

The Mortgage Piggy Bank: Building Wealth through Amortization

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Abstract

Mortgage amortization schedules are illiquid savings plans comparable in size to pension programs; however, little is known about their effects on wealth accumulation. Using individual administrative data and plausibly exogenous variation in the timing of home purchase (ex. childbirth-driven) around a 2013 Dutch reform, we find a near one-for-one rise in net worth for each dollar of amortization. Households leave other savings and liabilities unchanged, and instead increase labor supply and reduce consumption. Effects hold even for regular savers and older households. This has important macroprudential implications and suggests homeownership financed via amortizing mortgages is instrumental for household wealth building.

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"One nice thing about investing in a house is that you're committed to a mortgage payment. So if you don't take out a home equity line of credit or do something like that, you will accumulate wealth."

Nobel Laureate Robert Shiller (CNN Dec 4th, 2014)

When households purchase a home with a standard mortgage contract, they not only sign-up for a loan, but also a periodic debt repayment or amortization plan. These plans are designed to build-up substantial illiquid savings in the form of home equity prior to maturity of the loan, akin to a "mortgage piggy bank." Amortization plans are ubiquitous in most countries and typically not only substantial for each individual borrower, but also at a macroeconomic level. For example, in the U.S., households contribute hundreds of billions of dollars each year to mortgage amortization plans, which make them comparable in size with other major illiquid savings contributions, such as pension programs.¹

In this paper, we examine the effect of mortgage amortization on wealth accumulation. If households act as if mortgage repayments and non-mortgage savings are fungible, then there will be no effect on wealth accumulation – increases in mortgage repayments will perfectly crowd-out other savings. If, on the other hand, they are not fungible, then amortization could lead to substantial household wealth accumulation. While there is a broad literature on the effects of pension programs on savings and wealth accumulation (e.g. Poterba et al. 1995, 1996; Madrian and Shea 2001; Chetty et al. 2014; Beshears et al. 2019; Choukhmane 2019), there is no causal evidence on the effects of mortgage amortization.

Empirical evidence on the marginal wealth building from amortization, *MWA*², is critical for our understanding of the underlying mechanisms that alter household savings decisions, the impact of macroprudential policies, and the importance of homeownership for household wealth accumulation, retirement savings, and inequality. For example, if households compensate for increased debt repayments by reducing their non-mortgage savings, policies intended to encourage building up home equity could actually hurt financial stability. By contrast, if households do not treat mortgage amortization and non-mortgage savings as fungible, such policies could improve stability. Moreover, encouraging homeownership financed with amortizing mortgages could stimulate wealth accumulation.

The empirical identification of *MWA* is challenging though. Households endogenously select into homeownership and their choice of mortgage contract. Renters are unlikely to be a valid counterfactual for households able and willing to buy a home.³ The existing literature also shows that households who limit

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).

² The marginal dollar of wealth accumulation for a dollar increase in mortgage amortization.

³ Older households often have substantial home equity, but little liquid savings (ex. Kaplan et al. 2014). This could suggest a high *MWA*, but could also reflect that households with housing wealth might need little other savings.

amortization by taking up interest-only (IO) or alternative mortgage products (AMPs) often differ systematically in terms of their liquidity constraints, financial sophistication, savings preferences, and future income expectations (Cocco 2013; Cox et al. 2015; Kuchler 2015).

In this paper, we overcome these challenges and provide the first empirical evidence on the causal effects of mortgage amortization on wealth accumulation. We use individual administrative data to examine the January 2013 implementation of a mortgage reform in the Netherlands aimed at improving financial stability. 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 required monthly debt repayments. This novel quasi-experiment provides a unique opportunity to examine the role that mortgage amortization plays in wealth accumulation. Not only does the regulatory change provide plausibly exogenous variation in amortization schedules for FTHBs, our administrative data gives us precise measures of household wealth and its decomposition for every person reporting taxes in the Netherlands from 2006 to 2016. A key advantage of our setting is that virtually all homeowners were “compliers” both before and after the reform. As such, our results apply to the broader population, rather than a particular subset.

We compare all FTHBs with a mortgage right around the implementation of the reform and find little-to-no difference in non-mortgage savings, despite a significant increase in observed debt repayment. This holds even five years after the reform when, for the average treated household, the additional mortgage debt repayment over this period exceeds the stock of liquid assets. This implies a near one-for-one rise in net worth – a response consistent with little savings-debt repayment fungibility ($F \sim 0$) and a substantial effect of amortization on wealth accumulation ($MWA \sim 1$). We find that around $1/4^{\text{th}}\text{-}1/3^{\text{rd}}$ of the increased wealth accumulation is financed with higher future household labor income. This is driven entirely by increases in household hours worked, which exhibit no differential pre-trends. The remainder comes from a reduction in non-housing expenditures. We find no differences between observed and predicted (based on contract terms) amortization over this period, suggesting little “leakage” of treatment via differential home equity withdrawals or prepayment for those buying before vs. after.

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 (ex. January to December 2015). Differences in wealth accumulation are smooth and flat as a function of mortgage age before the reform, then jump up suddenly and persistently the month the reform takes effect. This indicates that results are not driven by differences in mortgage age. The reform was based on the time of going under contract on a house purchase and not the closing date, which typically takes at least two months. Therefore, households closing on their properties in January and February of 2013 were unlikely to be affected by the reform, while those closing in March

and April 2013 were. We find similar effects comparing households closing in this narrow four-month window.

This evidence is strongly supportive of a causal interpretation of our findings. Nevertheless, a key remaining concern for our identification is heterogeneous sorting. That is, our estimates would be biased if FTHBs strategically timed their home purchase to avoid the reform, *and* if this behavior is systematically correlated with later savings decisions (ex. those who buy earlier intend to save less).

First of all, we find no evidence that the reform affected the timing of home purchase. Even prior to the reform, payment-to-income (PTI) requirements were computed as if the loan was fully amortizing over 30 years, even if it was not. As such, the reform did not change the maximum mortgage a household could get based on its income. This likely explains why there is no apparent bunching in the number of transactions in the months prior to the reform, and no change in the probability and timing of home purchase after experiencing a life-event (ex. birth of a child).⁴ The reform also did not lead households to buy less expensive homes – average loan-to-income (LTI) ratios were unaffected by the reform.

Second, we find no systematic differences in the *observable* characteristics of households purchasing a home before or after the reform. House purchase values, origination LTV and LTI ratios, income, and income growth are smooth around the reform, and do not have differential pre-trends. Moreover, in the years before the reform, wealth and wealth growth are the same for those buying before vs. after. To confirm there are no systematic differences, we compare FTHBs with non-FTHBs who were partially grandfathered in under the old mortgage rules. This means that the jump in mortgage repayment was substantially smaller for the latter (even conditional on buying in the same month). We find that, relative to non-FTHBs, the change in observables for FTHBs is smooth across the reform.

Though suggestive, it is still possible that there was heterogeneous sorting on *unobservables*. We therefore focus on FTHBs who also had a “life-event” in the months around the reform. The high-quality administrative data in the Netherlands lets us identify the exact month when there are changes in the number of members of the household, such as the birth of a child. We show that the timing of a life-event is a strong predictor of the timing of home purchase and is unrelated to pre-reform household income, changes in non-mortgage savings, wealth accumulation, and house price appreciation after purchase. Using the month of a life-event as an instrumental variable confirms that increases in mortgage debt repayments are matched one-for-one with changes in wealth accumulation. This change is sudden and persistent. Given the implausibility of households timing life-events to avoid the reform, we conclude that there was indeed a causal effect on wealth accumulation consistent with an *MWA~I* for FTHBs.

⁴ In contrast, Best et al. (2018), DeFusco et al. (2020), and Backman and van Santen (2020) show that when regulatory constraints on leverage bind or involve interest-rate jumps, substantial bunching can occur. The evidence in this paper and Van Bekkum et al. (2019) shows that Dutch FTHBs tend to borrow near the regulatory limit.

Apart from selection, it is possible that the reform changed the supply-side of the Dutch mortgage market. Given the low default rates in the Netherlands and the wide use of floating rate mortgages, it is not obvious that supply effects are important. The lack of sorting around the reform suggests there were no sudden changes in screening by lenders. Furthermore, we find no differential change in mortgage rates around the reform for FTHBs, suggesting that other supply effects were limited.

Another concern for identification is that our treatment could be confounded by other major changes around the reform which also alter wealth accumulation. There could be seasonal effects if early year buyers have systematically different wealth accumulation patterns. That does not appear to be the case. Effects are persistent and hold for households who bought at the end of 2013, while there is no effect for those buying in the same months of the year in 2012.

A remaining concern is that the reform had liquidity and wealth effects that could explain our results. In particular, by forcing households to pay down more of their mortgage, the reform effectively reduces mortgage interest deductibility (MID) for those buying after. This means a reduction in future liquidity and life-time wealth, and a greater need to save.⁵ The complete absence of any effects on non-mortgage savings in our setting suggests this is a minor concern – it seems implausible that liquidity or wealth effects would lead to an increase in wealth accumulation via increased mortgage repayment, but no change at all in non-mortgage savings. Nevertheless, we take additional steps to evaluate the importance of the reduction in MID.

First, we show that *liquidity* effects are unlikely to explain our results. Our estimates of *MWA* are consistent across years looking at wealth accumulation in 2014, 2015, and 2016 separately. If liquidity effects were important, we would expect to see substantial differences between those years. In 2014, mortgages were barely amortized and differences in MID between those buying before or after the reform would have been minor. Yet, we still find a parameter not different from 1.

Next, we show that *wealth* effects are unlikely to explain our results. In a standard life-cycle model where households smooth consumption over time, a one-time reduction in life-time wealth should lead to a one-time permanent reduction in consumption. To make sure that such a response is not driving our results, we take advantage of the typical structure of the mortgage amortization schedule: mortgage repayment increases over the life of the mortgage. This allows us to compute the increase in the mortgage debt repayment within a given household over 2014 vs. 2016 and relate it to the change in household wealth (delta-in-delta). Again, we find a parameter estimate of about 1, suggesting it is unlikely our effects are

⁵ We expect the relative wealth and liquidity effects to be small. Because of the convex amortization schedule of annuity mortgages, most of the tax benefits accrue in the future; it is unclear whether existing homeowners expected MID rules to stay the same. At the time of the 2013 reform, it was hotly debated whether the MID reduction should not also apply to existing home owners. Moreover, the mortgage reform was the first significant MID reduction in decades, and signaled more reductions to come, which were eventually implemented starting in 2014.

confounded by a one-time wealth effect coming from the reduction in MID, or for that matter, any other one-time shock occurring at the time of going under contract.

We can further evaluate the possible impact of wealth effects by focusing on non-FTHBs. The reform's grandfather clause was only partial. Existing homeowners could only benefit from the old rules for the duration of their existing mortgage – the maturity could not be extended. Therefore, any perceived negative wealth effect from the reform should be larger for those with a shorter period to benefit from the more extensive MID. Comparing non-FTHBs with a remaining maturity on their mortgage of more or less than ten years, we find no difference in wealth accumulation. We also see no increase in non-FTHBs refinancing their mortgages prior to the reform, as would be expected if there were concerns about wealth effects and therefore efforts to avoid it. This is all consistent with wealth effects playing little observable role in confounding our estimates.

Who are our results likely to apply to? We show that our estimates hold for households with substantial liquid assets, either in terms of the flow or in their overall level, so that they could easily offset their mortgage repayment by altering their non-mortgage savings. This holds either using their actual non-mortgage savings in a year, or instrumenting for that using their non-mortgage savings in the years before they bought the house. We find similar results for those with low LTV ratios, who should be more easily able to access this home equity. Moreover, effects hold across age distribution, including FTHBs more than 50 years old, and non-FTHBs. This suggests that our effects are generally applicable and not confined to young, financially constrained households far away from retirement.

Our findings are consistent with related findings in other countries, suggesting a likely applicability beyond just the Netherlands. Prior work looking at the U.S., Canada, Denmark, and Finland does not examine wealth accumulation directly, but does find that reduced mortgage repayment increases consumption and reduces labor supply, consistent with our findings. Ganong and Noel (2019) provide such evidence for households in financial distress, Scholnik (2013), d'Astous (2019), and Andersen et al. (2019) for households fully paying off their mortgages, Larsen et al. (2018), and Backman and Khorunzhina (2020) for households choosing interest-only mortgages, and Vihriala (2021) for households with an option ARM period ending.

How long do the effects last? We find no evidence of offsetting behavior in the first five years after the reform, when our data end. As noted already, effects are similar for older households and those with high non-mortgage savings. Moreover, homeowners who sell and buy a new home within five years do not

appear to cash out. This suggests effects are fairly persistent.⁶ Though not central to our analysis, we provide evidence from aggregate statistics suggesting that effects could last beyond five years.

Our work is distinct from contributions looking at the marginal propensity to consume (MPC) or save (MPS) out of shocks to housing related wealth. In and of itself, the 2011 Dutch reform had no direct effect on net-wealth and the MPS and *MWA* estimate distinct economic primitives: the MPS measures how much of an exogenous shock to wealth is saved; by contrast, the *MWA* indicates how much households save when encouraged to build-up more wealth through an amortization plan. An MPS of 1 requires no action on the part of the household since the additional wealth is just exogenously given, while an *MWA* of 1 requires a household to cut consumption and/or increase income to build home equity and wealth on their own. Nevertheless, comparing our estimate the against that of the MPS in the literature provides a number of valuable insights. First, there is a literature looking at the effects of interest rate resets (Di Maggio et al. 2017; Zator 2019).⁷ This work shows a remarkably similar response: households appear to treat interest and amortization payments as equivalent even though they are fundamentally different. This has important policy implications, which we discuss in more detail in Section 5. Second, there is a large literature looking at the effects of house price changes. This work finds that increases in net-wealth are largely retained rather than consumed.⁸ This is consistent with our finding that *MWA* remains close to 1, even five years out, and even suggests that effects persist beyond that.

We provide compelling evidence of a substantial causal effect of mortgage amortization on wealth accumulation that is not just driven by non-savers. It is unclear what potential mechanisms could plausibly cause such a large response. One possible explanation is that there is a substantial (perceived) liquidity difference between mortgage debt repayment and non-mortgage savings. Extracting home equity is generally costly, and may even be infeasible in economic downturns when house prices and incomes fall, making it a poor substitute for non-mortgage savings in bad times (DeFusco and Mondragon 2019). This may be exacerbated if households are unwilling (or unable) to tap other forms of credit (Hundtofte et al. 2018). Our finding of no response in non-mortgage savings to the rise in debt repayment does suggest a

⁶ This stands in contrast with evidence that households undo an increase in current pension contributions (driven by some intervention) by reducing future contributions (Choukhmane 2019, Wang et al. 2020). This may be because undoing amortization, through refinancing or obtaining a second mortgage, is more costly than for pensions.

⁷ There is also work looking at wealth shocks through changes in anticipatable changes in tax rebates and other payouts (ex. Johnson et al. 2006; Agarwal et al. 2007; Parker et al. 2013; Kaplan and Violante 2014; Keung 2018 ; Cookson et al. 2019). Again, MPS estimates from this work are fundamentally different from our *MWA* estimate.

⁸ Most relevant to our work are contributions looking at partial equilibrium housing wealth/equity shocks (Engelhardt 1996; Disney et al. 2010b; Cooper 2013; Leth-Petersen 2010; Cloyne et al. 2019; Bernstein 2020; Ganong and Noel 2020). There is an even broader literature looking at regional changes in house prices (e.g. Mian et al. 2013; Farrell et al. 2020; Guren et al. 2020). While these may be less apt comparisons if they include some local general equilibrium effects, results are broadly consistent with the vast majority of housing wealth increases being saved rather than consumed, even after retirement (Feinstein and McFadden 1989; Venti and Wise 1989, 1990, 2002, 2004; Thaler 1990; Skinner 1994, 1996).

rather less standard model – households appear willing to cut consumption and increase labor supply substantially today in order to avoid *any* possible reduction in marginal consumption or increase in their labor supply in the future.

In Section 5, we discuss a number of behavioral channels that might help explain our results. We consider the importance of default settings that have been shown to increase pension contributions (Madrian and Shea 2001; Chetty et al. 2014; Beshears et al. 2019), mortgage amortization as a solution to present-bias and limited commitment to save for the long run (Beshears et al. 2015; Kovacs et al. 2020; Kovacs and Moran 2020; Attanasio et al. 2020; Vihriala 2021; Schlaefmann 2020), the ease and simplicity of small monthly payments to save (Beshears et al. 2013; Hershfield et al. 2019), and mental accounting causing households to treat mortgage debt repayments as bills, not as wealth accumulation (Camanho and Fernandes 2018).

Regardless of the underlying channel, the substantial effect of mortgage amortization on wealth accumulation that we find across a broad set of household types has important policy implications.

First, our results speak to the costs and benefits of interest-only (IO) mortgages or alternative mortgage products (AMP) (Mian and Sufi 2009; Adelino et al. 2016; Hertzberg et al. 2018). Ex-ante, one might expect that households with smaller amortization amounts have more non-mortgage savings. These households would therefore be less likely to default after a shock, leading IO mortgages and AMPs to actually improve financial stability (Svensson 2019; Svensson 2020). Our results suggest that this is not necessarily the case, as households do not seem to treat amortization and non-mortgage savings as perfect substitutes.⁹ It seems likely that the rise in the use of these products in the U.S. before the Global Financial Crisis likely had a detrimental effect on these borrowers' ability to eventually repay their debts.

Second, our results speak to macroprudential policies during recessions (Piskorski and Seru 2018). Most importantly, policies that encourage contracts with countercyclical amortization (Campbell et al. 2019; Kovacs and Moran 2020) are likely to have an even bigger impact than implied by standard models. Given the size of mortgage amortization in the U.S., this effect would be economically substantial. For example, freezing mortgage amortization payments for two years would be roughly equal to the dollar amount of all TARP (Trouble Asset Relief Program) payments in the four years following the Great Recession.

Third, our findings help reconcile different findings in the literature on the causal effect of homeownership on household wealth. Kaplan et al. (2014) find that homeowners typically accumulate substantial sums of housing wealth. On the other hand, Sodini et al. (2017), exploit plausibly exogenous

⁹ Amromin et al. (2018) show that, controlling for income and credit score, people who took out AMPs in the U.S. were twice as likely to default than those with amortizing mortgages. They did not use the extra funds available with interest-only mortgages to improve their non-mortgage savings, which they could then use to prevent a costly default.

variation in homeownership funded with interest-only mortgages and find little evidence that homeowners are richer. Our results could help reconcile these seemingly inconsistent findings if the effect of homeownership on wealth accumulation is mediated through mortgage amortization.

Finally, our findings could have implications for the optimal design of retirement programs. Beshears et al. (2019) argue that, under taste shocks and present-bias, the socially optimal retirement plan has three accounts, including one with early liquidation costs. Homeownership with mortgage amortization is such an account that appears to be a critical component of household wealth accumulation. If the effects on wealth accumulation are persistent and similar for those approaching retirement, then mortgage amortization should be considered a key component of retirement programs.

The rest of this paper is structured as follows. Section 1 discusses the mortgage environment in the Netherlands. Section 2 describes the underlying data. Section 3 discussed theoretical predictions and our empirical design. Section 4 has the main empirical results. Section 5 discusses the possible mechanisms explaining our results, their external validity, and policy implications. Section 6 concludes.

