Mortgage Amortization and Wealth Accumulation

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Abstract

Standard mortgage contracts include periodic debt repayment plans (amortization schedules) designed to build-up illiquid savings in the form of home equity, which can be substantial even from a macroeconomic standpoint. For example, U.S. households invest hundreds of ($) billions each year in mortgage amortization plans – comparable in size to pension program contributions. We provide the first empirical evidence on the causal effects of mortgage amortization on wealth accumulation. Ex-ante, effects are unclear. If increased debt repayments crowd-out households' non-mortgage savings, rather than alter their consumption/labor supply, there would be no effect on wealth. We use individual administrative data and plausibly exogenous variation in the timing of home purchases surrounding an interest-only mortgage reform in the Netherlands. We find little-to-no change in the accumulation of non-mortgage savings, even four years later, despite a significant increase in debt repayment. This lack of crowding-out implies a surprising near 1-for-1 rise in net worth and little savings-debt fungibility, financed via increased labor supply and reduced expenditures. Results hold using life-events (ex. birth of a child) as an instrument for the timing of home purchase, and appear unaffected by potential selection or confounded treatment concerns. Effects also hold focusing on buyers with substantial liquid savings and across the spectrum of ages, suggesting general applicability beyond just non-savers and the young. Our findings suggest that homeownership, when coupled with amortizing mortgages, is a key driver of household wealth building and inequality, and that the amortization-wealth elasticity is a crucial consideration for macroprudential policies.

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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)[1]

When households purchase a home with a standard mortgage contract, they not only sign-up for a loan, but also a periodic debt repayment plan, called an amortization schedule. These plans are designed to build-up substantial illiquid savings in the form of home equity prior to maturity of the loan. Amortization plans are ubiquitous across mortgages in most countries. They are not only substantial for each individual borrower, but also at a macroeconomic level. For example, in the U.S., households invest hundreds of billions of dollars each year in mortgage amortization plans, which make them comparable in size with other illiquid savings contributions, such as pension programs.[2]

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 mortgage 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 (ex. 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 elasticity between amortization and wealth accumulation (ϵ_{AW}) 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 building, retirement savings, and inequality. For example, if households compensate for increased debt repayments by reducing their liquid 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 household wealth accumulation.[3]

The empirical identification of ϵ_{AW} is challenging though. Households endogenously select into homeownership and their choice of mortgage contract. Renters are typically unlikely to be a valid

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[1] A special thanks to Patrick Moran for pointing us to this article (https://tinyurl.com/ycbott8f).

[2] In 2016, there were $10.3 trillion in U.S. residential mortgages (https://fred.stlouisfed.org/) 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).

[3] By contrast, policies reducing homeownership (either overall or for specific groups) could help explain historical differences in wealth accumulation among groups or periods (Charles and Hurst 2002; Krivo and Kaufman 2004).

counterfactual for households able and willing to buy a home[4]. The existing literature also shows that households who limit 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 typically borrowed 50% of the mortgage sum as interest-only. Afterwards, the vast majority borrowed the full 100% through a standard fully amortizing mortgage. The 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 first-time home buyers, our administrative data gives us precise measures of household wealth and its decomposition for every person living in the Netherlands from 2006 to 2016.[5]

We compare all first-time home buyers with a mortgage right around the implementation of the reform and find little-to-no difference in non-mortgage savings (the accumulation of bank deposits, stocks, or bonds) even four years after, despite a significant increase in observed debt repayment. This implies a surprising near one-for-one rise in net worth – a response consistent with little savings-debt repayment fungibility ($F\sim0$) and a substantial effect of amortization on wealth accumulation ($\epsilon_{AW}\sim1$). We find that around 26-36% of the increased wealth accumulation is financed with higher future household labor income, driven entirely by increases in household hours worked, despite no difference in income growth between the groups prior to home purchase. The remainder comes from a reduction in household expenditures, consistent with a substantial reduction in consumption[6]. 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 the regulation.

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[4] Older households often have substantial home equity, but little liquid savings (ex. Kaplan et al. 2014). This in itself could suggest a high ϵ_{AW}. However, this could also simply reflect the fact that these households have substantial housing wealth and therefore little need for other forms of savings.

[5] Also, since most first-time homebuyer mortgages were partially amortizing even prior to the regulation, effects are likely attributable to increases in the amortization amount, not a complete lack of familiarity with amortization.

[6] We are careful throughout our discussion that our measures of consumption are imputed from residuals, not actually observed consumption behavior. Given how much of the income statement/balance sheet of the household we can observe it is not unreasonable to assign virtually all of the changes in expenditures to changes in consumption, but as noted by Baker et al. (2018) there are circumstances where that distinction could be important to note.

