Δ#HH Earners '15-'12	(2) Δ#HH Earners '15-'12	(3) Single Earner HH '15-'12	(4) Single Earner HH '15-'12
Post	0.0239***	0.0299***	-0.0223***	-0.146***
	(3.36)	(2.65)	(-2.60)	(-2.65)
≥2 Working Age in HH	N/A	Y	Y	Y
ΔSingle Earner	N/A	N/A	N/A	Y
Obs	42,468	24,424	24,424	3,805
R^2	0.001	0.001	0.001	0.005
Mean Dep. Var. '12	1.38	1.69	0.27	0.48

V.B. Differential returns

Table A.7 evaluates whether households purchasing after the reform had different returns on their savings. We first consider liquid savings. There is no administrative data on capital gains, but there are various ways in which we can evaluate whether those buying before or after the reform experienced differential returns. Column (1) looks at whether a household has any stocks or bonds in 2015. In our sample, this is only the case for around 8% of households, and this does not differ between those buying before or after the reform. Column (2) looks at the amount invested in stocks and bonds. To accurately quantify the effect, we estimate the IV specifications from equations (14) and (15). The effect is economically small and statistically insignificant. Column (3) looks at estimated capital gains, where we take the change in the value of all liquid savings between 2014 and 2015. As long as people do not fully consume capital gains, this is a meaningful estimate. Column (4) looks at households' income from liquid savings (interest and dividends). For both, the effect is close to zero. Together, columns (1)-(4) do not suggest that households buying before or after the reform had different returns on their liquid savings.

Next, we consider illiquid savings, in particular mortgage interest payments. Because households buying after the reform have a lower mortgage balance, their mortgage interest payments are expected to be lower. In the very beginning of the mortgage, there should not be a difference yet, but this will grow over time as households buying after the reform amortize more. To clearly show this, we look at the change in mortgage interest payments between 2014 and 2015. The results in Column (5) indicate a meaningful difference. The estimate indicates that around an additional 3.8% of the additional amortization amount is covered by lower interest payments between 2014 and 2015. This grows over time. Calculating this back to levels (rather than changes) suggests that in 2015, a household saved around 8% of the additional amortization in the form of lower interest payments.

Table A.7 Differential returns

This table looks at whether households buying before or after the reform had differential returns on their liquid or illiquid (mortgage balance) savings. The table includes all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013. Columns (1) through (4) focus on returns on liquid savings. In column (1), we estimate a linear probability model with OLS predicting whether households have any liquid savings invested in stocks and bonds. In columns (2)-(4), we implement the IV estimates from equations (14) and (15), where we instrument mortgage repayment in 2015 with *Post*, a dummy for whether households close after March (May) 1st, 2013. Column (2) has the amount of liquid savings invested in stocks or bonds in 2015 as dependent variable, column (3) estimated capital gains between 2014 and 2015 (based on the value of liquid assets at year-end), and column (4) interest and dividend income in 2015. Column (5) focuses on returns on illiquid savings (mortgage balance). We use the same IV approach with the change in total mortgage interest payments between 2014 and 2015 as dependent variable. Below the coefficient estimates, we report t-statistics based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	OLS (1) Stocks & bonds '15 (dummy)	IV (2) Stocks & bonds '15	IV (3) Capital gains (est.) '14-'15	IV (4) Int. & div income '15	IV (5) ΔMTG interest '14-'15
Post	0.004				
	(1.08)				
MTG Repaid		0.034	0.003	-0.005	-0.038***
'15		[-0.030,0.098]	[-0.013,0.019]	[-0.010,0.001]	[-0.061,-0.014]
		(1.03)	(0.38)	(-1.57)	(-3.14)
IV		Post	Post	Post	Post
F-Stat		369.3	369.3	369.3	369.3
Obs	42,468	42,468	42,468	42,468	42,468
R^2	0.000	0.006	0.001	-0.033	0.016