1. Mortgage Environment in the Netherlands

1.1. Pre-Reform

Assisted by strong recourse laws¹⁰, and stimulated by generous mortgage interest deductibility (MID) policies, the Netherlands has had traditionally loose origination practices for mortgages. This facilitated large interest-only (IO) components and loan-to-value (LTV) ratios well in excess of 100%. Starting in 2001, the Dutch government began to place limits on these practices. In order to be eligible for MID and the national mortgage insurance (NHG)¹¹, the mortgage maturity was limited to 30 years and the IO part was capped at 75% of the mortgage sum. In an effort to keep MID tax benefits on the amortizing part, banks introduced linked savings accounts in which homeowners put funds that are invested to repay the mortgage at maturity. This contract is virtually identical to a standard amortizing mortgage, but with larger tax benefits. 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. Homeowners are not allowed to access the savings during the duration of the mortgage. Returns on savings are not taxed.

¹⁰ According to Moody's, recourse laws in the Netherlands are "very strong", also in terms of enforcement, while it is "weak" in the U.S. (NVB 2014). During the Great Recessions, foreclosure rates in the U.S. were almost a hundred times higher at their peak than in the Netherlands, even though a higher proportion of households had negative equity in the Netherlands.

¹¹ The insurance was only available if the house value was less than a maximum amount (€320 k in 2012), and if the payment-to-income was below a certain limit. The insurance provided additional protection and liquidity from originating banks, resulting in a pass-through effect of substantially lower interest rates for borrowers.

In 2007, banks signed a Code of Conduct for Mortgage Loans (CCM) that further tightened mortgage rules. Initially it set limits on payment-to-income (PTI) and LTV ratios. In August 2011, it set the maximum IO component of new mortgages to 50%. The other half could be in the form of a mortgage with a linked savings account, as long as it amortized over a period of 30 years (or less). Following this reform, the vast majority of mortgages originated had 50% IO and 50% linked accounts. In addition, the revised CCM set the maximum origination LTV at 106%, with 1% reductions each year afterwards until it finally reached 100% in January of 2018.¹² Similar to the uptake of IO mortgages, households tended to borrow up to the allowable regulatory limits. For FTHBs in 2013, more than 40% of mortgage offers were within 5 percentage points of the regulatory LTV limit and around 20% of all mortgage offers were at exactly the limit.

1.2. The 2013 Reform

For new home purchase contracts signed after January 1st, 2013 the Dutch government implemented a new macroprudential policy intended to promote “Financial Stability”. Proposed at the end of April 2012 and passed in October of that year, the new policy required FTHBs to have fully amortizing 30-year mortgages in order to retain MID and to be eligible for national mortgage insurance.¹³ During most of 2012, it remained uncertain whether the policy would pass and if so in what form. In an article published on August 31st, 2012 ABN Amro, one of the largest banks and residential 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.”¹⁴ In the end, the policy passed, applying to all FTHB mortgages and all existing homeowner mortgage *increases* – existing mortgages were grandfathered under the old rules. The policy did not allow FTHBs to have any IO mortgages or to amortize via a linked savings account.

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, causing a dramatic increase in the percentage of the mortgage balance expected to be repaid. 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 the Mortgage Data Network (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).

¹² See Struyven (2015) and van Bekkum et al. (2019) for more discussion of this regulatory change and its effects.

¹³ Parliamentary document 33405-29. “Wijziging van de Wet inkomenbelasting 2001 en enige andere wetten in verband met de herziening van de fiscale behandeling van de eigen woning (Wet herziening fiscale behandeling eigen woning)”.

¹⁴ “Covered Bonds in the Netherlands”, ABN Amro (September 2012).

Examining the Dutch 2013 reform has a number of benefits. 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, the National Institute for Budget Information (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. 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 the introduction of amortization itself. Fourth, we do not see evidence of other dramatic changes in macroeconomic and mortgage conditions around the reform. Figures A2 and A3 shows that house prices, mortgage rates, and average origination loan-to-value and income ratios varied smoothly around the reform. Even though increased amortization implies shorter duration, average mortgage interest rates are also smooth. 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 become floating within the first 10 years).

Existing homeowners (non-FTHBs) were also affected by the reform, but to a smaller degree. Mortgage balances outstanding on January 1, 2013 were fully grandfathered in the old rules. Conditional on meeting the 2011 CCM requirements, non-FTHBs wanting to buy a new home could carry-over existing interest-only or linked-savings mortgages. Any increase in the mortgage balance would fall under the new rules, and the maturity on existing mortgages could not be extended. In practice, this means that the effect of the reform was larger for non-FTHBs with mortgages with shorter remaining maturities.

2. Data description

2.1. Datasets and Sample

Our primary analysis takes advantage of administrative datasets from CBS, with individual-level financial information on every person living in the Netherlands from 2006-2017. The datasets are transactions in the existing purchase dwellings registry (*Bestaande Koopwoningen*), the universe of spells for individual addresses (*Adresbus*) and family structure (*Huishoudensbus*), household balance sheets (*Integraal Vermogen*) and the population socio-demographic characteristics (*Persoontab*). From the household spell registry, we obtain variables such as the household size, the type of household (ex. married without children) as well as the position of the individual in the household (ex. partner in married couple without children). These household structure variables allow us to pin down the timing of changes in family structure, such as the birth of a child, death of a family member, divorce, etc. Housing transactions are based on the month a household is registered as taking ownership of the property, which typically differs

by at least 2 months from the date they went under contract. Housing data comes from the *Kadaster* (deeds office), social and demographic characteristics come from the *Bevolkingsregister* (civil register, administered by local municipalities), while household balance sheets come from the national tax records and the national credit registry.

We focus our analysis on all 111,523 people in the Netherlands who bought their first home financed with any kind of mortgage in either 2012 or 2013 and examine their outcomes in the years around the house purchase. Table 1 provides simple summary statistics on these households. The strict recourse laws (and their enforcement) in the Netherlands are associated with high initial LTV ratios, usually well in excess of 100%. With a median of about 105%, this is true for the buyers in our sample as well. 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 €187k, with median total debt at €193k. 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.

2.2. Variability in Liquid Assets

Consistent with their relatively high income, the median household in our sample has a non-negligible amount of liquid assets. This has substantial variability. We measure liquid assets as the combination of all deposits (money in all checking and savings accounts) and financial instruments like stocks and bonds. The Netherlands has a wealth tax and information on liquid assets is collected comprehensively at the household level and verified by financial institutions. Table 1 shows that the median household has close to €8k in liquid assets, with the 25th percentile at €2.6k. The within household year-over-year standard deviation in liquid assets is about €14k between 2006 and 2017 (not reported). In 2014 this was about €9k. This variation appears to be driven by changes in economic conditions faced by households. In appendix Figure A1, we plot yearly changes in liquid assets 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 likely use their liquid assets as a buffer. This provides some validation of the administrative data collected and verified by the Dutch government, and shows that households have non-negligible stocks, flows, and variability in their liquid assets.

2.3. Measuring Wealth Accumulation

One of the advantages of exploring this reform in the Netherlands is the presence of detailed administrative data on wealth and its components at the household-level. In this paper we focus on wealth accumulation defined as the year-over-year change in a household's assets minus their liabilities.

For our primary analysis we include all assets reported by CBS that represent wealth accumulation decisions of the household. We consider the change in all liquid assets, as discussed in Section 2.2, as well as implied voluntary pension contributions, which together we refer to as financial assets.¹⁵ Since we do not observe the stock of voluntary pension contributions, only the flow, we only use the change in financial assets in the analysis. The changing value of household real estate is measured with substantial noise and most of the variation is not driven by household wealth accumulation decisions, so we explore that separately.¹⁶ Our measure is meant to capture wealth accumulation decisions by the household, not their total wealth, so it does not include the current discounted value of human capital (ex. income), mandatory pension contributions, etc. Apart from income, we do not expect any of these to change systematically around the reform. We explore income separately.

Liabilities include the outstanding mortgage balance and all other liabilities. Non-mortgage liabilities are provided by CBS and are based on national credit registry data merged to the household. We define non-mortgage savings as changes in financial assets net of changes in non-mortgage liabilities.

Outstanding mortgage liabilities are based on administrative tax records from CBS filed by households and verified by banks. These data do not include information about amortization within the linked savings accounts discussed in Section 1. To overcome this issue, we use data provided by HDN of mortgage offers. This data covers around 75% of mortgage offers as of December 2014. The dataset contains detailed information on loan characteristics including the size of the mortgage and mortgage contract type (ex. fully amortizing, interest-only, etc.). As we noted previously, prior to the 2013 reform, new mortgages had to be at least 50% amortizing to be eligible for interest deductibility and national mortgage insurance. We verify that most mortgages qualified, usually with amortization through a linked savings account. In our analysis, we treat linked savings as amortization. Therefore, if in CBS we observe a mortgage without a year-over-year change in its mortgage balance, we make the assumption that the household has an (amortizing) linked savings account for 50% of their mortgage.¹⁷ We then impute the amortization the household effectively made within the linked account, assuming these mortgages amortize as an annuity, using an interest rate of 4.50%. As we noted previously, households were unable to access linked savings before the end of the mortgage.

¹⁵ In the Netherlands, most pension contributions are mandatory and collected by employers. If these mandatory payments are below the statutory limit, individuals can make voluntary pension contributions. For tax purposes these are subtracted from a household's gross income, leading to a lower taxable income. We can observe each household's gross and taxable incomes, as well as other factors which cause differences between those two (ex. mortgage interest payments) allowing us to back out their voluntary pension contributions. We verify that these contributions are positively correlated with household income and are generally distributed in ways consistent with maximum contribution cut-offs, providing validity for our calculations.

¹⁶ Another issue is that since house prices are the discounted present value of future rental rates, house price changes may not reflect changes in wealth, if costs of living in that area rise as well. That being said, we show that our results are unchanged if we include changes in the value of real estate in our measure of wealth.

¹⁷ In our robustness checks, we show that results are virtually the same when we change these assumptions.

2.4. Life Events

Another benefit of examining this reform in the Netherlands is that CBS provides accurate and up-to-date information about household life circumstances. We use detailed information on the number of household members over time to create a sub-group of households who had “life events” between 2012 and 2013 and also bought their first home with a mortgage during that period. We define life events as any month where the number of household members changes (ex. birth of a child, death in the family, divorce, child moving out, etc.). For this sub-group, the timing of the first-home purchase is likely to be driven by the timing of the life-event and unlikely to be timed strategically to avoid the reform. We verify that the timing of life-events strongly predicts the timing of home purchase. We use the timing of life-events, rather than home purchase, as an instrument for the reform-induced additional mortgage amortization.

3. Theoretical Predictions and Empirical Design

The main analyses in the paper are based on the following decomposition:

$$\text{Wealth Accumulation}_{i,t,t+1} \equiv \text{Mortgage Amortization}_{i,t,t+1} + \text{Non-mortgage Savings}_{i,t,t+1} \quad (1)$$

where wealth accumulation for household i from date t to $t+1$ is equal to the mortgage amortization, including all mortgage debt repayment, plus any non-mortgage savings over that same period. Non-mortgage savings includes all other components of household wealth accumulation except mortgage repayment, including the build-up of financial assets (deposits, stocks, bonds, and voluntary pension contributions) or reductions in non-housing liabilities, such as consumer loans. A change in amortization requirements only increases wealth accumulation if it is not offset by changes in other net savings. For example, if a household is forced to amortize its mortgage with an additional €1k in a given year, its total net wealth is only going to increase by €1k if it does not sell stocks worth €1k, or dissolve in another way.

The key aim of the paper is to establish the fungibility between different types of savings, and to provide one estimate that pins down the following two parameters:

$$\text{Fungibility } (F) := -\frac{\partial S}{\partial A} \quad (2)$$

$$\text{Marginal Wealth from Amortization } (MWA) := \frac{\partial W}{\partial A} := I - F \quad (3)$$

where F is the fungibility between mortgage repayment induced by amortization and net non-mortgage savings, while MWA is the change in wealth for a change in mortgage amortization. If mortgage repayments and non-mortgage savings are treated as perfect substitutes, then $F=1$ and $MWA = 0$. In that case, any changes in mortgage repayments are offset by changes in non-mortgage savings, leaving wealth accumulation unchanged. On the other hand, if $F=0$ then households do not alter their behavior in their other accounts which means increased debt repayments leads to more wealth accumulation.

To estimate these parameters, we compare outcomes over the same time period (ex. Jan-Dec 2015) for FTHBs who bought between 2012 and 2013 – comparing those who bought before vs. after the reform. As an initial exercise, we compare average mortgage repayments and wealth accumulation by month of closing relative to the average in a given month (ex. February of 2013):

$$A_{Jan-Dec 2015,i} = \sum \delta_C \times 1_{C,i} + \eta_i \quad (4)$$

$$W_{Jan-Dec 2015,i} = \sum \beta_C \times 1_{C,i} + u_i \quad (5)$$

where A and W are mortgage repayment (amortization) and wealth accumulation, respectively. In each regression, the only independent variable is the cohort $1_{C,i}$: the month a household closed on their house. The reform was binding for those who went under contract after January 1st, 2013. Typically, it takes two months to close. Therefore, we consider households who closed after March 1st, 2013 as treated (intent-to-treat). From these estimates, we calculate $\widehat{MWA} = \frac{\bar{\beta}_{treated} - \bar{\beta}_{control}}{\bar{\delta}_{treated} - \bar{\delta}_{control}}$ using just these simple averages.

We estimate this parameter more formally with two-stage least squares (2SLS), using the closing date as an instrument for mortgage repayments. In particular, we estimate the following first stage:

$$A_{Jan-Dec 2015,i} = \delta_{treated} 1_{treated,i} + \lambda_r + X_i' \beta + \eta_i \quad (6)$$

where A is debt repayment driven by mortgage amortization from January till December of 2015 for household i , $1_{treated,i}$ is a dummy variable equal to 1 if a household i closed on their house after March 1st, 2013, λ_r are location fixed effects, and X_i are household controls in the years prior to home purchase (ex. 2010 household gross income). If the reform increased mortgage repayment, we would expect $\delta_{treated}$ to be positive and highly statistically significant. The second stage estimates the effect of the predicted mortgage amortization from equation (6) on wealth accumulation (we run this using 2SLS to obtain the correct standard errors):

$$W_{Jan-Dec 2015,i} = \gamma_{treated} \hat{A}_{Jan-Dec 2015,i} + \lambda_r + X_i' \beta + u_i \quad (7)$$

One concern is that the timing of closing may be correlated with household preferred wealth accumulation. This selection effect could bias our estimates. To address this issue, we run the same 2SLS specification from equations (6) and (7) restricted to the set of buyers who also had a life-event during this period and use the month of the life-event, not the actual month of closing, to determine whether a household is treated or not.

Wealth accumulation cannot arise out of nowhere. By definition:

$$A_{2015,i} \equiv Y_{2015,i} - E_{2015,i} \quad (8)$$

where Y is after-tax income and E is expenditures of household i . Therefore, if households accumulate more wealth because they do not fully adjust non-mortgage savings, then they must either increase their income

or reduce their expenditures. We use the detailed administrative data to measure changes in income and we interpret the remaining variation as changes in expenditures.

4. Results

4.1. Mortgage amortization and wealth accumulation

We examine mortgage amortization and wealth accumulation among FTHBs who closed on their house at different times around the reform. In Figure 2, we compare the amount of mortgage repayment from January to December 2015 cohort-by-cohort following the methodology outlined in equation (4). The March 2013 cohort includes the first households to have gone under contract after the reform. The solid black line is the estimated amount of mortgage repayment relative to the omitted February 2013 cohort (the last cohort not affected by the reform). We do not include any other controls and use the full sample of FTHBs. Those who closed up to February 2013 had similar amounts of mortgage repayment in 2015. By contrast, buyers who closed in March 2013 and later have a sudden and persistent rise in amortization, flattening out at about a €2k increase. The lack of pre-trend and sudden change around the reform suggest that “age” (the number of months since closing, indicated on the horizontal axis) has no effect on 2015 financial decisions in and of itself. To evaluate the economic magnitude of the effect, we scale it by median household liquid assets as of the end of 2014 (small percent values in brackets along the y-axis in Figure 2). The estimated annual increase in amortization for treated cohorts is about a quarter of the median stock of liquid assets. This implies a substantial increase in mortgage debt repayments.

Next, we examine the change in households’ non-mortgage savings and the net-effect on wealth accumulation. The yellow dotted line in Figure 2 is the change in non-mortgage savings (liquid assets plus voluntary pension contributions minus non-mortgage liabilities) over 2015 for the same cohorts, again relative to the omitted February 2013 cohort. Those who closed up to February 2013 had similar amounts of non-mortgage savings in 2015. However, in contrast to the change in mortgage repayments, we find little evidence that households buying after the reform reduced their non-mortgage savings to compensate for the increased debt repayment. Households appear to act as if these accounts are infungible ($F \sim 0$). The dashed gray line in Figure 2 shows a near 1-for-1 increase in net wealth accumulation.

In Table 2, we formalize this analysis following the 2SLS procedure outlined in equations (6) and (7). 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. Column 1 shows that cohorts almost surely buying after the reform had a ~€2k higher mortgage debt repayment in 2015. This is our first stage estimate. Columns 2 shows a nearly identical effect on wealth accumulation for the same households over the same year. Consistent with these results, column 3 gives an estimate of the marginal wealth accumulation from amortization, MWA , of 0.993, which

is statistically different from 0, but not from 1 (the 95% confidence interval is between 0.88 and 1.10). Column 4 shows there is no statistically significant effect on non-mortgage savings. In appendix Figures A4 and A5, we separate this effect into changes in financial assets (A4) and non-mortgage liabilities (A5) – neither display an offsetting effect.¹⁸

In Table 3, we examine how households adjust to the increased mortgage repayment using the income/expenditures/savings identity from equation (8). Column 1 shows that within a given household, gross income increases by ~€1,270k between 2012 and 2015 for those who bought after relative to before the reform. This is about 62% of the increase in mortgage amortization and wealth accumulation in Table 2, columns 1 and 2. In appendix Tables A5 and A6, we use detailed administrative information to show that virtually of the post-reform rise in household income comes from an increase in labor supply.¹⁹ In appendix Table A5 we look at labor market participation. Columns 1 and 2 show an increase in the number of wage earners in the household, both overall and for those households with at least two working age members. Column 3 shows that the probability a household has only a single earner falls from around 27 to 25%. Column 4 shows that this holds for the subset of households that experience a change in single earner status from either single earner to multi-earner or the reverse. In appendix Table A6 we look at total hours worked. Columns 1 and 2 show that hours worked are highly correlated with household income, both in levels and in changes. Columns 3 and 4 confirm that the total household hours worked increased between 2012 and 2015. Finally, column 5 shows that controlling for changes in hours worked reduces the effect of the reform on income growth to close to zero.

In columns 2 and 3 of Table 3, we formally run 2SLS regressions to look at effects of the reform on the *change* in gross household income. Column 2 has no controls and shows an estimate of 0.621. Since marginal tax rates are about 42% in the Netherlands for our group of buyers, this suggests that approximately $0.621 \times (1-42\%) \sim 36\%$ of the increase in wealth accumulation is paid for by an increase in after-tax household income (95% confidence interval between 22 and 51%). Column 3 controls for financial circumstances well before the reform, in particular the log of gross household income and financial assets

¹⁸ In appendix Table A1, we show that these results are robust to including an alternative measure of voluntary pension contributions that includes all imputed values (Column 1), including the appraised value of real estate in our measure of wealth (column 2), including both (column 3), or running a levels-on-levels regression of the households' home equity value on net worth (total assets – liabilities) as of the end of 2015 (columns 4 and 5). Findings are also robust to using an alternative sample (Table A2), that includes even unusually large wealth (column 1) or mortgage (column 2) changes, or every single household in our sample that buys a home, including those with large changes in wealth/mortgage balances and those who are not buying a house for the first time (column 3). Findings are equally robust to varying the amortization and interest rate assumptions for unobserved linked mortgage accounts (Table A3) or the choice of method to compute standard errors (Table A4).