We look at all first-time home buyers 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). This allows us to avoid any issues related to different macroeconomic conditions or any other period effects.[7] 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. Since 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, 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.

A key concern for our identification is heterogeneous sorting. That is, our estimates would be biased if first-time home buyers strategically timed their home purchase to avoid the reform, *and* if this behavior is systematically correlated with their subsequent savings decisions (those who buy earlier intend to save less).

First of all, we find no evidence that the reform affected the timing of a home purchase. There is no apparent bunching in the number of transactions in the months prior to the reform. Moreover, the reform did not change the probability of a household eventually buying its first home after experiencing a life-event (ex. birth of a child). The timing of a home purchase after a life-event also did not change. These findings may be at least partially explained by the fact that even prior to the reform, loan-to-income 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 sum a household could borrow based on its income.[8]

Second, we find no systematic differences in the underlying characteristics of households purchasing a home before or after the reform. House purchase values and origination LTVs vary smoothly across the reform. In addition, we do not find a sudden jump in other observable differences between those who bought before or after. In particular, pre-regulation income and income growth are smooth around the reform. There is also no evidence of differential trends, with wealth levels and accumulation (growth) during the years prior to the regulation the same for those buying before vs. after the regulation. To confirm there are truly no systematic differences, we compare first-time-homebuyers (FTHBs) with non-first-time-

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[7] Though it is worth noting we also don't find evidence of sudden macroeconomic changes in the period surrounding the regulation.

[8] By contrast, DeFusco et al. (2020) and Backman and van Santen (2020) show that in settings where regulatory constraints on leverage bind substantial bunching can occur. As we show later on and has been argued by Van Bekkum et al. (2019) Dutch households tend to borrow almost as much as they can up to the regulatory limit, which is unlikely to be a circumstance unique to just the Dutch setting.

homebuyers (non-FTHBs). Non-FTHB mortgages were partially grandfathered in under the old mortgage rules. This means that the reform's treatment was substantially smaller for this group – the jump in mortgage repayment around the time of the reform was much more limited (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. FTHBs also show no differential change in future non-mortgage savings, despite differences in treatment intensity relative to non-FTHBs, again consistent with no significant heterogeneous sorting. $\epsilon_{AW} \sim 1$ The lack of sorting around the regulation also suggests it is unlikely there were sudden changes in screening by lenders in response to the regulation.[9]

Though suggestive, this does not completely dispel the possibility of heterogeneous sorting around the reform. We therefore re-run our analysis focusing on first-time home buyers who also had a "life-event" in the months surrounding the regulation. The high-quality nature of 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. Moreover, the timing of life-events among our sub-group of first-time home buyers is unrelated with pre-regulation household income, changes in non-mortgage savings, or wealth accumulation (nor the appreciation of their house value after purchase), providing additional plausibility for the validity of this instrument. Using the month of a household life-event as an instrument we find that life-events after the reform are associated with increases in mortgage debt repayments, still matched one-for-one with changes in wealth accumulation. Given the sudden and persistent rise in wealth accumulation based on life-events around the reform, and the implausibility that households were timing life-events to avoid the reform, we conclude that there was indeed a causal effect of wealth accumulation consistent with an $\epsilon_{AW} \sim 1$ for first-time home buyers.

Another concern for identification is that our treatment is confounded by other major changes around the regulation which also alter wealth accumulation. First of all, there could be seasonal effects – early year buyers systematically differ in their wealth accumulation patterns. That does not appear to be an issue. Effects are persistent for those who went under contract after the regulation, even for those who bought at the end of 2013, but there are no effects for those same months of the year in 2012. Second, as of January 2013, the maximum allowed loan-to-value (LTV) ratio dropped by one percentage point. Despite

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[9] This lack of evidence for changes in screening is likely at least partially a function of the relatively low concern about default in the Netherlands and the wide use of floating rate mortgages. The Dutch have some of the harshest recourse laws in the world and a relatively strong social safety net, resulting in foreclosure rates, even at the peak of the financial crisis, that were multiple orders of magnitude lower than in the United States, despite generally higher loan-to-value ratios. Also, among first-time homebuyers in 2012, around 85% had rates that became floating within the first 10 years (Hypotheken Data Network), which substantially reduces any differences in initial rates based on duration differences caused by different amortization schedules.

this, there is no change in average LTV ratios. For completeness, we re-run our analysis among households with origination LTV ratios far below the regulatory thresholds and again find that $\epsilon_{AW} \sim 1$.