V.C. Car purchases

Table A.8 analyzes the effect of the reform on car expenditures. We consider expenditures net of the value of trade-ins. Since car purchases are an infrequent event, we look at the full time period between 2013-2017. The data is based on car registrations that provide information about a car's key characteristics, in particular type (e.g. car vs truck), weight, fuel type, engine size, and age. The data mixes actual purchases with leases (usually paid for by employers) and we link car registrations to household income statements to separate the two. We map characteristics into an estimated price using information from the subset of leases. We first take the full sample of car registrations in 2015 and filter out the leases. From the income statements we can infer the leased cars' catalogue value. We then estimate the relation between characteristics (size, age, type, and fuel type) and catalogue value and extrapolate this to the car registrations in our particular sample. Finally, we impute the current value of a car assuming an annual depreciation of 9 or 10.5% for small and large cars, respectively. Columns (1) takes the full sample and shows that those buying after the reform spent around €600 less on cars; this includes leases. The effect increases to around €675 in Column (2) where we omit all individuals with a car lease. Column (3) shows that the effect on the extensive margin is small and only statistically significant at the 10% level, indicating that the drop in expenditures is driven by households purchasing cheaper cars. Column (4) uses the IV strategy from equations (14) and (15) to quantify what fraction of the additional amortization is covered by declining car expenditures. This amounts to around 11%.

Table A.8 Car purchases

This table looks at the reform's impact on car purchases. Column (1) includes all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013, that we can link to the car registration records. Columns (2) through (4) omit all individuals who have a (employer sponsored) car lease. Columns (1) and (2) provide reduced form estimates, looking at expenditures net of the value of trade-ins, while column (3) looks at the likelihood of buying a car. Column (4) provides IV estimates following equations (14) and (15), using a dummy for house closings on May 1st or later as instrument. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

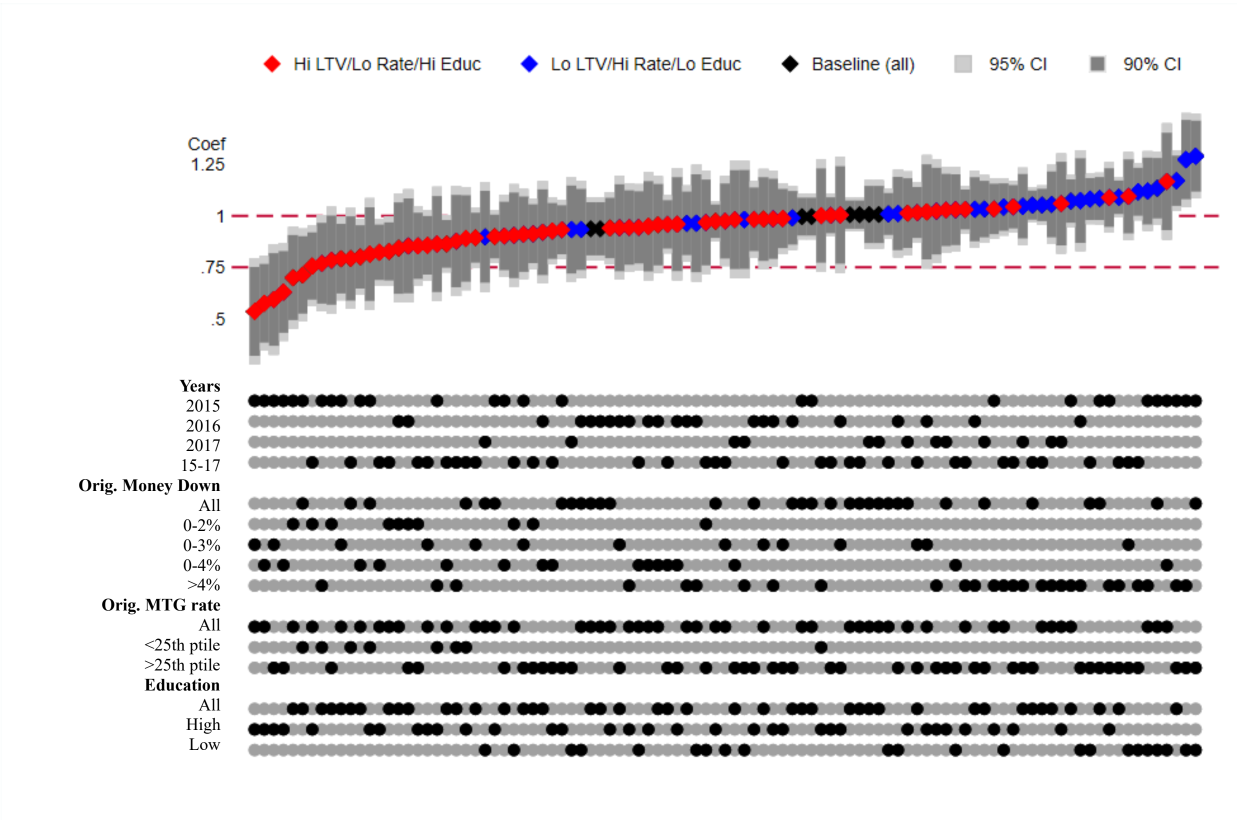
	RF (1) Net Car Buys € '13-'17	RF (2) Net Car Buys € '13-'17	RF (3) Net Car Buys # '13-'17	IV (4) Net Car Buys € '13-'17
Post	-598.9^{***}	-676.1^{***}	-0.0244^{*}	
	(-3.42)	(-3.94)	(-1.88)	
MTG Repaid '13-'17				-0.107^{***}
				[-0.162,-0.0532]
				(-3.88)
Leases	Y	N	N	N
IV	-	-	-	Post
F-Stat				587.4
Obs	33,732	27,467	27,467	27,467
R^2	0.001	0.001	0.000	-0.035