¹⁹ Information on hours worked by employees are mandated to be reported monthly by employers to the Ministry of Social Affairs in order to track required social benefits and are linked to the primary data sources via unique person-level identifiers by CBS.

in 2011, and location fixed effects, and shows a similar estimate.²⁰ In columns 4 and 5 we run 2SLS regressions looking at income *levels* in 2012 and 2015, respectively. Results show that the change in income does not come from a lower income right before the reform in 2012, but from a higher income in 2015. The level-estimate in column 5 suggests that about 26% of the increase in debt repayment was paid for by a rise in after-tax household income. Taken together, our point estimates suggest that households compensated between 26 and 36% of the rise in mortgage amortization by increasing after-tax household income. The remainder must be driven by lower household expenditures.

4.2. Addressing selection concerns

Our findings are consistent with a large response of wealth accumulation to mortgage amortization. However, since the timing of home purchase is not randomly assigned it is possible that our estimates are confounded by selection concerns. If households who want to save less are able to systematically buy before the reform, leaving only those that do not mind saving more to buy after, then this would bias our estimates upwards. In appendix Figure A6, we examine the number of home closings per month for our group of buyers and do not find any evidence consistent with bunching around the reform.²¹

That being said, it is theoretically possible (though not ex-ante obvious) that sorting could shift transactions across time, without any variation in the total level of transactions, in a way that causes systematic bias in our estimates. Evidence in appendix Figure A7 suggests that such concerns are unlikely to be a major factor in this setting. We break-out our analysis into FTHBs and non-FTHBs, conducting the same analysis based on the purchase cohort month around the 2013 reform. FTHBs are treated more (Panel A), so if there is selection around the reform cut-off that keeps the total number of buyers smooth, but systematically sorted, we would expect to see a sudden non-linear change in FTHB co-variates right around the regulatory change, matching Panel A. We would also expect to see a similar, but smaller, non-linear movement in these variables for non-FTHBs, since they are treated, but not quite as much. Across all variables though, whether it is house value (Panel B), pre-reform liquid assets (Panel E), gross household income (Panel F), liquid asset accumulation (Panel G), or income growth (Panel H) we see no evidence of sharp non-linear changes in the co-variates of FTHBs, non-FTHBs, and the differences between them in

²⁰ For comparison, Column 6 shows that our initial estimate of *MWA* is unchanged if we include the same set of pre-reform and location controls.

²¹ 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. Responses to the transaction tax are consistent with prior findings (Best and Kleven 2018) that such taxes can cause distortions across a number of dimensions, including the timing of purchases. In our primary analyses and figures we exclude buyers who purchased around June 2012 for exactly this reason.

the cohorts before and after the reform.²² We also see no evidence for changes in liquid assets for either group around the reform (Panels C and D).

While unlikely, it is possible that there was a sudden change in *unobservable* buyer characteristics. To alleviate this concern, we use a novel feature of our setting and data: the occurrence of life events. These are changes in the number of people in a household, for example the birth of a child, after which households are much more likely to move. We focus on the subset of our original sample of FTHBs who experience a life-event during the same period when they purchase their homes (2012-2013). The high-quality of the Dutch administrative data lets us identify the exact month such events took place. In Figure 3, we perform the same analysis as in Figure 2, focusing on this subset of buyers. Because sample size is smaller, we focus on cohorts grouped by quarter. We plot the effects by the quarter of the life-event, not the actual closing. In Figure 3, we show that relative to the omitted cohorts – Q4 of 2012 and Q1 of 2013 – mortgage amortization over 2015 is similar for 2012 cohorts (gray points). For 2013 cohorts we find substantially higher mortgage repayments. Similar to Figure 2, increases in mortgage amortization are matched nearly one-for-one with increases in wealth accumulation over 2015.

We run this analysis formally in Table 4 using the same 2SLS methodology as before, now on the subset of buyers with life-events, using the month of their life-event rather than the closing of the home purchase as instrument. Columns 1-3 show no difference in pre-reform household income, net financial asset accumulation, or overall wealth accumulation in 2010. Columns 4 and 5 do show significant increases in the amount of mortgage repayment and wealth accumulation. Column 6 shows that these differences are not offset by changes in the assessed value of homes, indicating it is unlikely that these effects are driven by differential home investment or better timing of purchase.

We find little evidence that the reform changed homeownership rates. In column 7, we look at all households who do not own a home at the end of 2011 (not only those buying in 2012/2013), and who have a life-event between 2012 and 2013. In a linear regression, we predict whether these households own any real estate by the end of 2016 (which occurs 16.9% of the time) with the occurrence of a life event after the reform. We find little predictive power. We show this more clearly in appendix Figure A8 where we estimate the regression month-by-month. We see no evidence of any effect around the reform.

As before, the IV specification in column 8 shows an estimate of the *MWA* of 0.864, which is statistically different from 0, but not from 1 (the 95% confidence interval is between 0.54 and 1.19).²³

²² In Panel F, there is a slow downward trend in FTHBs that is steeper than for non-FTHBs. However, there is little evidence of a sharp non-linear change around the reform. Moreover, the lack of any difference for non-FTHBs buying before vs. after (and no differential future financial asset accumulation for either group) make it unlikely this is driven by the reform.

²³ We show in appendix Table A1 column 6 that we obtain consistent results running the analysis in levels of home equity on net worth as of the end of 2016 (instead of in changes) and including the appraised value of the home.

One potential concern is that household records are more likely to be updated when there is a move. In that case we would still be relying on variation that, at least in part, comes from the timing of the home purchase. To alleviate this concern, we re-run our analysis in Column 9 focusing on the subset of households who have a life-event month that differs from the month of the house transfer. We again find an estimate of *MWA* close to 1 (0.931, with the 95% confidence interval between 0.41 and 1.45).

To sum up: we find little evidence for bunching of transactions before the reform, suggesting that selection is not a first order concern. This is confirmed by an analysis where we use life-events, which are unlikely to be selected strategically around the reform, as a different source of variation. We obtain similar estimates with *MWA* close to 1. This suggests that the overall findings are unlikely to be contaminated by selection effects.

4.3. Addressing confounded treatment concerns

Next, we examine whether effects might be confounded by other changes around the reform.

One potential confound might come from differences between groups at year-end that arise from the date of closing (rather than going under contract) occurring before or after year-end. One such candidate is the Dutch wealth tax that is levied on mortgage savings as of January 1st. There were no changes in the wealth tax from 2012 to 2013. However, those households who closed after January 1st 2013 might have had more non-housing savings on that date than those who closed earlier, and therefore had to pay a higher wealth tax (at 1.2%). It is unlikely that this effect lasted until 2015. Nevertheless, in our setting there is a straightforward way to address this issue and other issues arising from similar year-end effects. In Table 5, column 1, we re-estimate our primary specification with households that closed either in January and February or March and April 2013. As explained before, the former less likely to be affected by the reform than the latter. Results are virtually the same as before, suggesting that any year-end policies that were based on the date of closing are unlikely to drive results. This exercise also confirms that “age” (months since closing), which is similar for the two groups, is an unlikely confound.

Another potential confound are other effects from the reform itself. In particular, households who purchased their homes under the new rules lost part of the mortgage interest deductibility (MID). The reform mandated faster repayment, which reduced the MID amount. All else equal, this means an increase in tax liabilities. This affects both the liquidity and life-time wealth of home buyers.

There are several reasons why these effects might be small in our setting. Given the convex amortization scheme of annuity mortgages, the increase in tax liabilities predominantly accrues later in the life of the mortgage. This means that the liquidity effect will be small in the first few years of the mortgage, on average amounting to substantially less than the €2000 baseline effect we find. The life-time wealth effect is potentially larger. This depends on people’s expectations about the future of MID, which was highly uncertain. The Dutch Council of State was concerned that the reform would lead to an unjustifiable

unequal treatment of FTHBs and non-FHTBs.²⁴ Existing homeowners may have expected to lose part of the MID as well. Moreover, the reform was the first substantial change in the Dutch MID regime in decades, suggesting more restrictions were to follow.²⁵ As we discussed earlier, we find no evidence of bunching around the 2013 reform, but we do around a possible increase in the transaction tax in June of 2012 (appendix Figure A6). This confirms that households did not interpret the 2013 reform as a wealth shock similar to the June 2012 transaction tax.²⁶

Nevertheless, differences in MID are still a concern worth addressing. First, we consider liquidity. In Table 5, columns 2 and 3, we show that our estimates of *MWA* are similar if we estimate it for 2014 or 2016 (rather than 2015).²⁷ If liquidity effects from tax differences were important, we would expect to see substantial differences between those years. Also, in 2014, tax differences should have been minor since an annuity mortgage hardly amortizes anything in the first few months after origination. Nevertheless, we still find an effect not different from 1.

Second, we explore life-time wealth effects. According to the life-time income hypothesis, the effects on consumption and savings from a one-time change in expected wealth are smoothed across the life cycle. That means that households purchasing after the reform would be expected to permanently increase their savings (reduce their borrowing) in anticipation of lower tax deductibility in the future. To address that concern, we take advantage of the convexity of the amortization schedule of annuity mortgages. Each month, the amortization amount increases. In other words, our treatment grows over the life of the mortgage. This allows us to compute the increase in the mortgage debt repayment within a given household over 2014 vs. 2016. We relate this to the change in wealth accumulation over the same period. Again, in Column 4, we find an estimate of *MWA* of about 1. This suggests that it is unlikely our effects are confounded by one-time wealth shocks occurring at the time of going under contract.

²⁴ 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.

²⁵ 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 reached the tax rate of the lowest tax bracket. In October 2017, a new government decided to speed this up to 3% per year.

²⁶ It may also be that the MID had relatively little effect on household decisions to buy a home or not. The existing literature has been somewhat mixed with Jappelli and Pistaferri (2007) finding no effect of the MID on mortgage debt at either the extensive or intensive margin in response to an Italian reform, while Gruber et al. (2019) analyzed a reform in Denmark and found no changes in homeownership decisions, but did find changes for some households in the size of houses purchased.

²⁷ For all analyses looking at wealth accumulation over 2014, we exclude households closing near the end of 2013. Households tend to save and dissave substantial sums in the months around purchasing a home. This means that households that closed near the end of 2013 display systematically different savings patterns in 2014 than households closing at an earlier date. Since we do not have monthly savings data, we cannot adjust for this. This makes it impossible to make clean comparisons for this group. The households that remain in our treatment group all bought a home at an earlier point in 2013, closer to the implementation of the reform. If anything, this makes them a more natural comparison group for our control group of households who bought right before the reform.

Finally, in appendix Figure A9, we compare non-FTHBs buying a new home with different lengths of their previous residence housing spell. In particular, we compare buyers who lived in their prior residence for more versus less than 10 years (the average is 16 years). The second group likely had longer maturities remaining on their mortgages, and therefore faced a larger (perceived) future wealth loss from the reform's MID reduction. Nevertheless, the behavior of the two groups around the reform is virtually the same. Panels A and B show that both groups faced similar short-run changes in mortgage debt repayment around the time of the reform, and bought similar sized houses, while Panels C and D show no difference in liquid asset accumulation. Also, based on HDN data there is no evidence of non-FHTBs rushing to refinance their mortgages and extend maturities prior to the reform. In the last quarter of 2012, only 33.4% of non-FTHB mortgage applications were to refinance, slightly lower than the 38.9% in the first quarter of 2013. Again, these results suggest it is unlikely that a (perceived) wealth shock from limiting the MID biases our estimates in a meaningful way.

4.5. Not just non-savers

For the correct interpretation of our findings, it is important to pin down what type of households drive our results. In this section, we study whether our effect is isolated to non-savers or holds more generally.

Households with limited liquid assets might be forced to accumulate wealth since there is no easy way of undoing higher amortization payments. If such households represented the majority of cases in our sample, our results would certainly still be important, but less generally applicable. As we discuss in Section 2.2, most households in our sample have more than enough liquid assets to pay for the increased mortgage amortization. In Table 6, we confirm that our results are the same for households that do not appear to be financially constrained. Columns 1 and 2 consider households with loan-to-value ratios at the end of 2014 of less than 90% and loan-to-income ratios below 4. In both cases, households are putting down significantly more funds than they need to at the time of initial home purchase, which makes it less likely that they are liquidity constrained.²⁸ Column 3 looks at the subset of households who either have at least €10k in liquid assets at the end of 2015 or save at least €3k in that year. Both groups would be capable of paying for the increased mortgage repayment out of their liquid assets, suggesting they are unlikely to be up against their financial constraints. Results are the same.

It is possible that households with substantial liquid assets have a high demand for precautionary savings (or for some sort of indivisible consumption in the future). In that case, this group would still be unwilling to reduce its liquid assets in response to increased mortgage debt repayments. We address this concern in Columns 4-6, focusing on households had *at least* €10k in liquid assets at the end of 2011, two

²⁸ This provides additional evidence that our effects are unlikely to be confounded by concurrent policy changes that affected maximum LTV and LTI ratios, since these groups should be largely unaffected by such constraints.

years before the reform. Column 4 shows that these households had on average €43k more than others at the end of 2011. Column 5 shows that this difference is persistent: at the end of 2015, they still had €26k more. Column 6 shows that the *MWA* is the same for this subgroup, suggesting that even the most liquid households increased their wealth accumulation one-for-one with amortization.

4.6. Persistence

So far, we have shown that *MWA* increases persistently after the reform, even in the face of substantial additional amortization. In appendix Table A7, we examine this further. Columns 1 and 2 show that over the four years from December 2013 to December 2017, FTHBs buying after the reform accumulated more than €8k in additional home equity through increased amortization, without an offsetting reduction in non-mortgage savings, leading again to an estimate for *MWA* not statistically different from 1. In Column 3, we compute the ratio of mortgage repayment over those four years divided by the level of all liquid assets at the end of 2017.²⁹ We find that the increase in net-wealth due to increased amortization is on average larger than the stock of liquid assets. This implies a high degree of persistence and indicates that households do not undo the effects of increased amortization over the length of a typical business cycle.³⁰ Our paper therefore makes an important contribution establishing substantial effects of amortization on household wealth, with important implications for debt repayment-savings fungibility, macroprudential policies, and the life-cycle dynamics of household savings, consumption, and labor supply decisions.

Though not the primary focus of this paper, it is still interesting to consider longer run effects. Do affected households access the extra funds prior to or upon retirement, or do they pass it on as bequests to the next generation? Since the reform only occurred a few years ago, we are of course unable to track the lifetime of wealth accumulation by these households, let alone those of the next generation. However, there is some suggestive evidence to suggest that effects might persistent beyond five years.

First, we explore whether effects change as households approach retirement. In Table 7 we show that our results appear to be pervasive across age groups. In Columns 1-3 we re-run our primary analysis but focus on households where the maximum age in a household is above 30, 40, and 50 respectively. In all cases we find results in line with our baseline estimates. In the Netherlands, there are few multi-generation households so it is unlikely we misclassify households. Nevertheless, in Column 4 we try to alleviate such concerns by re-running the analysis of Column 3 on households with a maximum age above 50, excluding households with age differences of more than 20 years. Our results are consistent.

Next, we examine whether moving represents an opportunity for households to re-lever and extract home equity to consume. Our sample includes FTHBs from 2012 and 2013 and there is only a relatively

²⁹ We exclude households with less than €100 and a ratio greater than 50 to makes sure that outliers do not drive the effects. If anything, this reduces the size and significance of the effect.

³⁰ Over the past half-century, the average NBER dated business cycle was around 5 years.

small group that buy and sell another home during our period. In appendix Table A8, we consider the subsample of 1,768 people who have resold their home by December 2016. Columns 1 and 2 shows that by December 2017, those buying after the reform have substantially more home equity. Nevertheless, column 3 shows that they do not appear to extract more of this home equity when moving. This is confirmed by Columns 4 and 5, which show that these households also do not have any additional financial assets, which would be likely if there was differential home equity withdrawal. Columns 6 calculates the *MWA* for this group and shows that this is not statistically different from 1. In sum, household do not appear extract home equity and re-lever around a move.

These subsample results line up well with results for Dutch homeowners overall. For all non-FTHBs over 40 who purchase a home in 2012-2013, the median LTV the year after purchase is only 86% and the 25th percentile is only 59%, well below the median and 25th percentile for FTHBs of 105% and 101%, respectively. In other words, it does not appear that households who sell one house and buy another re-lever to anywhere near the regulatory LTV limits. Results are also consistent with general patterns in the U.S., where non-FHTBs do not appear to re-lever back-up to regulatory limits or to the level of FTHBs (Patrabansh 2013; Patrabansh 2015; Bai et al. 2015). In the PSID (Panel Survey of Income Dynamics) sample, we find little evidence that homeowners who sell and buy a house systematically extract home equity.³¹ Each \$1 of additional home equity at the time of sale is associated with on average \$0.88 of additional home equity at the time of the purchase of the new home. That would suggest most home equity accumulation is passed through when moving.³² Taken together, this is suggestive that moving houses may not necessarily be an event that leads to the substantial extraction of home equity due to increased mortgage amortization.

Finally, we examine households' general tendency to repay their mortgages over the life cycle. We compare the Netherlands, where pre-2013 mortgages had a substantial interest-only component, with the U.S., where most mortgages were fully amortizing. Appendix Figure A10 shows the percentage of Dutch and U.S. homeowners with relatively little liquid assets ("hand-to-mouth") that have a mortgage. In both countries, FTHBs typically finance house purchases with mortgages, as can be seen for individuals aged 25-30 in Panel B. Nevertheless, older Dutch homeowners are much more likely to still have mortgages. In their early-to-mid 60s, about 94% for Dutch homeowners still have mortgage debt, while this only 61% for U.S. homeowners. If anything, Dutch FTHBs purchase a home earlier in life, giving them more time to repay their mortgages. This suggests that Dutch homeowners with interest-only mortgages do not save in a separate account to repay their mortgage,³³ while American homeowners with amortizing mortgages

³¹ This is based on families in the 1999 wave and their housing decisions from 1969-2017.

³² Fagereng et al. (2019) document a similarly important role for "passive" wealth accumulation in Norway.

³³ In fact, among all Dutch homeowners aged 60-65 with a mortgage, the average amount of accessible financial assets is only about €26k.

generally do not undo the amortization through home equity withdrawals. This is supported by evidence from the PSID. Fully amortizing 30-year mortgages repay about 3.3% of principal per year (excluding the final year). Among U.S. non-movers in the PSID, who own a home in concurrent waves and have a mortgage in the prior wave, the average (median) mortgage balance falls by 4.4% (4.6%). Of course, there are many other differences between the U.S. and the Netherlands apart from different amortization standards, but if these tendencies hold for those buying around the 2013 reform in the Netherlands, it would be suggestive of longer run effects on wealth accumulation.