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 reduced mortgage interest deductibility (MID) for those buying after, meaning a reduction in future liquidity and life-time wealth.[10] 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 ascertain the importance of liquidity or wealth effects caused by the reduction in MID. We first show that liquidity effects are unlikely to explain our results. Our estimates of ϵ_{AW} 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 were no more than tens of euros. Yet, we still find an effect not different from an elasticity of 1.

Next, we evaluate whether our results could be driven by wealth effects. 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 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 an elasticity of about 1, suggesting it is unlikely our effects are 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 explore 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 older mortgages for whom the period

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[10] 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 regulation, it was hotly debated whether the MID reduction should not also apply to existing home owners. Moreover, the mortgage regulation was the first significant MID reduction in decades, and signaled more reductions to come, which were eventually implemented starting in 2014. In sum, the expected relative wealth and liquidity effects may have been small, but it is still a concern worth addressing.

they can benefit from a more extensive MID is shorter. In particular, we compare non-FTHBs with a remaining maturity on their mortgage of more or less than ten years. Comparing these groups, we find no difference in wealth accumulation. This is consistent with wealth effects playing little observable role in confounding our estimates of the effect of mortgage amortization on wealth accumulation.

While there is compelling evidence in our setting of a substantial causal effect of mortgage amortization on wealth accumulation, it is unclear what potential mechanisms could plausibly cause such a large response. We first show that our results are the same even among households with more than enough liquid assets, either in the flow they save in a given year or in their overall level, so that they could easily offset their mortgage repayment by altering their liquid savings. This holds either using their actual liquid savings in a year, or instrumenting for that using their liquid savings in the years before they bought the house. This suggests that our results are not just driven by non-savers and that results are likely to apply to the broader population.

Since our results are not just driven by non-savers, one possible explanation is that there is a substantial (perceived) liquidity difference between mortgage debt repayment and liquid 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 liquid 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 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.

This suggests behavioral channels may play a role. In the discussion we discuss which of these channels might be most relevant. One potential explanation is that mortgage amortization constitutes a default setting to accumulate wealth. Default settings have been shown to increase pension contributions (Madrian and Shea 2001; Chetty et al. 2014). Also, Beshears et al. (2019) provide evidence that these increased pension contributions are not necessarily offset by increases in liabilities – consistent with the little-to-no debt-savings fungibility or crowding-out we find in our setting[11]. Another possibility, as has been proposed theoretically (Kovacs and Moran 2019; Vihriala 2019), is that mortgages could act as a commitment device for present-biased households. There is experimental support for the idea that people value commitment devices (Beshears et al. 2015) and evidence in Finland that households may value

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[11] Our findings also relate to an established and closely related debate about the crowding out effects of public pensions on private savings (ex. Modigliani and Brumberg 1954; Friedman 1957; Feldstein 1974; Dicks-Mireaux and King 1984; Gale 1998; Attanasio and Rohwedder 2003; Attanasio and Brugiavini 2003; Engelhardt and Kumar 2011; Arnberg and Barslund 2013).

exactly that feature of amortizing mortgages (Vihriala 2019). It may also be that the splitting a large long-run savings plan into small monthly installments helps to make it easier for households to accumulate wealth, just like has been shown in some experimental settings (Hershfield et al. 2019) or simplifies the savings decision (Beshears et al. 2013).

Another plausible behavioral mechanism could be that for mental accounting reasons, households treat mortgage debt repayments as bills, not as wealth accumulation. Interest rate resets which change monthly mortgage payments alter consumption (Di Maggio et al. 2017) and labor supply (Zator 2019) both qualitatively and quantitatively similar to what we find for changes in mortgage repayment. If households are treating mortgage payments the same, regardless of whether they come from amortization or interest payments, then this could explain the relatively large response we observe. This would be consistent with what Camanho and Fernandes (2018) document in a lab setting and call the "mortgage illusion". It would also suggest that many of the responses shown to macroprudential policies aimed at changes in interest rates would be likely to carry over to changes in debt repayment caused by changes in amortization speeds (Piskorski and Seru 2018).