VI. Heterogeneity

In Section IV.D and Table 7 we perform a number of heterogeneity tests. Figure A.7 provides a specification curve where we experiment with different cut-offs for how much money people put down, and different periods over which we measure amortization and wealth building. Further, we combine different sample selection criteria. The figure shows that our results our robust to definition and specification choice.

Figure A.7 Specification Curve Heterogeneity Analysis

This figure presents a specification curve where we experiment with different cut-offs for how much money people put down, and different periods over which we measure amortization and wealth building. Further, we combine different sample selection criteria. *Years* indicates over which period we estimate the MWA, *Orig. Money Down* indicates the fraction of the house price was paid down at purchase, *Orig. MTG Rate* indicates whether a mortgage's interest rate, when originated, was in the lowest 25[th] percentile of the distribution in a given month or not, and *Education* indicates whether at least one household member had a theoretical ("high") rather than practical degree. Red (blue) dots indicate combinations which, based on the stylized model of Section II.A, we would expect to have a low (high) MWA. Black dots indicate our baseline estimates. The vertical bars show 90 or 95% confidence intervals which are based on standard errors clustered at the household level.



VII. Effects by age and year, and descriptive statistics on long-run effects

In this section, we use the available evidence to evaluate how long the effects of additional amortization on wealth building might persist. Table A.9, Panel A estimates the MWA over different years up to 2017 when our data ends. The Table shows that the MWA is close to one, independent of what exact year we use. There is no downward trend visible. Table A.9, Panel B estimates the MWA for different age ranges, up to households with someone older than 50. The final column applies an additional filter ensuring the sample excludes cases where a grandparent lives in with the family (an uncommon situation in the Netherlands to begin with). The table consistently find a large MWA close to one, even as individuals get closer to retirement. This suggests that MWA will remain high, even as people age. Together, these results suggest that effects could be persistent.

Table A.9 Effects by year and age

Panel A provides the MWA estimated over different years between 2014 and 2017 (when our data ends). The table includes all first-time home buyers closing between October 2012 and September 2013, omitting households closing in March and April 2013. We estimate the MWA using the IV strategy following equations (14) and (15), where we instrument mortgage repayment in a given year with *Post*, a dummy for home closings on May 1[st] or later. As in Table 2, columns (5) and (6), we omit the post-reform cohorts of August and September 2013 for the 2014 estimate. The estimate for 2015 is the same as in Table 2, column (3). Panel B table replicates the MWA estimate from Table 2, column (3), for different age groups, imposing a minimum age (30, 50 or 50) on the oldest household member. Column (4) aims to omit households with a grandparent living in by excluding households with age differences of more than 20 years. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

Panel A. Effects by year

	IV (1) ΔWealth, '14	IV (2) ΔWealth, '15	IV (2) ΔWealth, '16	IV (3) ΔWealth, '17
MTG Repaid 'year	0.924^{***}	0.997^{***}	0.943^{***}	1.023^{***}
	[0.82,1.03]	[0.88,1.10]	[0.79,1.10]	[0.85,1.19]
	(17.86)	(17.74)	(12.09)	(11.85)
Year	2014	2015	2016	2017
IV	Post	Post	Post	Post
F-Stat	247.4	369.3	270.3	202.6
Obs	32,804	42,468	39,312	34,634
R^2	0.557	0.330	0.401	0.409

Panel B. Effects by age

	ΔWealth, '15	ΔWealth, '15	ΔWealth, '15	ΔWealth, '15
MTG Repaid,	0.992^{***}	1.080^{***}	1.081^{***}	1.266^{***}
'15	[0.866,1.119]	[0.871,1.289]	[0.701,1.460]	[0.756,1.776]
	(15.38)	(10.11)	(5.59)	(4.87)
Age	>30	>40	>50	>50
Grandparent filter	N	N	N	Y
IV	Post	Post	Post	Post
F-Stat	274.2	105.0	40.6	25.2
Obs	34,185	15,668	6,416	5,268
R^2	0.325	0.298	0.289	0.179