In summary, we provide evidence that mortgage amortization causes a substantial increase in wealth accumulation in the first years after home purchase. These effects are similar for those with substantial home equity, across age groups, and for movers, suggesting a general applicability and importance of our findings. That said, it seems entirely plausible that households may eventually access some of this additional amortization-induced wealth, but aggregate statistics among elderly households are consistent with a substantial persistence of these effects across the life-cycle.

5. Interpretation of results

5.1. Plausible mechanisms

For a broad set of households, not just the young or those with no or low liquid savings, we find an estimate of MWA close to 1. What mechanisms induce households to act as if there is little-to-no fungibility between mortgage repayments and other savings? Here we discuss (rational) liquidity wedges, and a number of behavioral factors, such as mental accounting, default settings and pre-commitment.

One possible mechanism could be a liquidity wedge. Extracting home equity involves non-trivial cost and time (Campbell et al. 2019). In addition, if house values fall during economic downturns, then funds paid into reducing mortgage debt could be inaccessible (DeFusco and Mondragon 2019). This could be exacerbated if refinancing costs or interest rates would be too high, especially for households with high loan-to-income ratios. Moreover, states of the world in which a household might want to tap into its savings (ex. a job loss) might be situations when constraints make it costly or impossible to access home equity. Consequently, households might rationally treat mortgage repayment and non-mortgage savings as not fungible from a short (or medium)-term consumption smoothing standpoint, even if they have similar effects on long-term wealth accumulation. It is possible that households with more liquid savings have them because they face higher precautionary savings needs, which is why MWA is high for this group as well.

While such a liquidity wedge undoubtedly plays a role in our findings, it is not clear it is supported by the full set of our findings. First, households appear willing to cut consumption substantially today in order to avoid *any* increased risk in the need to cut marginal consumption in the future, even though they are able and willing to alter labor supply in the present and have chosen their current level of precautionary

savings as a buffer against shocks. Second, as noted earlier, we see no evidence of bunching around the regulatory change. If households were this averse to illiquid wealth accumulation, we might expect them to avoid a policy forcing them to engage in exactly that.

Another possibility is that households have bounded rationality. For example, they might overestimate their need for liquidity in the future. D’Acunto et al. (2020) find that when given access to an overdraft facility, even very liquid users act as if they are severely constrained. This is consistent with individuals responding to randomized credit line expansions (Aydin 2019) and income payments (Olafsson and Pagel 2017), even if they have substantial liquidity. Alternatively, households might follow a simple saving heuristic, which, in many countries, are provided by official agencies. For example, since 2008, the Dutch National Institute for Budget Information (NIBUD) has offered an online tool to advise people on their optimal savings. This depends on household size and income, and makes no allowances for wealth accumulation through amortization.³⁴

Apart from this perceived or heuristic precautionary savings motive, there is a wider range of behavioral models that could explain our findings. If households are present-biased and have self-control problems, they will save too little for the future (Kovacs et al. 2020; Kovacs and Moran 2020, Attanasio et al. 2020, and Vihriala 2021). In such a setting, forcing individuals to amortize their mortgage will increase net wealth accumulation.³⁵ In our setting though, self-control problems appear specific to long-term wealth accumulation, since we find a similar effect among households with substantial liquid assets.

Another plausible mechanism is that households may, for mental accounting reasons, treat mortgage repayments as bills instead of wealth accumulation. This is consistent with what Camanho and Fernandes (2018) call the “mortgage illusion”. In an experimental setting, they show that households compare rental to mortgage payments when deciding to rent or own. They do not account for the fact that some of the mortgage payment is amortization. In fact, they are less likely to buy a house when the choices include a shorter duration mortgage, just because faster amortizing means higher monthly payments. These findings are consistent with Argyle et al. (2019) who document that auto loan borrowers dislike shorter maturity loans. This likely reflects a strong aversion to higher monthly payments, even if these reflect amortization. Similar to our setting, these results hold even among unconstrained borrowers, which suggests that liquidity constraints by themselves offer only a partial explanation.

Another possible behavioral mechanism could be that amortization constitutes a “default” setting or “nudge” to accumulate wealth (Thaler and Sunstein 2009). Default settings with passive choice, such as

³⁴ <https://www.nibud.nl/beroepsmaatig/vernieuwde-bufferberekenaar/>

³⁵ Under this interpretation, amortizing mortgages act as a commitment device. In an experimental setting, Beshears et al. (2015) find that people appear to value commitment devices. Yet Laibson (2015) notes that, based on the empirical evidence, there are far fewer such devices than one would expect. Our works suggests that amortizing mortgages might be just such a mechanism. Vihriala (2021) provides evidence from Finland consistent with households purposefully picking a mortgage contract that commits them to saving.

automatically signing individuals up for pension contributions unless they opt out, can have substantial effects on wealth accumulation (Madrian and Shea 2001; Chetty et al. 2014). Policies that require an active choice to opt-in, such as voluntary debt repayments, typically do not have such effects (ex. Kuchler and Pagel 2019). In our setting, amortizing mortgages become the default contract after 2013. Beshears et al. (2019) explore whether default settings that increase U.S. individual pension contributions are offset by increased liabilities. They find only limited evidence, suggestive of a low fungibility between savings and liabilities.³⁶ However, they do not observe the full household balance sheet. Our results support their conclusions based on more detailed data, including households' home equity, financial assets, and other liabilities. Moreover, our results suggest that this low fungibility is symmetric, which is not clear ex-ante.

5.2. External Validity

There is no silver-bullet to verify the external validity of our results outside the Netherlands. Nevertheless, prior work looking at other countries is broadly consistent, suggesting that even if exact responses may vary in other settings, the finding of a large effect of mortgage amortization on wealth accumulation is likely to hold. Ganong and Noel (2019) look at mortgage modifications in the U.S. that grant maturity extensions, which reduce mortgage amortization. This also leads to substantial increases in consumption. Scholnik (2013), d'Astous (2019), and Andersen et al. (2019) look at mortgage run-offs using administrative data in the U.S., Canada, and Denmark. They show increases in consumption and decreases in labor income after people have fully paid off their mortgage debt. Though supportive (at least qualitatively), these papers differ substantially from our work. First of all, they do not look at the effects on wealth accumulation. Moreover, mortgage modifications for distressed households, or mortgage run-offs for older households, may be special events in a lifecycle that do not directly speak to the general effects of mortgage amortization on wealth accumulation.³⁷

Closest to our work are Larsen et al. (2018) and Backman and Khorunzhina (2020) who analyze the introduction of interest-only mortgages (IO) in Denmark in 2003. Backman and Khorunzhina (2020) find that financially constrained households, who are more likely to refinance using IO mortgages, appear to have higher consumption growth. Larsen et al. (2018) find that those who choose to take out IO mortgages consumed more afterwards, but did not alter their financial assets. This is consistent with our findings. However, the selection into IO mortgages in Denmark was likely endogenous. Kuchler (2015) shows that households who choose to use IO mortgages have lower savings rates ex-ante and higher loan-

³⁶ Garcia et al. (2020) also find little evidence that savings nudges change high-interest unsecured borrowing.

³⁷ Households in sufficient distress to file for mortgage assistance might be especially financially constrained. Households about to pay-off their mortgage appear to increase bank loans and already start to reduce their labor income years prior to the run-off, making it difficult to assign all effects to just the change in amortization.

to-value ratios. Moreover, the timing of refinancing into an IO mortgage is almost certainly related to household time-varying conditions.

Findings from a literature on alternative mortgage products (AMPs), such as interest-only loans and option adjustable-rate mortgages, are also broadly consistent. People who took out AMPs in the U.S. had higher incomes and income growth (Cocco 2013). Taking the higher income and credit scores into account, Amromin et al. (2018) show that default rates were twice as high as for normal mortgages. Though the choice of mortgage type is endogenous, these results suggest that U.S. households may not use the extra funds available from option ARMs to increase their non-mortgage savings as a buffer against costly default.³⁸

Aggregate statistics on retired households also suggest that our findings hold in other settings. 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). Even among retired households, real estate is by far the largest single component of savings, making up 47.9% of all non-annuitized household net worth, and is more than twice as large as all assets held in personal retirement accounts such as individual retirement accounts or 401(k) plans (Poterba et al. 2013). Using survey data for the U.S., Canada, Australia, the U.K., Germany, France, Italy, and Spain, Kaplan et al. (2014) document that households with substantial illiquid wealth (such as housing) often hold little or no liquid assets. This is consistent with households treating home equity and liquid assets differently, although there are of course other explanations.³⁹

Even though our results are broadly consistent with findings from these other studies, the exact *MWA* might differ across periods, settings, and sub-groups. To provide some additional structure to this discussion, we can decompose *MWA* into the marginal change in debt repayment for a change in amortization schedule, *MDA*, times the marginal change in wealth accumulation for a change in debt repayment, *MWD*: $MWA = MDA \times MWD$. First consider *MDA*. This parameter depends on how easy it is for households to undo the effects of amortization through home equity withdrawals. In settings where this is harder, *MDA* may be higher.⁴⁰ Next consider *MWD*. If this is driven by fairly universal behavioral factors, such as different mental accounts for home equity and other savings (Camanho and Fernandes

³⁸ This raises the question whether option ARMs are as attractive as some models might suggest (Piskorski 2010).

³⁹ Households may prefer to invest in housing as an asset, either because they believe it has a higher risk adjusted return (Kaplan and Violante 2014), or because it acts as a hedge for local rental rates (Sinai and Souleles 2005). Alternatively, households may prefer to pay down mortgage debt quickly because they are debt averse, and since they have built up substantial wealth in housing, they do not need any other wealth.

⁴⁰ That said, reforms that have made it easier to withdraw home equity for consumption purposes have been shown to have relatively small effects on consumption, let alone wealth accumulation in both Denmark (Leth-Petersen 2010) and the U.S. (Brown et al. 2015; Kovacs and Moran 2020). Though not directly comparable, even in settings where home equity is relatively liquid so that regional house price changes can lead to home equity withdrawals, consumption responses are typically more muted (Mian et al. 2013; Cloyne et al. 2019). This prior work suggests that *MDA* may not vary that much with the ease of home equity withdrawals.

2018), then we would expect MWD to be close to 1 in other settings as well. On the other hand, if MWD is primarily driven by rational liquidity differences between home equity and other savings, then low-cost access to home equity may lead to lower levels of MWD . Both parameters might differ over the life cycle. In our setting, both parameters appear to be close to 1, but just like any microeconomic parameter it is not a universal constant, and therefore will need to be examined in other situations. This emphasizes the importance of prior and future work examining how debt-savings fungibility, home equity withdrawals, housing leverage, and refinancing differ across economic conditions, life-cycles, and regulatory environments (ex. Keys et al. 2016; Bhutta and Keys 2016; DeFusco 2018; Keys and Wang 2019; Beshears et al. 2019; Amromin et al. 2020; Andersen et al. 2020; DeFusco et al. 2020).

5.3. Policy Implications

Our results have important implications for macroprudential policies. The Dutch government intended the specific policy we study in this paper to improve financial stability by stimulating amortization. If households responded to the reform by transferring liquid assets into mortgage repayment, with no change in net-wealth, such a policy would have been ineffective. By contrast, our findings confirm that extra amortization increased household home equity, with potential benefits from having fewer underwater mortgages and reduced housing lock (Bernstein and Struyven 2019), without reducing non-mortgage savings. This means the reform worked as intended, with important implications for other countries currently considering related policies (Svensson 2019; Svensson 2020).⁴¹

Our findings also suggest that proposed macroprudential policies, such as changing amortizing mortgages into interest-only during recessions (Campbell et al. 2019; Kovacs and Moran 2020), are likely to have even larger effects that might be expected in more standard economic models. In contrast to these models, we find no substitutability between debt repayment and non-mortgage savings. This suggests that countercyclical amortization reductions could have consumption and labor responses similar to those seen for mortgage designs with countercyclical interest rates (ex. Guren et al. 2019), but with very different costs and wealth implications. In fact, the labor response we document in this paper is quantitatively similar to what Zator (2019) finds for interest rates changes in Poland, despite different shocks at play.

Our results also have implications for the debate about the benefits of homeownership, with some arguing that that owning a home is the main way in which households accumulate wealth (Li and Yang

⁴¹ This has of course come at the cost of reduced consumption and increased household labor supply. Without further analysis, which is outside the scope of this paper, we cannot make any welfare statements. Contrary to our setting, changes in amortization rules are often accompanied by changes in regulatory DTI limits (ex. Amromin et al. 2018; Dokko et al. 2019), which prior work has shown excludes certain borrowers (Backman and van Santen 2020). Our findings suggest that such exclusions are likely to have substantial causal effects on household wealth accumulation, highlighting the importance of continued research understanding how regulatory constraints can alter borrower composition.

2010). The evidence to support that view is mixed. Homeowners do save more (ex. Belsky & Prakken 2004; Rossi & Sierminksa 2018), but are of course systematically different across observables and likely unobservable dimensions as well. By contrast, Sodini et al. (2017) show that plausibly exogenous variation in homeownership in Sweden had little effect on wealth accumulation. Sodini et al. focus on condos, for which mortgages were close to interest-only, with an average expected repayment period of 186 years in 2007 (Hullgren and Soderberg 2016, Swedish Financial Supervisory Authority Report 2008). Taking their and our results together suggests that homeownership is most likely to lead to wealth accumulation if it is coupled with an amortizing mortgage.⁴² This conclusion is broadly consistent with aggregate statistics showing that households at retirement have substantial illiquid wealth in the form of housing and few liquid financial assets (Kaplan et al. 2014). If true, this may mean that differential access to homeownership, either overall or for specific groups, could help explain historical differences in wealth accumulation between groups, such as between black and white households (Charles and Hurst 2002; Krivo and Kaufman 2004; Appel and Nickerson 2016; Aaronson et al. 2017; Anders 2018; Krimmel 2018; Stein and Yannelis 2020). This has important implications for the debate about the general benefits of stimulating homeownership.

Finally, our results could have implications for the optimal design of retirement programs. Beshears et al. (2019) argue that, under taste shock and present bias, the socially optimal mandatory contribution plan has three accounts: one liquid, one illiquid, and one with early liquidation costs. Homeownership with mortgage amortization fits in the third category, and might be a critical component of the optimal policy to stimulate long-run household wealth accumulation. For pensions, Choukhmane (2019) shows that default autoenrollments appear to substantially raise short-run contributions (ex. Madrian and Shea 2001), but are partially offset by lower contributions later on. While default pension contributions, at least in the U.S., might be relatively easy to intertemporally substitute away⁴³, this is not the case for amortization. Building up additional home equity does not preclude the need to make amortization repayments in subsequent periods. At any point, accessing that additional home equity via home equity withdrawals requires substantial implicit and explicit costs. They also typically require debt-to-income and loan-to-value requirements which may not be met for unemployed or retired households, or during periods of macroeconomic distress. This illiquidity may be costly, but may also be a potential benefit (ex. Beshears et al. 2015b; Beshears et al. 2019b). It could mean that the wealth accumulation we observe might be fairly persistent in the longer run.

⁴² This is not to say that systematic variation in housing asset performance among subgroups could not drive some degree of wealth differences between those groups (ex. Goldsmith-Pinkham and Shue 2020).

⁴³ Beyond intertemporal substitution, retirement account leakage can also be substantial during job transition and around retirement age (Huurd and Panis 2006; Poterba et al. 2013; Argento et al. 2014; Wang et al. 2019).

6. Conclusion

We provide the first empirical evidence on the effects of mortgage amortization (debt repayment) on wealth accumulation by using detailed individual-level administrative data and variation in the timing of purchases by first time 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 near 1-for-1 rise in net worth with the rise in amortization. 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 (ex. 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. Our results are consistent with a relatively atypical rational model of household liquidity preferences, or behavioral models that have been shown to drive wealth accumulation in other settings (such as pensions), including limited commitment to save for the long run, mental accounting, and default settings.

Regardless of the underlying mechanism, 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 policies 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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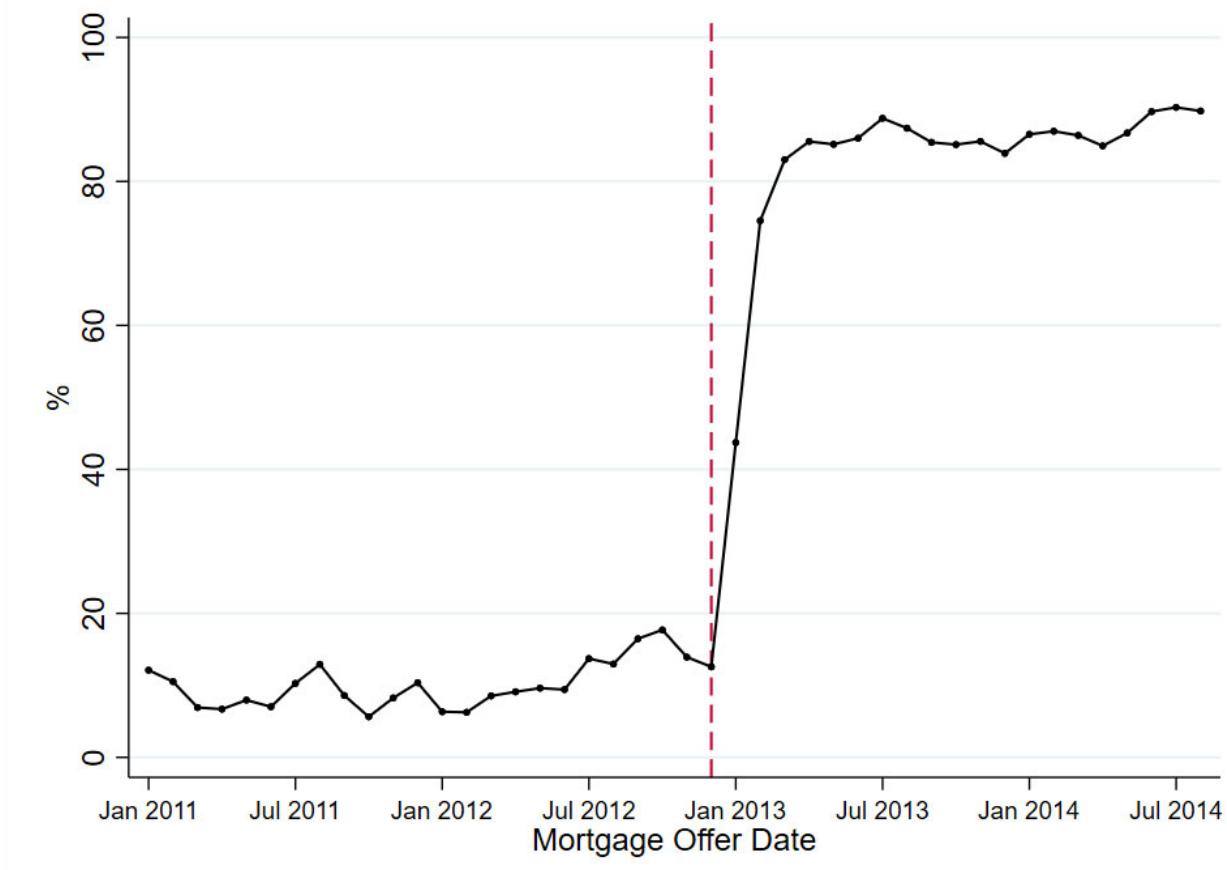
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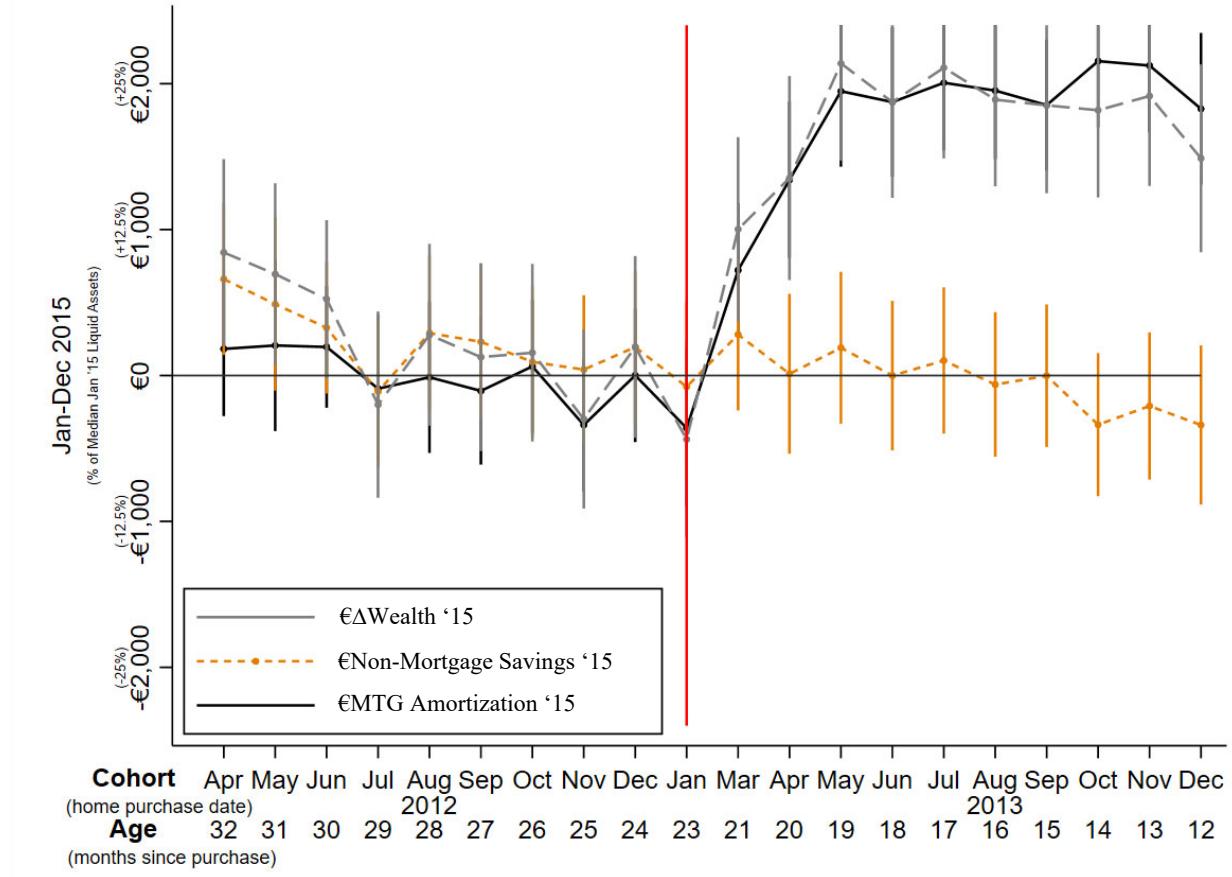
Figure 1. % of New Mortgage Offers Fully Amortizing