Households treating interest rates and mortgage repayments similarly would have very different economic implications though. There is a fundamental difference between our estimate of ϵ_{AW} and the estimates in the existing literature looking at changes in household behavior caused by changes in interest rates (Di Maggio et al. 2017; Zator 2019). Both are examining changes in monthly mortgage payments. However, absent any other changes, a change in interest rates directly changes net-worth. Interest rates are fees paid to the bank and lower interest rates make households richer. By contrast higher mortgage amortization requirements increase debt repayments. This means a reduction in household liabilities, not an increase in fees paid to the lender. Debt repayment itself has no direct effect on net-wealth. The same argument holds comparing our work to papers looking at changes in anticipatable changes in mortgage interest rates, tax rebates and other governmental payouts (ex. Johnson et al. 2006; Agarwal et al. 2007; Parker et al. 2013; Kaplan and Violante 2014; Keung 2018, Cookson et al. 2019). In all these cases, changes in liquidity translate one-for-one into changes into net-wealth. Nevertheless, our analysis shows a remarkable similarity in people's response to changes in mortgage payments coming either from debt repayment or interest payments. The fact that households appear to treat these fundamentally different forms of payments as equivalent has important microeconomic, macroeconomic, and policy implications, which we discuss in more detail in Section 5.

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. Ex-ante, one might expect that households with interest-only (IO) mortgages or alternative mortgage products

(AMP), which have smaller amortization amounts, would have more liquid non-mortgage savings. These households would therefore be less likely to default after a shock, leading amortization requirements to actually reduce financial stability (Svensson 2019; Svensson 2020). Our results suggest that this is not the case, as households do not seem to treat amortization and non-mortgage savings as substitutes.[12] It also suggests that AMPs not only cause perverse selection effects (ex. Hertzberg et al. 2018), they also appear to have substantial detrimental effects on the health of household balance sheets. It therefore seems likely that the rise in the use of these products in the U.S. before the Global Financial Crisis[13] likely had a detrimental effect on these borrowers' ability to eventually repay their debts.

This discussion suggests that stimulating mortgage amortization would help improved financial stability. In fact, this was the main motivation for the 2013 mortgage reform in the Netherlands we study in this paper. Our results show that the reform was effective: amortization increased household home equity, without leading to a reduction in liquid assets. This also suggest that macroprudential policies that encourage contracts with countercyclical amortization (Campbell et al. 2019) 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.

Our findings also have potential implications for the optimal design of retirement programs. Beshears et al. (2019) argue that the socially optimal retirement plan with taste shocks and present-bias should have 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.

Finally, our findings help reconcile different findings in the literature on the causal effect of homeownership on household wealth. Kaplan et al. (2014) show with survey data from the U.S., Canada, Australia, the U.K., Germany, France, Italy, and Spain that households often obtain substantial illiquid wealth in the form of housing and hold little or no liquid wealth. Sodini et al. (2017), however, explore plausibly exogenous variation in homeownership with interest-only mortgages and find little evidence that changes in owning a property raise future wealth accumulation. Our results help reconcile these seemingly inconsistent findings if amortization of the mortgage, typically coupled with the home purchase, is one of the critical drivers of the relationship between homeownership and wealth accumulation.

In a similar vein, our findings may help explain the black white-wealth gap. The existing literature attributes this gap, at least in part, to historical differences in homeownership driven by differential access

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[12] This is consistent with the evidence from Amromin et al. (2018).
[13] See Mian and Sufi (2009) and Adelino et al. (2016) for more details.

to financing (ex. Charles and Hurst 2002; Appel and Nickerson 2016; Aaronson et al. 2017; Anders 2018; Krimmel 2018). Our results suggest that it is not just the ability to purchase a home, but also the differential access to mortgages, usually linked to a fixed amortizing schedule, that can explain wealth differences.[14]

Given the potential importance of our results for policymakers, it is important to assess to whom our results are likely to apply to. To help examine the general validity of our findings we show that our elasticity holds across the spectrum of ages of first-time home buyers, including those more than 50 years old, and, though treatment is smaller, for non-first-time home buyers. This suggests that our effects are not confined to young households far away from retirement and are more broadly applicable.

The fact that we observe that the elasticity is the same for older households, also suggests that effects could be long-lasting. In the pension literature, there is debate whether households undo an increase in current pension contributions (driven by some intervention) by reducing future contributions (Choukhmane 2019, Wang et al. 2020). Unlike pensions contributions, offsetting mortgage amortization typically requires substantial costs. A future pension contribution rate can be reduced with only minimal effort and virtually no cost. By contrast, a household can't just choose to pay less of the amortization amount in the future without defaulting on the mortgage or paying substantial costs to obtain a new mortgage. This may explain why in the first four years we examine we find no evidence of any offsetting behavior.