Next, we evaluate (less-well) identified evidence from aggregate statistics to gauge the extent to which households might undo the additional amortization over time. We compare the Netherlands, where pre-2013 mortgages had a substantial interest-only component, with the U.S., where most mortgages are fully amortizing. Figure A.8 focuses on Dutch and U.S. homeowners with relatively little liquid assets ("hand-to-mouth") who likely have the highest demand to extract home equity. Panel A shows that the homeownership rates increase faster with age in the Netherlands than in U.S., suggesting that Dutch households have more time to repay their mortgage. Nevertheless, Panel B shows that that the fraction of households who still have an outstanding mortgage is stable around 95% for all age bins in the Netherlands, and is not declining with age as in the U.S.. This suggests that Dutch households do not typically save money in other accounts to repay their mortgage, while U.S. household do not typically extract home equity to undo their mortgages' full amortization schemes.

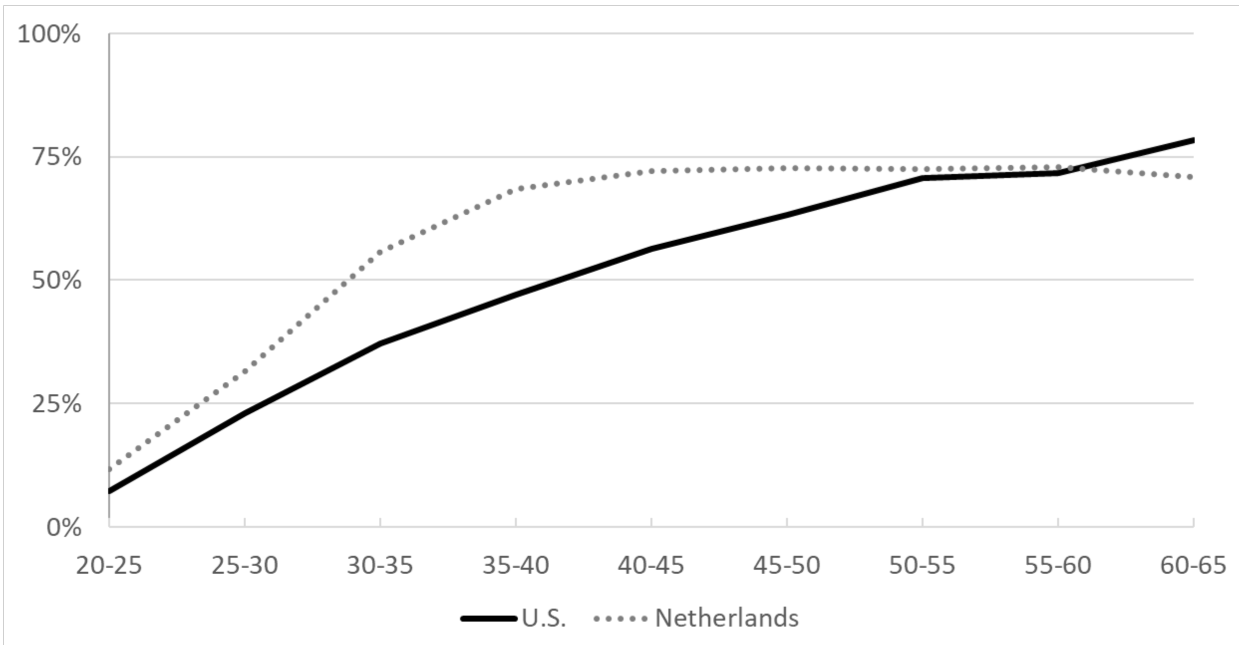
This pattern is supported by evidence from the Panel Study of Income Dynamics (PSID) in the U.S.. Fully amortizing 30-year mortgages repay on average 3.3% of principal per year (excluding the final year). Among U.S. non-movers in the PSID who own a home in concurrent (biannual) waves and have a mortgage in the prior wave, the average (median) mortgage balance falls by 4.4% (4.6%). This evidence is consistent with prior literature in public economics noting the surprising lack of home equity withdrawal following moves even among the elderly (e.g., Venti and Wise 1989).