This figure shows the % 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 reform examined in this paper discouraging the use of interest-only loans.



**Figure 2. Mortgage Amortization & Wealth Accumulation in 2015
by Date of Home Purchase: 1st Time Home Buyers '12-'13**

This figure shows the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 reform, following Equations 4 and 5 in the paper. The sample includes all first-time home buyers in the Netherlands who closed on their home between April 2012 and December 2013. In particular, we regress mortgage repayment (solid black line), wealth accumulation (gray dashed line), and non-mortgage savings (yellow dotted line), all from January to December 2015, on categorical dummy variables for each cohort (month of closing on the house), where February 2013 is the omitted month. No other control variables are included. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The smaller values in parentheses on the y-axis are the coefficients divided by the median household liquid assets as of the beginning of 2015. This simply scales the effect to demonstrate its economic magnitude and is not in any way part of the actual analysis. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). Confidence intervals are based on heteroskedasticity robust standard errors clustered at the household level.



**Figure 3. Mortgage Amortization & Wealth Accumulation in 2015
by Date of Life-Event: 1st Time Home Buyers '12-'13 w/ Life-Event**

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 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 (ex. birth of a child). In particular, 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), where Q4 2012 and Q1 2013 are the omitted quarters. No other control variables are included. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The smaller values in parentheses on the y-axis are the coefficients divided by the median household liquid assets as of the beginning of 2015. This simply scales the effect to demonstrate its economic magnitude and is not in any way part of the actual analysis. The x-axis includes the cohort (quarter of life-event) and the age (quarters from life-event till the beginning of 2015). Confidence intervals are based on heteroskedasticity robust standard errors clustered at the household level.

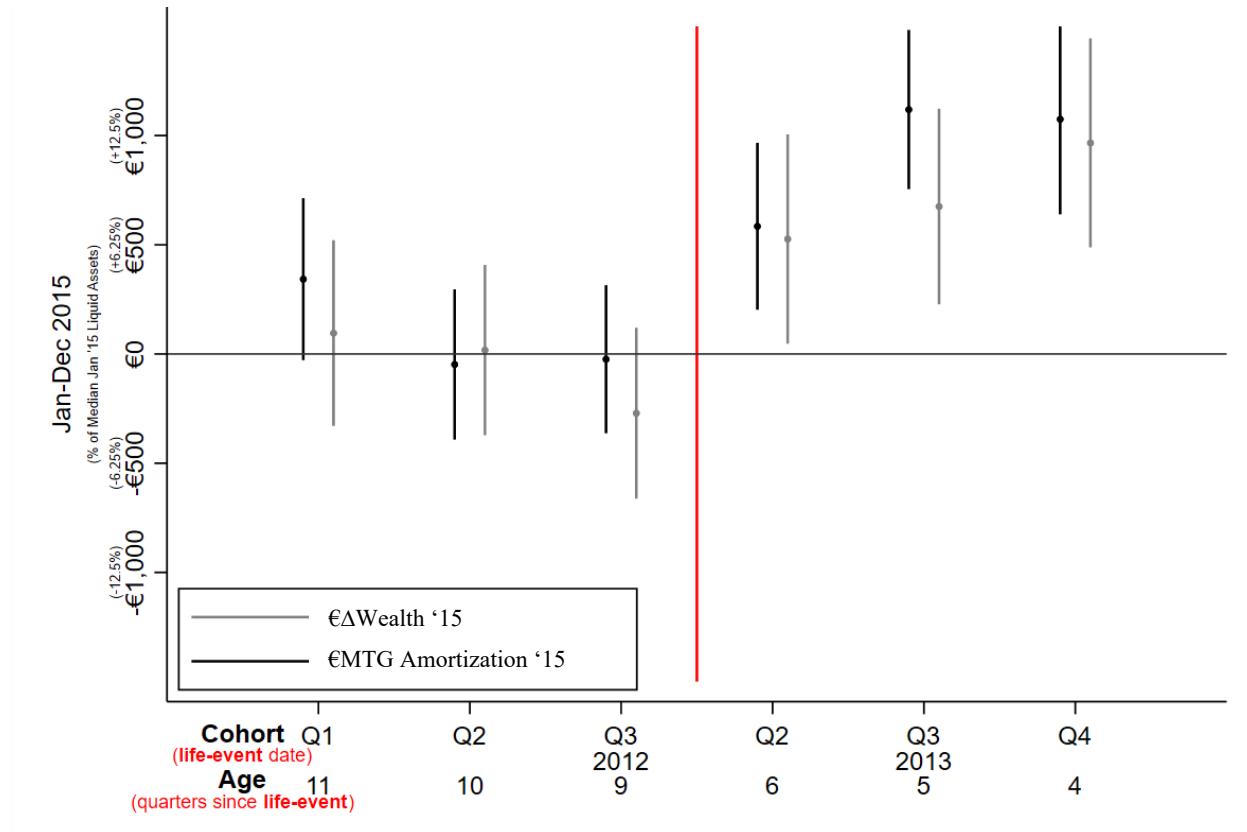


Table 1. Summary Statistics for 1st Time Home Buyers '12-'13 in the Netherlands

These are summary statistics for 2014 from the CBS administrative datasets for first-time home buyers in 2012 and 2013 in the Netherlands, who financed their purchase with a mortgage. This 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 assets = all bank deposits (checking + savings + other) + stocks + bonds + other marketable securities. Δ Financial assets = Δ liquid assets + voluntary pension contributions. Non-mortgage savings = Δ Financial assets - Δ non-mortgage liabilities. We do not have a measure of the level of voluntary pension assets, only the yearly flow so we can only measure financial assets and non-mortgage savings in yearly flows, while liquid assets we can measure in both levels and flows.

	Mean	Median	Stdev	25 th	75 th	N
Mtg LTV Year-end '14	1.02	1.05	0.19	1.01	1.09	111,523
Mtg Balance Year-end '14 (€)	203k	187k	88k	151k	234k	111,523
Total Liabilities Year-end '14 (€)	211k	193k	97k	155k	242k	111,523
Income Year-end '14 (€)	73k	66k	36k	49k	88k	111,523
Liquid assets Year-end '14 (€)	18k	7.8k	34k	2.6k	21k	111,523
Δ Liquid assets Year-end '14-15 (€)	1.3k	0.3k	8.6k	-1.2k	3.4k	111,523
Δ Financial assets Year-end '14-15 (€)	1.5k	0.4k	8.4k	-1.1k	3.6k	111,523
Non-Mtg Savings Year-end '14-15 (€)	1.8k	0.7k	9.7k	-1.5k	4.5k	111,523

Table 2. Mortgage Amortization and Wealth Accumulation

This table shows the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 reform. The sample includes all first-time home buyers who closed on their home between October 2012 and February 2013 (control) and May to September 2013 (treatment). The reform applied to the timing of going under contract, not closing, which typically takes at least two months. It is uncertain whether those who closed in March and April went under contract before or after January 1st. We therefore exclude them. Relative to the sample we use in Figure 2 and Table 2, this is a smaller group buying closer to the reform. Column 1 is the first stage of our two-stage least squares regression. It regresses the amount of mortgage repayment from January to December 2015 for a given household on *Post*, a dummy variable equal to 1 if they closed on their house after May 1st, 2013. Column 2 has the same specification as Column 1, but the dependent variable is wealth accumulation over 2015, and represents our reduced form regression. Column 3 is a combination of Columns 1 and 2, where we formally estimate a two-stage least squares regression using the dummy variable, *Post*, as an instrument for the amount of mortgage repayment in 2015. We estimate the effects on wealth accumulation over the same period. Column 4 has the same specification as Column 3, but looks at only non-mortgage savings as the dependent variable. T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; ***1%.

	1 st Stage (1)	RF (2)	IV (3)	IV (4)
	MTG Repaid '15	ΔWealth '15	ΔWealth '15	Non-MTG Savings '15
Post	2045.0 *** (19.22)	2030.8 *** (14.34)		
MTG Repaid '15			0.993 *** [0.88,1.10] (17.62)	-0.007 [-0.12,0.11] (-0.09)
IV	-	-	Post	Post
F-Stat	-	-	369.3	369.3
Obs	42,468	42,468	42,468	42,468
Adj. R ²	0.020	0.011	0.331	0.002

Table 3. “Paying” for Wealth Accumulation

This table shows how households alter their labor supply in order to pay for the increase in wealth accumulation caused by the rise in mortgage amortization. The sample includes all first-time home buyers who closed on their home between October 2012 and February 2013 (control) and May to September 2013 (treatment). The reform applied to the timing of going under contract, not closing, which typically takes at least two months. It is uncertain whether those who closed in March and April went under contract before or after January 1st. We therefore exclude them. Relative to the sample we use in Figure 2 and Table 2, this is a smaller group buying closer to the reform. Column 1 regresses the change in household gross income from 2012 to 2015 for a given household on *Post*, a dummy variable equal to 1 if they closed on their house after May 1st, 2013. Column 1 is a reduced form estimate of the effect of mortgage amortization on changes in household income. In Column 2, we re-run the two-stage least squares regression from Table 2, replacing the dependent variable with the change in gross household income from 2012 to 2015. Column 3 has the same specification as Column 2, but includes fixed effects for municipality and controls for the natural log of household 2010 income and liquid assets. Columns 4 and 5 have the same specification as Column 3, but look at 2012 and 2015 gross household income as dependent variables, respectively. To facilitate comparison, Column 6 replicates the estimates from Table 2, Column 3 (with the change in wealth accumulation over 2015 as dependent variable) including municipality fixed effects and additional controls. T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; *** 1%.

	RF (1) ΔIncome '15-'12	IV (2) ΔIncome '15-'12	IV (3) ΔIncome '15-'12	IV (4) Income '12	IV (5) Income '15	IV (6) ΔWealth '15
Post	1270.1*** (7.71)					
MTG Repaid '15		0.621*** [0.38,0.87] (4.97)	0.576*** [0.29,0.90] (3.83)	-0.119 [-0.39,0.04] (-1.60)	0.457** [0.04,0.79] (2.20)	1.022*** [0.92,1.13] (18.91)
Muni FE	N	N	Y	Y	Y	Y
Add. Cntrls	N	N	Y	Y	Y	Y
IV	-	Post	Post	Post	Post	Post
F-Stat	-	369.3	141.6	141.6	141.6	355.7
Obs	42,468	42,468	40,352	40,352	40,352	42,409
Adj. R ²	0.001	0.001	-0.046	-0.015	-0.005	0.319

Table 4. Instrumenting for Timing of Purchase w/ Date of Life-Event

This table shows the effect of mortgage amortization on wealth accumulation using the timing of a “life-event” as an instrument for the timing of home purchase around the 2013 reform. Life-events are defined to be months with changes in the number of members of a household (ex. birth of a child). With the exception of Column 7, 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. This is an intent-to-treat analysis. Unlike Table 2 and 3, we are therefore less concerned about including households closing in March and April for whom it is uncertain whether they went under contract before or after the reform. Columns 1-3 are covariate balance tests to show that the timing of the life-event does not appear correlated with pre-reform household characteristics. In particular, Column 1 regresses gross household income in 2010 on the *Post(life event)* dummy variable that equals 1 if the household had a life-event after March 1st, 2013. The control group are all buyers with a life-event from November 2012 to February 2013, while the treated are those with a life-event from March to September 2013. Columns 2 and 3 have the same specification as Column 1, but with 2010 non-mortgage savings or wealth accumulation as dependent variable, respectively. Columns 4 through 6 also have the same specification as Column 1. Columns 4 uses the amount of mortgage repayment from January to December 2015 as the dependent variable. This is the first stage of the two stage least square regressions in this table. Column 5 uses wealth accumulation in 2015 as dependent variable. This is the key reduced form regression of this table. Column 6 uses the percent increase in the assessed value of the house over 2015 as dependent variable. Column 7 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 occurs after, relative to before the reform. Columns 8 gives the formal two-stage least squares regression using post-reform life-events to instrument for mortgage repayment in 2015. It uses wealth accumulation over the same period as dependent variable. Column 9 is the same as Column 8, but excludes any life-events that happen in the same month the household moves. Column 9 also excludes households with a life-event in March. A substantial fraction of these households likely went under contract before the reform and this reduces the power of the first stage, raising concerns about statistical bias. T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; *** 1%.

	Covariate Balance Tests			1 st Stage		RF	RF	RF	IV	IV
	(1) HH Income '10	(2) ΔFinancial Assets '10	(3) ΔWealth '10	(4) MTG Repaid '15	(5) ΔWealth '15	(6) %ΔHome Value '15	(7) Have Real Estate '16	(8) ΔWealth '15	(9) ΔWealth '15	
MTG Repaid '15								0.864*** [0.54,1.19] (5.26)	0.931*** [0.41,1.45] (3.52)	
Post(life event)	-249.5 (-0.36)	-57.89 (-0.17)	383.1 (0.32)	792.8*** (4.60)	685.2*** (3.10)	0.00261 (0.02)	-0.00002 (-0.01)			
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	382,374	16,581	11,363	
Adj. R ²	-0.000	-0.000	-0.000	0.003	0.001	-0.000	-0.000	0.355	0.360	

Table 5. Year-end Effects, Persistence & Convexity of Amortization Schedule

This table analyzes the effect of increased amortization on wealth accumulation using different samples, periods and specifications. All columns report estimates from two-stage least square regressions using variation in the timing of home purchase for first-time home buyers around the 2013 reform as instrument. Column 1 replicates the regression from Table 2, Column 3, but restricts the sample to the subset of first-time home buyers who closed on their properties in the first four months of 2013. In particular, the dependent variable is wealth accumulation over 2015 and the endogenous variable is mortgage repayment over the same period, instrumented with the dummy variable, *Post*, that is equal to 1 if a buyer closed on their house after March 1st, 2013. The control group are all buyers who closed on their homes from January to February 2013, while the treated are those who closed from March to April 2013. Columns 2 and 3 also have the same specification as Table 2, Column 3 but focus on mortgage amortization and wealth accumulation over 2016 and 2014 (rather than 2015), respectively. As before, the Column 2 restricts the sample to first-time home buyers buying a home between October 2012 and September 2013, omitting those who closed in March and April 2013. Column 3 also excludes households who closed in September and October 2013. Households tend to save and dissave substantial sums in the months around purchasing a home. This means that households that closed near the end of 2013 display systematically different savings patterns in 2014 than households closing at an earlier date. Column 4 provides a delta-in-delta estimate. The dependent variable is the wealth accumulation from January to December 2016 minus the wealth accumulation from January to December 2014 and the endogenous variable is mortgage repayment from January to December 2016 minus mortgage repayment from January to December 2014. To boost power for a valid first stage the column has a slightly larger sample of first-time home buyers buying a home between July (rather than October) 2012 and July 2013 and including transactions right around the reform itself in March and April 2013. We also exclude cases with very large changes in the mortgage repayment for 2016 relative to 2014 ($\Delta\text{MTG Repaid } 2016 - 2014 > €20k$), so we can focus on variation coming from convexity and in doing so improve power for validity of the first-stage. T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; *** 1%.

	IV (1) ΔWealth '15	IV (2) ΔWealth '16	IV (3) ΔWealth '14	IV (4) $\Delta\Delta\text{Wealth}$ '16-'14
MTG Repaid '15	1.182*** [0.82,1.55] (6.32)			
MTG Repaid '16		0.936*** [0.82,1.05] (15.49)		
MTG Repaid '14			0.887*** [0.50,1.28] (4.49)	
$\Delta\text{MTG Repaid}$ '16-'14				1.115** [0.11,2.12] (2.17)
Control Group	1/13-2/13	10/12-2/13	10/12-2/13	7/12-2/13
Treated Group	3/13-4/13	5/13-9/13	5/13-7/13	3/13-7/13
IV	Post	Post	Post	Post
Exclude Large ΔRepaid	-	-	-	Y
F-Stat	37.4	428.9	145.5	22.7
Obs	15,223	38,741	35,148	36,650
Adj. R ²	0.259	0.326	0.098	0.062

Table 6. Not Just Non-Saving Households

This table restricts the analysis to households with significant savings. Columns (1)-(3) replicate the two-stage least squares regression in Table 2, Column 3, but restrict the sample to the subset of buyers with (1) a loan-to-value ratio below 90% as of the end of 2014, (2) a loan-to-gross household income ratio below 4 at the end of 2014, and (3) at least €10k in liquid financial assets at the end of 2015 or an increase in liquid assets by at least €3k in 2015, respectively. Columns 4 and 5 look at the same sub-group of buyers in Table 2, Column 3, but the dependent variable is household liquid assets at the end of 2011 or 2015, respectively. We regress this on a dummy variable that is equal to 1 if the household has more than €10k in liquid financial assets at the end of 2011. Column 6 has the same specification as Column 1 of this table, but restricts the sample to the subset of households with at least €10k in liquid financial assets at the end of 2011. T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; *** 1%.

	IV (1) ΔWealth '15	IV (2) ΔWealth '15	IV (3) ΔWealth '15	OLS (4) Fin. Asset '11	OLS (5) Fin. Asset '15	IV (6) ΔWealth '15
MTG Repaid '15	1.315*** [0.91,1.72] (6.37)	0.959*** [0.82,1.10] (13.76)	0.997*** [0.84,1.15] (12.80)			0.956*** [0.84,1.07] (15.87)
FinAsset'11>10k				43,445*** (96.66)	26,486*** (81.06)	
LTV '14	<0.9	-	-	-	-	-
LTI '14	-	<4	-	-	-	-
FinAsset'15	-	-	>10k >3k	-	-	-
FinAsset'11	-	-	-	-	-	>10k
IV	Post	Post	Post	-	-	Post
F-Stat	32.5	265.5	223.0	N/A	N/A	350.3
Obs	5,762	27,569	22,005	42,468	42,468	17,268
Adj. R ²	0.202	0.328	0.252	0.243	0.173	0.302

Table 7. Pervasive Effects by Age

This table looks at effects for households with a different age. All columns replicate the two-stage least squares regression in Table 2, Column 3. Columns (1)-(3) restrict the sample to the subset of buyers where the oldest member of the household is older than 30, 40 or 50 years old as of the end of 2015, respectively. We exclude households where the oldest member is older than 75 years. Column 4 has the same specification as Column 3, but excludes multi-generation households by omitting any household where the maximum age difference between any two adults is more than 20 years. T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; *** 1%.

	IV (1) ΔWealth '15	IV (2) ΔWealth '15	IV (3) ΔWealth '15	IV (4) ΔWealth '15
MTG Repaid '15	0.986*** [0.86,1.11] (15.24)	1.074*** [0.86,1.28] (10.04)	1.077*** [0.70,1.46] (5.55)	1.272*** [0.76,1.79] (4.87)
Age	>30	>40	>50	>50
GParentFilt	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
Adj. R ²	0.327	0.301	0.289	0.177

APPENDIX For Online Publication

Figure A1. Variability in Liquid Wealth Accumulation

We compute the yearly change in liquid assets for the full sample of first-time home buyers from Table 1 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 assets on dummy variables for the years before or after the decline in income. We include both household and year fixed effects. Vertical lines give 95% confidence intervals that are based on heteroskedasticity robust standard errors clustered at the household level.

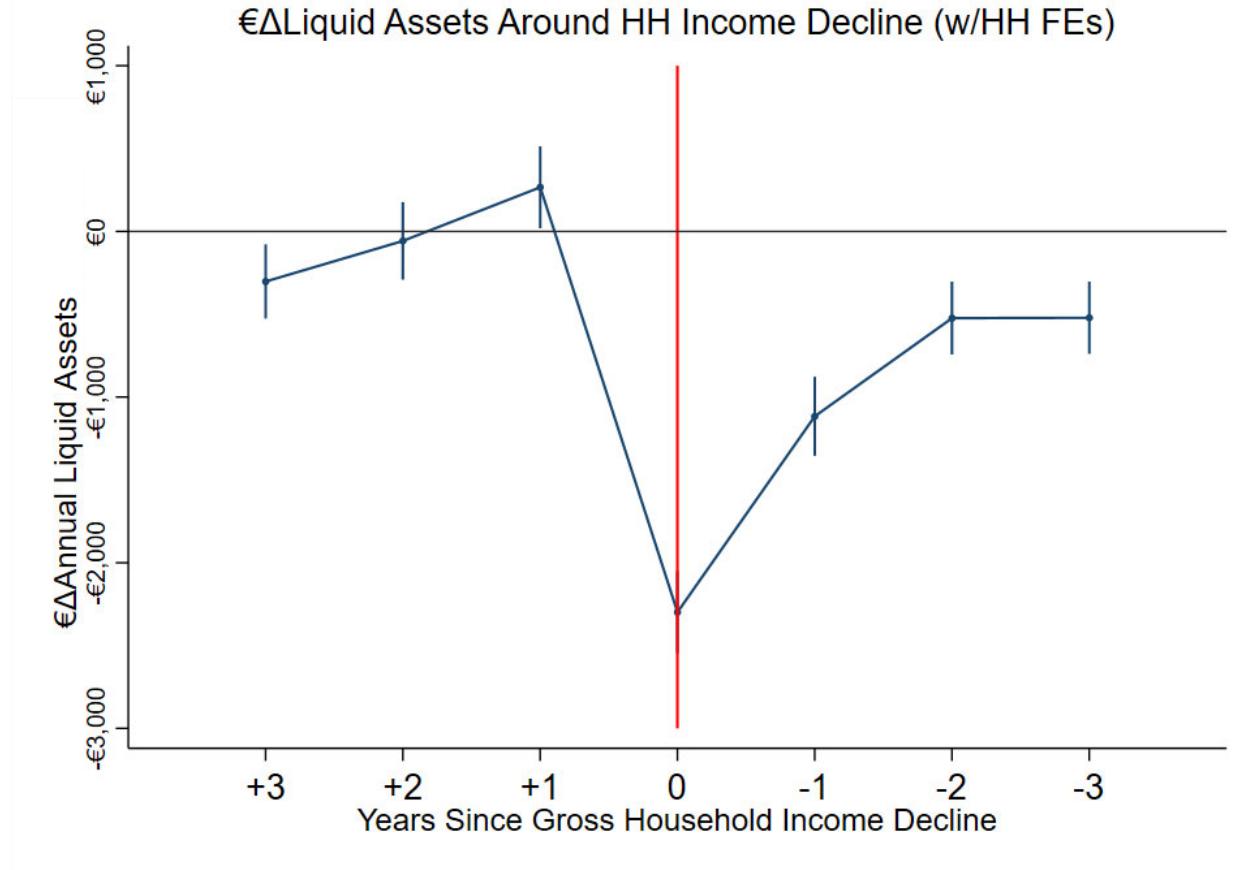
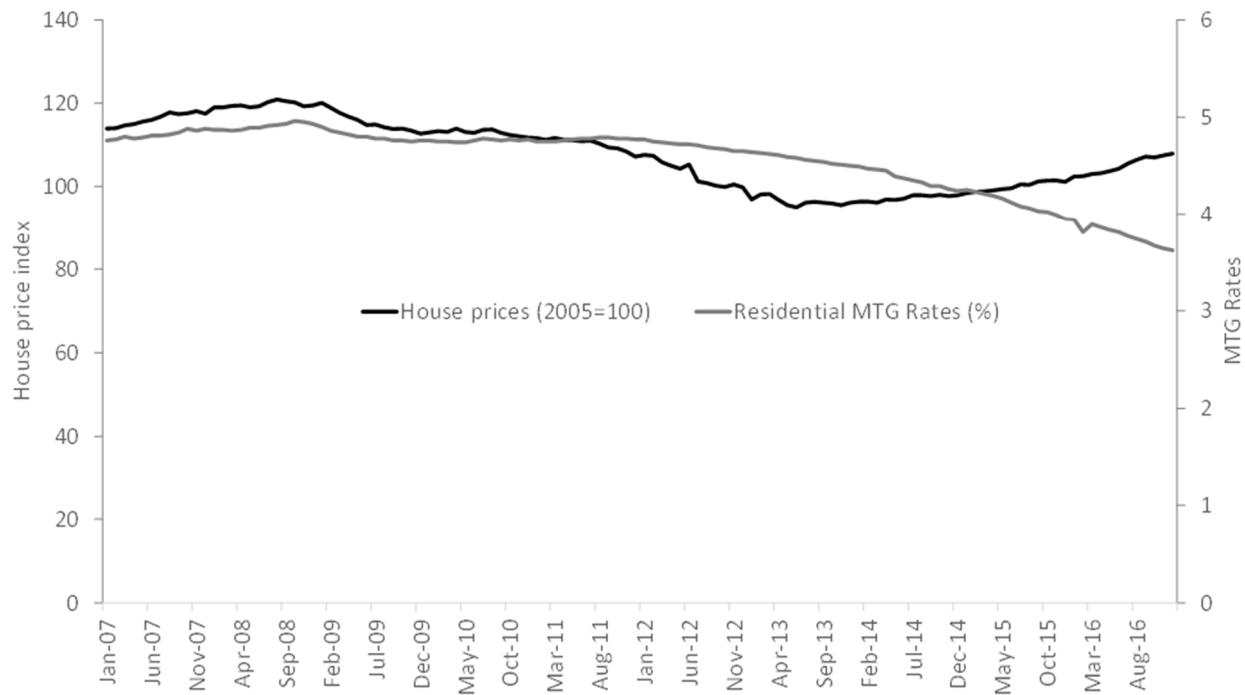


Figure A2. Dutch Macroeconomic Housing Statistics '07-'16

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.



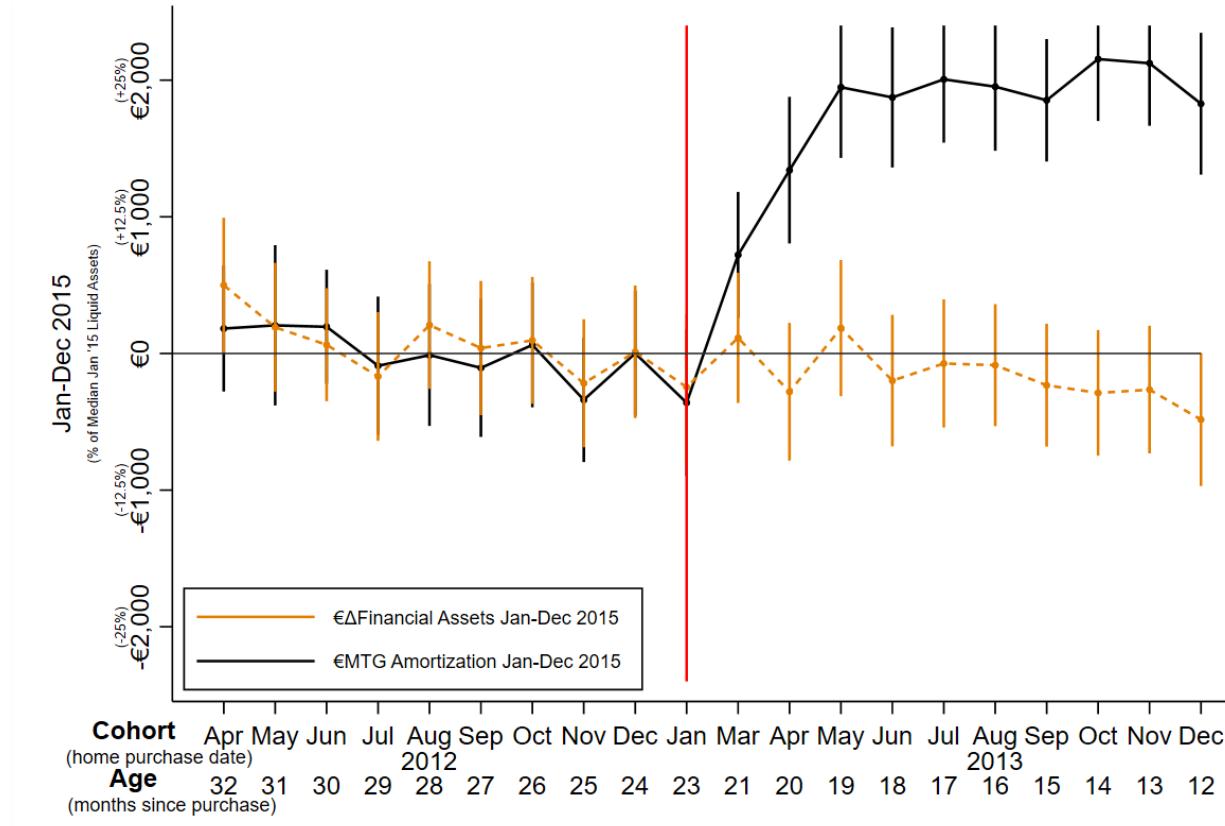
**Figure A3. Origination Loan-to-Value and Loan-to-Income (mean)
by Mortgage Offer Date for First-Time Homebuyers 2012-2013**

This figure depicts the average (mean) origination loan-to-value and loan-to-income of mortgage offers for first-time homebuyers in 2012 and 2013 by mortgage offer dates. Typically the period between Data come from HDN and cover about 3/4s of mortgage offers as of December 2014 (see Data section of paper for more details). 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).



Figure A4. Mortgage Amortization in 2015 & Δ Financial Assets by Date of Home Purchase: 1st Time Home Buyers '12-'13

This figure shows the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 reform. The sample includes all first-time home buyers in the Netherlands who closed on their house between April 2012 and December 2013. In particular, we regress mortgage repayment (solid black line), and change in financial assets (yellow dotted line), both from January to December 2015, on categorical dummy variables for each cohort (month of closing on the house), where February 2013 is the omitted month. No other control variables are included. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The smaller values in parentheses on the y-axis are the coefficients divided by the median household liquid assets as of the beginning of 2015. This simply scales the effect to demonstrate its economic magnitude and is not in any way part of the actual analysis. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). Confidence intervals are based on heteroskedasticity robust standard errors clustered at the household level.



**Figure A5. Mortgage Amortization in 2015 & Δ Non-MTG Liab.
by Date of Home Purchase: 1st Time Home Buyers '12-'13**

This figure shows the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 reform. The sample includes all first-time home buyers in the Netherlands who closed on their house between April 2012 and December 2013. In particular, we regress mortgage repayment (solid black line), and change in non-mortgage liabilities (yellow dotted line), both from January to December 2015, on categorical dummy variables for each cohort (month of closing on the house), where February 2013 is the omitted month. No other control variables are included. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The smaller values in parentheses on the y-axis are the coefficients divided by the median household liquid assets as of the beginning of 2015. This simply scales the effect to demonstrate its economic magnitude and is not in any way part of the actual analysis. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). Confidence intervals are based on heteroskedasticity robust standard errors clustered at the household level.

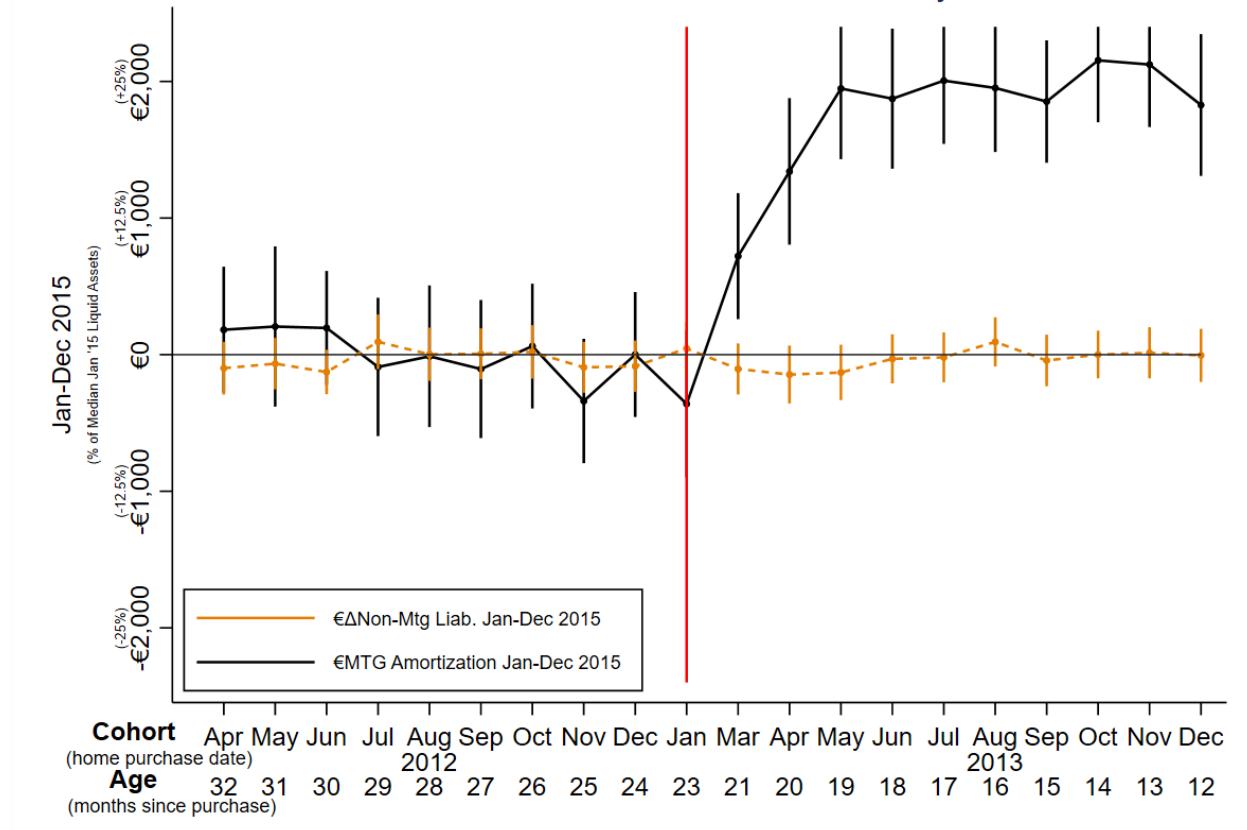


Figure A6. Mortgage Amortization in 2015 & # of Transactions by Date of Closing: 1st Time Home Buyers '12-'13

This figure shows compares the increase in mortgage induced by the 2013 reform with the number of housing transactions in the same period. The sample includes all first-time home buyers in the Netherlands who closed on their house between April 2012 and December 2013. In particular, we regress mortgage repayment (solid black line) from January to December 2015 on categorical dummy variables for each cohort (month of closing on the house), where February 2013 is the omitted month. No other control variables are included. We also plot just the number of housing transactions for each cohort (brown solid line) in each month on the second (right) y-axis. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). Confidence intervals are based on heteroskedasticity robust standard errors clustered at the household level.