In sum, households in the Netherlands and the U.S. seem to closely follow the amortization schedules during their life-cycle, neither undoing it through extra mortgage debt repayment from other sources, nor through extracting home equity. Though there are other differences between the U.S. and the Netherlands, this suggests that higher amortization requirements can have persistent effects on wealth accumulation.

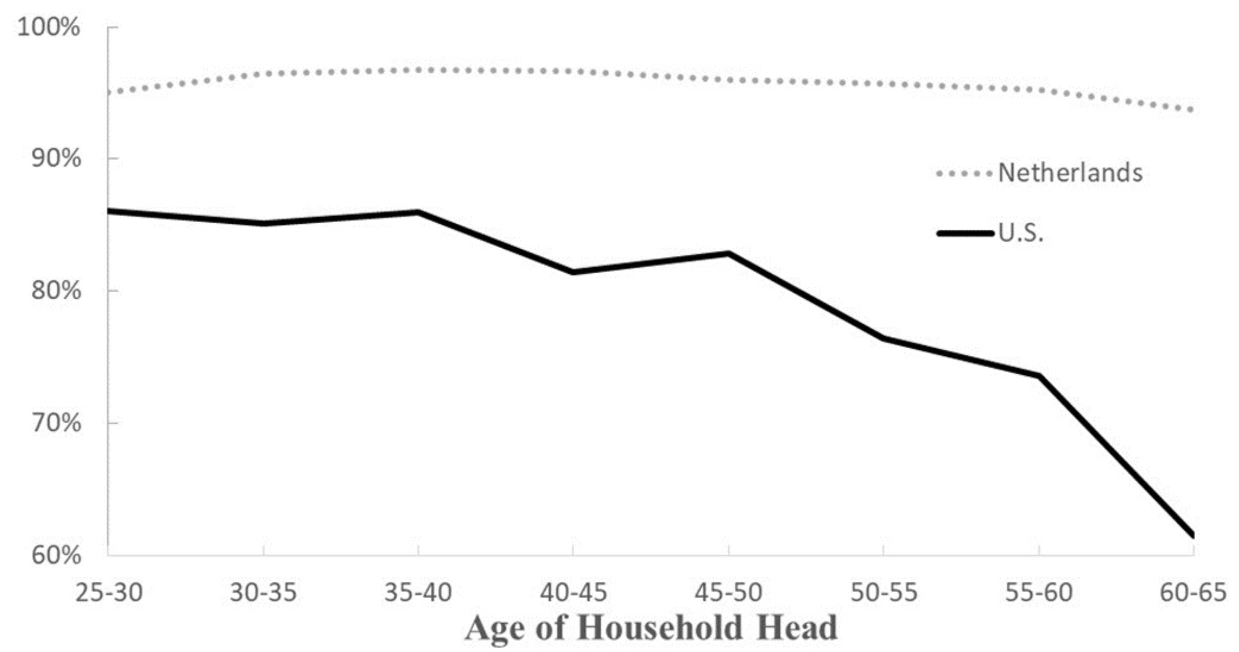
Figure A.8 Dutch vs. U.S. Homeowners by Age

This figure compares the likelihood of households still having a mortgage in later life-stages between the Netherlands and the U.S.. Panel A shows the percentage of household heads who report having real estate by five-year age group categories between 20 and 65. Panel B reports the percentage of homeowners that are "hand-to-mouth" – those without significant levels of liquidity (<\$10k/€7K) who have an outstanding mortgage balance remaining – for the same age groups as in Panel A. Data on U.S. households (solid black line) comes from the 2016 Survey of Consumer Finances, while data for Dutch households (dotted gray line) comes from CBS as of 2012.

Panel A. Homeownership Rate



Panel B. Percentage of Homeowners w/ an Outstanding Mortgage Balance Remaining



VIII. Effect of the reform on mortgage interest deductibility (MID)

By increasing amortization requirements, the reform reduced the net-present value (NPV) of future MID savings. In this section, we evaluate whether this could drive (part of) our results.

Given the convex shape of amortization in a standard annuity mortgage, most of the difference in MID between a 50% interest-only (IO) and a fully amortizing mortgage accrues later in the life of a mortgage. Around the time of the reform, there was substantial uncertainty about future MID. The Dutch Council of State expressed concerns that the reform would lead to an unjustifiable unequal treatment of FTHBs and non-FHTBs.[22] Moreover, the reform was the first substantial change in the Dutch MID regime in decades, suggesting more restrictions were to follow.[23] As a result, households buying before the reform may have expected to lose part of the MID as well. This suggests that the NPV of expected future MID savings was not dramatically different between those buying before or after the reform. In Figure 2, Panel B, we find no evidence of bunching around the 2013 reform, but we do around a possible increase in the transaction tax in June 2012. This confirms that households did not interpret the 2013 reform as a wealth shock similar to the June 2012 transaction tax.[24] Further, we find no effect of the reform on liquid savings, which appears inconsistent with a substantial change in (perceived) life-time wealth.