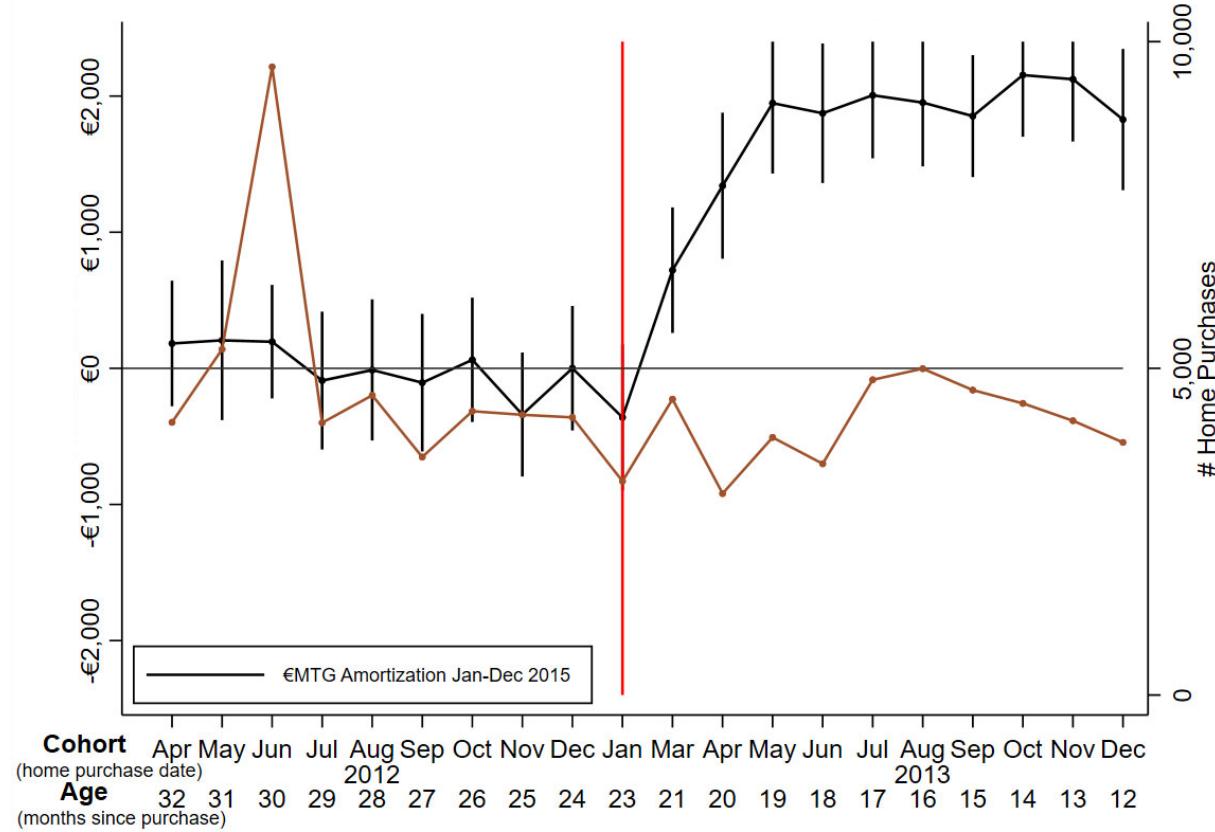


Figure A7. First-Time Homebuyers vs. All Other Buyers

This figure shows that the 2013 reform had a differential treatment effect for first-time homebuyers (FTHBs) vs. all other buyers (non-FTHBs). At the same time, there are no sharp jumps in other variables around that date, neither in absolute terms or relative to each other. We include all households buying a house between April 2012 and December 2013. FTHBs (solid black lines) and non-FTHBs (dashed yellow lines) are households without or with real estate or a mortgage in the two years prior to the reform, respectively. In panel A, we regress the % of the observed mortgage balance reduction, not including any amortization for linked accounts, from January to December 2015 on categorical dummy variables for each cohort (month of closing on the house), where February 2013 is the omitted month. No other control variables are included. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The x-axis has the cohort (month of closing). Panels B-H are the same as Panel A, but have different dependent variables: (B) the initial home purchase price in thousands of euros, (C) the change in the natural log of liquid assets over 2015, (D) the change in liquid assets over 2015 divided by the mortgage balance at the end of 2014, (E) the natural log of liquid assets as of Dec 2010, (F) the natural log of household gross income as of Dec 2010, (G) the change in the natural log of liquid assets over 2011, and (H) the change in the natural log of household gross income over 2011. Confidence intervals are based on heteroskedasticity robust standard errors clustered at the household level.

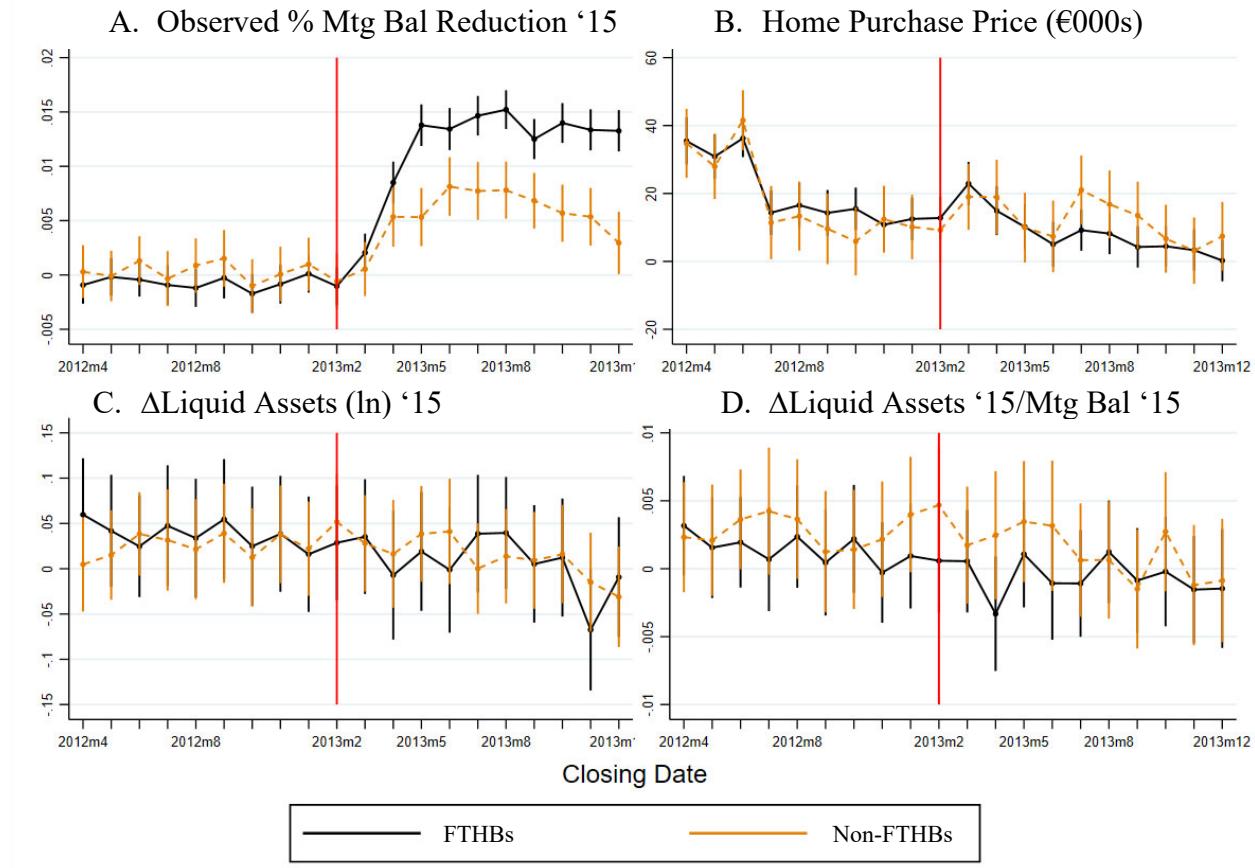
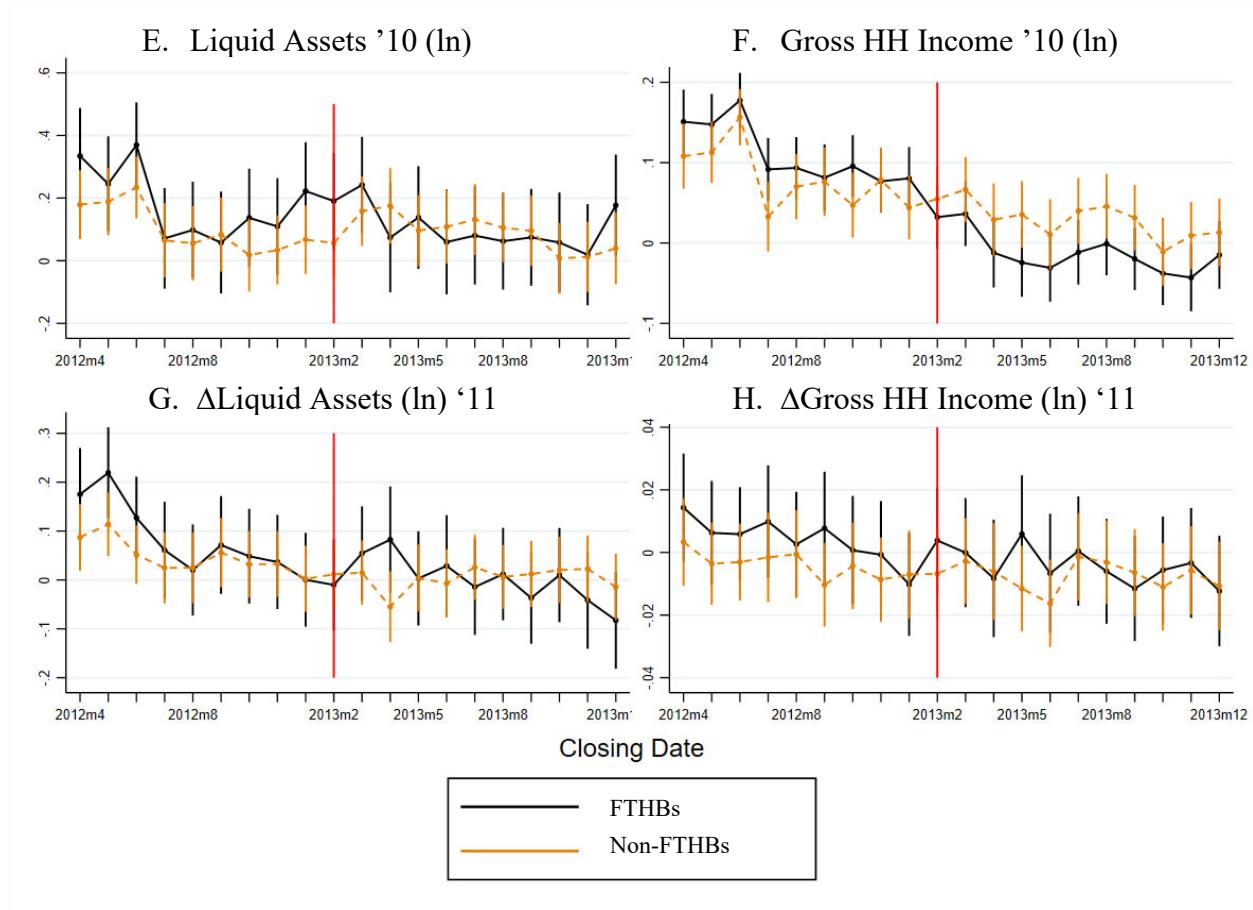


Figure A7. First-Time Homebuyers vs. All Other Buyers (Cont.)



**Figure A8. Probability of Homeownership by Dec-2016:
All Households w/ Life-Events 2012-2013**

This figure shows that there is no observable effect of life-events on the probability of ever owning a house during our sample period. We look at all households with a life-event between 2012 and 2013 who do not own a home at the end of 2011. We do not require them to become a first-time homebuyer during this period. 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 (ex. birth of a child). The vertical lines show 95% confidence intervals which are based on heteroskedasticity robust standard errors clustered at the household level

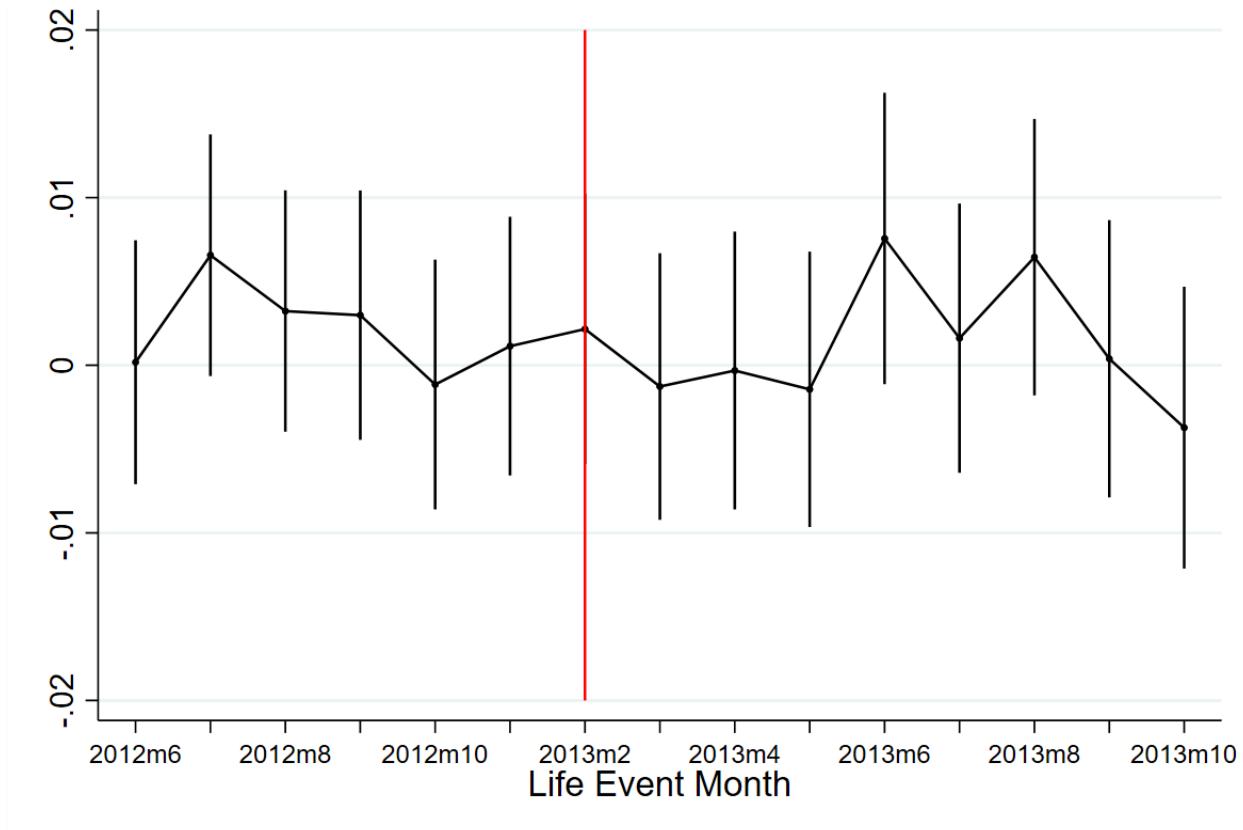


Figure A9. Non-First-Time Homebuyers by Previous Housing Tenure Length

This figure looks at the effects of the 2013 reform on non-first-time homebuyers (non-FTHBs) with shorter previous housing spells (<10 yrs – solid black lines) vs. those that were longer (≥ 10 yrs – dashed yellow lines). There are no sharp jumps in any of the relevant variables around that date, neither in absolute terms or relative to each other. We use the full set of all non-FTHBs buying a house between April 2012 and December 2013. In panel A, we regress the % of the observed mortgage balance reduction, not including any amortization for linked accounts, from January to December 2015 on categorical dummy variables for each cohort (month of closing on the house), where February 2013 is the omitted month. No other control variables are included. Panels B-D are the same as Panel A, but have different dependent variables: (B) the initial home purchase price in thousands of euros, (C) the change in the natural log of liquid assets over 2015, and (D) the change in liquid assets over 2015 divided by the mortgage balance at the end of 2014 (D). Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The x-axis includes the cohort (month of closing). Confidence intervals are based on heteroskedasticity robust standard errors clustered at the household level.

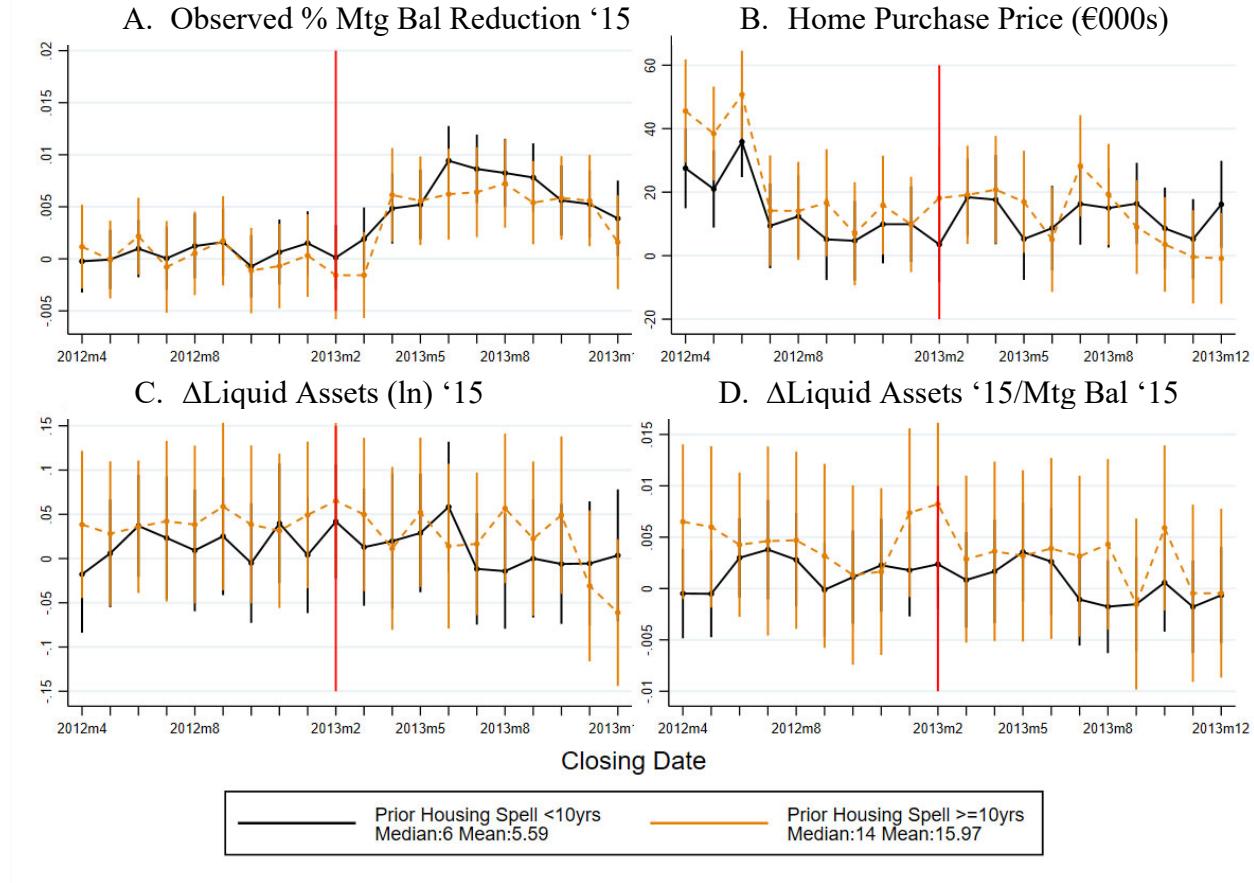
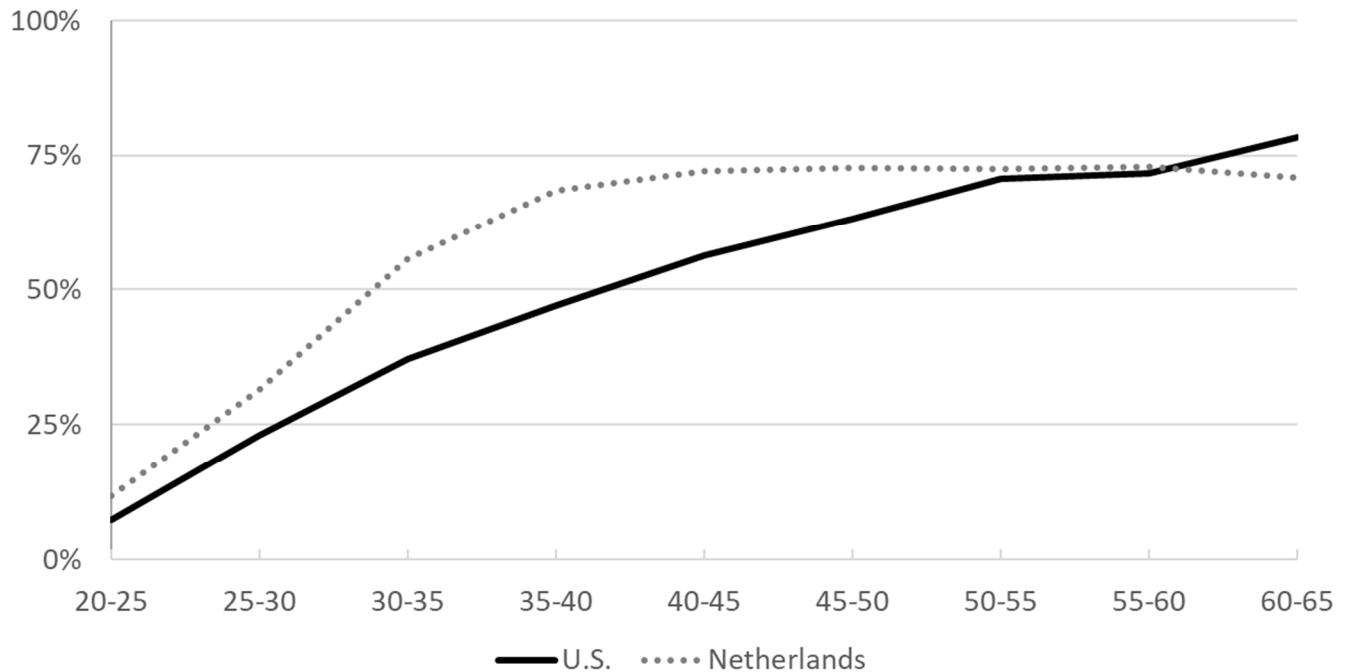