The differences in MID savings in the short run, over which there was substantially less uncertainty, is small, and are unlikely to drive our results. For an average mortgage of €203k with an interest rate of 4.5%, the difference in MID savings, evaluated at a 42% marginal tax rate, is on average 66 euros per year during the first five years of the mortgage. This is only 3.3% of the additional amortization amount and it seems unlikely that this has a direct impact on current saving decisions.

Nevertheless, we do two things to evaluate whether this effect could increase our MWA estimates. First, we use the convexity of the amortization schedule of an annuity mortgage to estimate the MWA with an alternative delta-in-delta method. The reform means than the additional amortization amount increases year-by-year. We can therefore relate the change in the mortgage repaid to the change in wealth accumulation. The 2SLS square analysis from Equations (14) and (15) can be rewritten as:

$$\Delta\Delta M_{2017-2014,i} = \delta\, 1_{Post,i} + \eta_i \quad \text{(A1)}$$

$$\Delta\Delta S_{2017-2014,i} = \gamma \widehat{\Delta\Delta M}_{2017-2014,i} + u_i \quad \text{(A2)}$$

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[22] *Advies Raad van State betreffende wijziging van de Wet inkomstenbelasting 2001 en enige andere wetten in verband met de herziening van de fiscale behandeling van de eigen woning (Wet herziening fiscale behandeling eigen woning)*, 10 September 2012.

[23] In fact, starting in 2014, the maximum marginal tax rate at which people could deduct interest payments was reduced by 0.5% each year until it would reach the tax rate of the lowest tax bracket. In October 2017, a new government decided to speed this up to 3% per year.

[24] Note that the uncertainty about future MID does not mean that we would expect households to keep contracting 50% IO mortgages after the reform, because these would lose MID on the full mortgage balance with certainty for the foreseeable future.

where $\Delta\Delta M_{2017-2014,i}$ and $\Delta\Delta S_{2017-2014,i}$ and the differences in mortgage repayment and wealth accumulation, respectively, between 2017 and 2014. The instrument is the same as before. Again, γ gives the MWA. The estimates in Table A.10 indicate that the MWA is also large and close to one in this alternative specification.

Second, we take a closer look at our sample of non-first time home buyers (non-FTHBs). These households were grandfathered under the old rules but only for the existing mortgage balance and for the remainder of the (30 year) maturity. This means than non-FTHBs with a shorter maturity on their existing mortgage saw a larger decline in the NPV of future MID. Figure A.9 implements the purchase-cohort-month analysis from equation (12) for non-FTHBs, adding a comparison between those with a remaining maturity of more or less than 10 years. The figure finds that the response to the reform is the same for both groups. Amortization is 1% higher for both groups in 2015, while liquid savings are unchanged. This suggests that the loss of future MID savings is not affecting our estimates.

Table A.10 Convexity of the Amortization Schedule

This table uses a delta-in-delta IV approach to estimate the MWA. The table includes all first-time home buyers closing between August 2012 and July 2013. We only include cases that appear consistent with the mortgage's amortization schedule (i.e. we exclude cases in which mortgage repayment increased or decreased dramatically by more than €20k). Further, we exclude outliers in ΔΔWealth,'17 − '13 that are larger than €30k. We estimate the MWA using the IV strategy following equations (A1) and (A2), where we instrument the change in mortgage repayment in a given year with *Post*, a dummy for home closings on March 1st or later. As in Table 2, columns (5) and (6), we omit the post-reform cohorts of August and September 2013. Below the coefficient estimates, we report t-statistics and 95% confidence intervals based on standard errors clustered at the household level. ***, **, and * indicate statistical significance and the 1%, 5% and 10% level, respectively.

	IV (1) ΔΔWealth,'17 − '13
ΔMTG Repaid, '17 − '13	1.155**
	[0.18,2.13]
	(2.32)
IV	Post
F-Stat	15.9
Obs	25,001
R^2	0.144

Figure A.9 Non-FTHBs: effect by remaining mortgage maturity

This figure implements the purchase-month cohort analysis of equation (12) for non-FTHBs, differentiated by whether the remaining maturity on their mortgage is more or less than ten years, looking at different outcome variables.