Figure A10. Dutch vs. U.S. Homeowners by Age

Panel A depicts the percent of household heads who report having real estate by 5-year age group categories between 20 and 65. Panel B reports the percent of homeowners that are “hand-to-mouth” – those without significant levels of liquidity (<\$10 USD/€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. % of Homeowners w/ an Outstanding Mortgage Balance Remaining

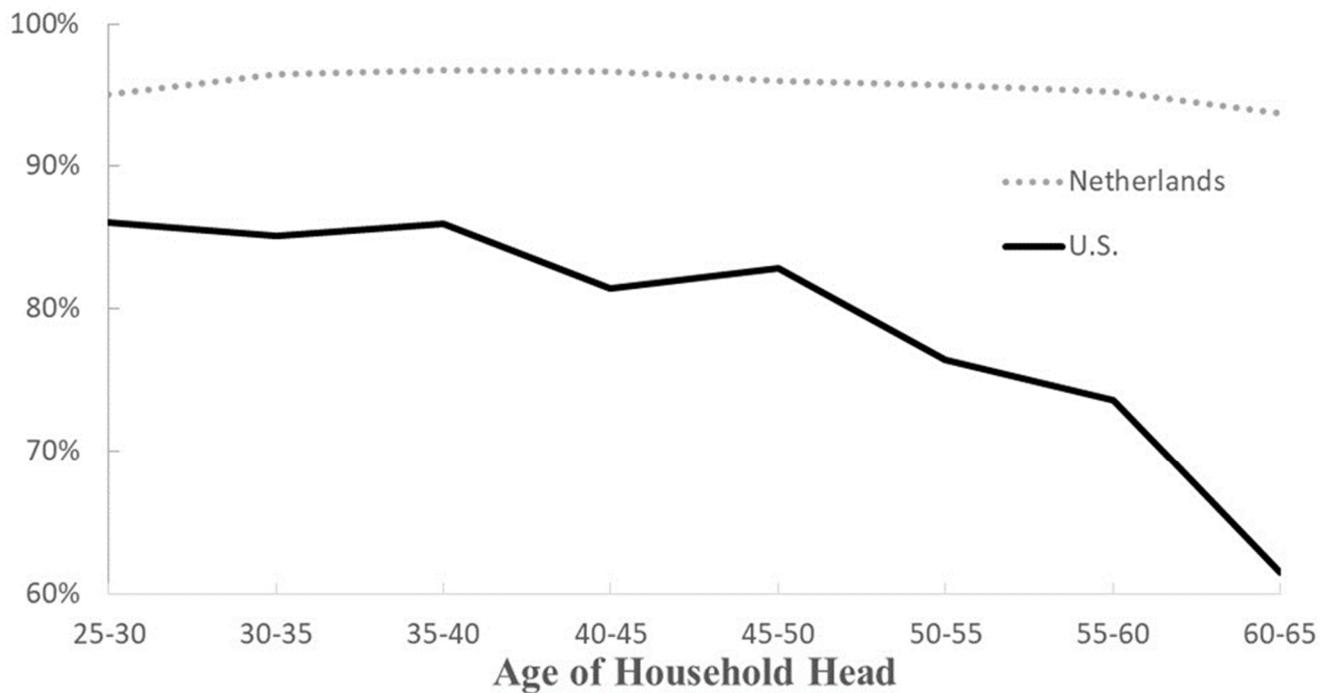


Table A1. Robustness: Alternative Wealth Measures

This table shows that the effect of mortgage amortization on wealth accumulation is robust to the specific measure of wealth used. Columns 1-5 replicate the two-stage least squares regression in Table 2, Column 3, but use different wealth measures to construct the dependent variable. Column 1 uses an alternative for voluntary pension contributions. In particular, it includes all pension contributions no matter their size and drops any instances of missing values (as opposed to setting them to 0 as is done in the main specification). Column 2 includes the appraised value of real estate in the measure of wealth. Column 3 combines Columns 1 and 2. Column 4 looks at the levels (rather than changes) of household net worth (all assets – liabilities) and home equity, both as of December 2015. Column 5 also looks at the levels of household net worth and home equity. This column replicates the specification from Table 4, Column 8 and uses life-events as instrument. T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; *** 1%.

	IV (1)	IV (2)	IV (3)	IV (4)	IV (5)
	ΔWealth '15	ΔWealth '15	ΔWealth '15	Net Worth '15	Net Worth '15
MTG Repaid '15	0.921*** [0.78,1.06] (13.18)	1.232*** [0.98,1.49] (9.47)	1.167*** [0.90,1.43] (8.57)		
MTG Repaid '14+'15					
Home Equity '15				0.970*** [0.88,1.06] (21.85)	0.983*** [0.62,1.35] (5.26)
Pension Alt. Measure	Y	-	Y	-	-
Include Real Estate	-	Y	Y	Y	Y
IV	Post(buy)	Post(buy)	Post(buy)	Post(buy)	Post(life)
F-Stat	378.0	369.3	378.0	472.5	27.0
Obs	41,559	42,468	41,559	42,468	16,581
Adj. R ²	0.316	0.126	0.119	0.663	0.656

Table A2. Robustness: Alternative Samples

This table shows that the effect of mortgage amortization on wealth accumulation is robust to the sample used. All columns replicate the two-stage least squares regression from Table 2, Column 3, but include larger samples. Columns 1 includes all wealth changes (not just those <€100k as in the main analysis of the paper). Column 2 includes all mortgage changes (not just those where the year-over-year % change is <30% as in the main analysis of the paper). Column 3 includes all households and observations regardless of size and previous home-owner status as long as they purchase a home during the period of interest. This means including all the observations in Columns 1 and 2, and non-first-time homebuyers. T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; *** 1%.

	IV (1) ΔWealth '15	IV (2) ΔWealth '15	IV (3) ΔWealth '15
MTG Repaid '15	1.013*** [0.87,1.12] (13.65)	0.976*** [0.85,1.12] (14.82)	1.000*** [0.92,1.08] (24.49)
Include large wealth Δs	Y	-	Y
Include large mtg %Δs	-	Y	Y
Include all	-	-	Y
IV	Post(buy)	Post(buy)	Post(buy)
F-Stat	229.9	143.3	35.1
Obs	42,666	44,555	113,231
Adj. R ²	0.418	0.615	0.944

Table A3. Robustness: Alternative Amortization Assumptions

This table shows that the effect of mortgage amortization on wealth accumulation is robust to the amortization assumptions used for linked mortgage accounts. All columns replicate the two-stage least squares regression in Table 2, Column 3, but make different assumptions to impute the unobserved amortization through linked savings accounts for mortgages contracted before the reform. Column 1 simply uses the raw data (no adjustment). Columns 2-5 assume that pre-reform mortgages have a larger or smaller amortizing component than the 50% we assume in the main analysis: 30%, 40%, 60%, and 70%, respectively. Column 6 and 7 assume an interest rate on pre-reform mortgages and their linked savings accounts that is higher or lower than the 4.5% we assume in the main text: 6.0 and 3.0%, respectively (this matters for the speed of amortization). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses (brackets). P-Values: * 10%; ** 5%; *** 1%.

	IV (1) ΔWealth '15	IV (2) ΔWealth '15	IV (3) ΔWealth '15	IV (4) ΔWealth '15	IV (5) ΔWealth '15	IV (6) ΔWealth '15	IV (7) ΔWealth '15
MTG Repaid '15	0.996*** [0.92,1.07] (27.03)	0.994*** [0.90,1.09] (21.37)	0.994*** [0.90,1.09] (19.49)	0.993*** [0.87,1.12] (15.74)	0.993*** [0.85,1.13] (13.88)	0.994*** [0.90,1.10] (19.62)	0.994*** [0.90,1.09] (15.25)
% of pre-reform mortgage that is amortizing – assumed interest rate – assumed	0	30%	40%	60%	70%	50%	50%
IV	N/A	4.5%	4.5%	4.5%	4.5%	6.0%	3.0%
F-Stat	853.4	541.4	451.5	294.9	228.6	457.6	276.3
Obs	42,468	42,468	42,468	42,468	42,468	42,468	42,468
Adj. R ²	0.342	0.333	0.333	0.330	0.331	0.332	0.330

Table A4. Robustness: Standard Errors

This table shows that the effect of mortgage amortization on wealth accumulation is robust to the specific method of computing standard errors. All columns replicate the two-stage least squares regression in Table 2, Column 3, but make different assumptions about the error structure – in the main analysis we cluster at the household-level. Column 1 computes heteroskedasticity robust standard errors without any clustering. Columns 3 and 4 cluster at the level of four digit postal code (PC4) or municipality (*gemeente*), respectively. Column 4 collapses the data at the level of the household head and clusters standard errors at the municipality level. P-Values: * 10%; ** 5%; ***1%.

	IV (1) ΔWealth '15	IV (2) ΔWealth '15	IV (3) ΔWealth '15	IV (4) ΔWealth '15
MTG Repaid '15	0.993*** [0.92,1.07] (25.97)	0.993*** [0.89,1.11] (17.29)	0.993*** [0.88,1.11] (17.26)	0.978*** [0.87,1.09] (18.00)
Standard Error Clustering	None (robust)	PC4	Muni	Muni
Collapse	-	-	-	HH-level
IV	Post(buy)	Post(buy)	Post(buy)	Post(buy)
F-Stat	847.7	336.3	322.3	458.1
Obs	42,468	42,468	42,468	25,248
Adj. R ²	0.331	0.331	0.331	0.321

Table A5. Labor Supply: # of HH Earners

This table shows that the number of earners within a household increase in order to pay for the increase in wealth accumulation caused by the additional mortgage amortization. The sample includes all first-time home buyers who closed on their home between October 2012 and September 2013. Column 1 looks at the change in the number of household members who are reported to work at least an average of 10 hours per week over a given year from 2012 to 2015 for a given household. We regress this on *Post*, a dummy variable equal to 1 if a household closed on their house after May 1st, 2013. The control group are all buyers who closed on their homes from October 2012 to February 2013, while the treated are those who closed from May to September 2013. The reform applied to the timing of going under contract, not closing, which typically takes at least two months. It is uncertain whether those who closed in March and April went under contract before or after January 1st. We therefore exclude them. Column 2 restricts the sample to the subset of households with at least two working age people living in the household as of 2012. Column 3 uses the same sample as Column 2, but looks at the change in a dummy variable equal to 1 if there is only a single earner in the household. Column 4 is the same as column 3, but restricts the sample to households who experience a change from single earner to not, or the reverse. T-statistics with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses. P-Values: * 10%; ** 5%; *** 1%.

	OLS (1)	OLS (2)	OLS (3)	OLS (4)
	$\Delta\#HH$ Earners '15-'12	$\Delta\#HH$ Earners '15-'12	ΔSingle Earner HH '15-'12	ΔSingle Earner HH '15-'12
Post	0.0239*** (3.36)	0.0299*** (2.65)	-0.0223*** (-2.60)	-0.146*** (-2.65)
>1 Working Age in HH	-	Y	Y	Y
Chg in #Single Earner	-	-	-	Y
F-Stat	-	369.3	141.6	141.6
Obs	42,468	24,424	24,424	3,805
R ²	0.001	0.001	0.001	0.005
Mean '12 Dep Var	1.38	1.69	0.27	0.48

Table A6. Labor Supply: Hours Worked

This table shows that households increase the number of hours worked to pay for the increase in wealth accumulation caused by the rise in mortgage amortization. The rise in hours worked explains all of the observed future rise in household gross income. Column 1 regresses 2012 household gross income for first-time homebuyers in our main sample on their total household hours worked in 2012. Column 2 has the same specification as Column 1, but looks at changes in gross household income and household hours worked from 2012 to 2015. Columns 3 and 4 use the change in total household hours worked (either in level or in logs) as dependent variable and regress this on *Post*, a dummy variable equal to 1 if a household closed on their house after May 1st, 2013. Column 5 replicates the estimates from Table 3, Column 1, but includes a control for the change in total household hours worked from 2012 to 2015 – this explains virtually all of the income increase. T-statistics with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses. P-Values: * 10%; ** 5%; *** 1%.

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
Income '12	ΔIncome '15-'12	$\Delta\text{Hrs Worked}$ '15-'12	$\Delta\text{Hrs Worked}$ '15-'12 (ln)	$\Delta\text{Hrs Worked}$ '15-'12 (ln)	ΔIncome '15-'12
Post			86.12*** (8.35)	0.0492*** (3.22)	364.2 (1.59)
$\Delta\text{Hrs Worked}$ '15-'12		10.54*** (40.79)			10.52*** (40.62)
Hrs Worked '12	15.64*** (74.63)				
Obs	42,468	42,468	42,468	42,468	42,468
R ²	0.310	0.175	0.004	0.000	0.175

Table A7. Four-Year Cumulative Effects

This table shows that the 2013 reform's effect on mortgage amortization and wealth accumulation is highly persistent. The sample includes all first-time home buyers who closed on their home between October 2012 and July 2013. The reform applied to the timing of going under contract, not closing, which typically takes at least two months. It is uncertain whether those who closed in March and April went under contract before or after January 1st. We therefore exclude them. The table looks at outcomes over the four years from December 2013 till December 2017. Column 1 reports estimates from two-stage least square regressions using variation in the timing of home purchase for first-time home buyers around the 2013 reform as instrument. The endogenous variable of interest is the amount of the mortgage balance that is repaid over that four-year period and the instrumental variable, *Post*, is a dummy variable equal to 1 if a household closed on their home after May 1st, 2013. The control group are all buyers who closed on their homes from October 2012 to February 2013, while the treated are those who closed from May to July 2013. The dependent variable is the change in wealth, or wealth accumulation, from the end of December 2013 till December 2017. As with all analysis that examine savings in 2014, we exclude buyers near the end of 2013, since they will have just purchased a home, making them less comparable to the those buying near the reform at the beginning of 2013. Column 2 is the same as column 1, but just shows the first stage of the two-stage least squares analysis conducted in column 1. Column 3 is the same as Column 2, but the dependent variable is the amount of the mortgage balance that is repaid over that four-year period divided by the amount of all liquid assets as of December of 2017. Some households have a low level of liquid assets in 2017 and, because the dependent variable in Column 3 is a ratio, this creates an outlier problem. We therefore exclude households with less than €100 in liquid assets and those with a ratio is greater than 50. T-statistics with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses. P-Values: * 10%; ** 5%; ***1%.

	IV (1)	OLS (2)	OLS (3)
Δ Wealth '13-'17	MTG Repaid '13-'17	MTG Repaid '13-'17	MTG Repaid '13-'17/ Liquid Assets '17
MTG Repaid '13-'17	0.887*** [0.73,1.05] (10.81)		
Post		8139.6*** (23.91)	1.205*** (11.10)
Control Group	10/12-2/13	10/12-2/13	10/12-2/13
Treated Group	5/13-7/13	5/13-7/13	5/13-7/13
IV	Post	-	-
F-Stat	508.8	-	-
Obs	25,169	25,169	25,169
Adj. R ²	0.176	0.053	0.012

Table A8. Resellers Sample

This table shows that effects for households selling and buying a new home (“resellers”) are similar for the sample as a whole – they do not appear to extract large quantities of home equity. The table examines the sub-sample who bought their first home between 2012 and 2013 and then resold it by December of 2016. The focus is on whether resellers who bought after the 2013 reform take the opportunity of a later move to extract any additional home equity accumulated due to the extra amortization. Column 1 regresses the December 2017 mortgage balance on *Post*, a dummy variable equal to 1 if they closed on their house after May 1st, 2013. The control group are all buyers who closed on their homes from October 2012 to February 2013, while the treated are those who closed from May to September 2013. The reform applied to the timing of going under contract, not closing, which typically takes at least two months. It is uncertain whether those who closed in March and April went under contract before or after January 1st. We therefore exclude them. Columns 2-5 have the same specification as Column 1, but use different dependent variables: the mortgage balance and the stock of liquid assets (deposits + stocks + bonds), either in levels or as the natural logarithm, all as of December 2017, or the amount of home equity extracted at the time of sale (change in home equity between the times of sales and purchase). Column 6 is the same as the two-stage least squares specification in Table 5, Column 2, but the instrument and the endogenous variable include an interaction with a dummy variable equal to 1 if the household resold their home by the end of 2016. T-statistics with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses. P-Values: * 10%; ** 5%; *** 1%.

	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) Δ Wealth '16
Post	-15,507.0** (-2.04)	-0.0629** (-2.00)	-1,303.0 (-0.25)	-1,056.9 (-0.40)	-0.0702 (-0.58)	
MTG Repaid '16						1.214*** [0.84,1.59] (6.34)
MTG Repaid '16 x Reseller Sample						-0.149 (-1.25)
Resellers '13-'16	Y	Y	Y	Y	Y	-
IV	-	-	-	-	-	Post
F-Stat	-	-	-	-	-	261.9
Obs	1,768	1,768	1,768	1,768	1,768	38,741
Adj. R ²	0.0029	0.0032	0.0001	-0.0003	-0.0003	0.290