Now it is possible that eventually households amortizing less will "catch-up" by building up substantially more savings later in the life of the mortgage, but even that seems questionable given more aggregate statistics. In the U.S. according to the 2016 Survey of Consumer Finances among homeowners without very high levels of liquid assets, the percent of households with a mortgage falls sharply with age. Around 90% have a mortgage in their 30s, but only 59% do by their mid-to-early 60s. By contrast, this is much flatter in age in the Netherlands where about 90% still have a mortgage by their mid-to-early 60s. Now some households won't become first-time homebuyers till later in life and clearly not all mortgages in the Netherlands/U.S. are interest-only/fully amortizing. Not to mention all the other differences between these countries which could confound cross-country comparisons. That being said, it is still striking how different mortgage indebtedness is for older homeowners in these two settings and suggestive that: 1) in the Netherlands where non-fully amortizing mortgages were dominant, it appears that households have predominantly refinanced their mortgages in order to avoid the large balloon payments that would be otherwise required at the maturity of these mortgage contracts, rather than building up financial assets on their own or steadily prepaying their mortgages over time; 2) in the U.S., households don't appear to have "undone" all the amortization via home equity withdrawals or re-levering at the time of moving. If these

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[14] This may be especially true if strong housing wealth bequest motives lead to substantial intergenerational transfers and persistence (McGee 2019).

tendencies are likely to hold for those buying around the regulation in the Netherlands it would certainly be suggestive of longer run effects on wealth accumulation.

Another possible concern for external validity is that, because of our intent-to-treat design, we could be measuring a local average treatment effect on compliers, and not an average treatment effect. This concern seems unlikely to be important in our setting. In many countries such as the U.S., UK, and Denmark households choosing non-amortizing mortgages are relatively atypical (Cocco 2013; Cox et al. 2015; Kuchler 2015). An advantage of our setting is that prior to the regulation most households had partially amortizing mortgages, while afterwards the vast majority of households had fully amortizing mortgages. This leaves little room for quantitatively important differences based on the treated population. Given all this, it seems likely that our estimates are likely to be valid across a broad set of household types, at least within the Netherlands.

Our findings also appear consistent with related findings in other countries, suggesting that the finding of a substantial effect of mortgage amortization on wealth accumulation is likely to hold in other settings. Prior work looking at the U.S., Canada, Denmark, and Finland does not examine wealth accumulation directly, but does find that reduced mortgage repayment increase consumption and reduce 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 (2019) for households with an option ARM period ending. 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. This is suggestive that households in the U.S. 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.

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 5. Section 5 discusses the possible mechanisms explaining our results. Section 6 concludes.

1. Mortgage Environment in the Netherlands

1.1. Pre-Regulation

In the recent past, households in the Netherlands have had exceptionally high loan-to-value (LTV) ratios for their mortgages. This is a combination of harsh recourse laws[15], generous mortgage interest deductibility (MID) for tax purposes in combination with a high marginal tax rate, and relatively loose macroprudential policies. In the late 1990s LTV ratios were usually in excess of 100% at origination and often as high as 120%, and very little mortgage amortization. By doing so, households were able to maximize their MID for tax purposes. Funds greater than the home value were used to finance moving costs including property transfer taxes, realtor fees, explicit costs of moving such as moving trucks, and renovations and refurbishing.

Starting in 2001 the Dutch government began to place limits on these origination practices, requiring mortgages to mature in less than 30 years and amortize at least 25% by maturity in order to be eligible for the MID. In an effort to avoid losing MID tax benefits, banks began to originate not only standard amortization mortgages, but also what are called linked-accounts. These were typically either linked savings or life insurance accounts where households could deposit funds as if they were paying the principal component of their monthly mortgage payments. The banks would not actually pay down the outstanding balance on these mortgages. Rather, these payments would be deposited in the bank and the interest on deposits would be used to pay for the interest on the mortgage, thus acting as if the mortgage balance had been paid down[16]. By doing so, the households could retain full their full MID, while still meeting regulatory requirements on the minimum amount of a mortgage that had to be amortizing.

In 2007 the Code of Conduct for Mortgage Loans (CCM) initially set limits on payment-to-income (PTI) and LTV ratios, but would eventually set additional requirements on amortization schedules as well in order for mortgages to have MID and conforming eligibility. Conforming loans were eligible for national mortgage insurance (NHG), providing additional protection and liquidity from originating banks, and resulting in a pass-through effect of substantially lower interest rates for borrowers. In particular the revised CCM implemented in August of 2011 set maximum interest-only (IO) components of new mortgages to

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[15] When Moody's ranked developed countries in terms of both the legal right for recourse and the practical application of recourse, the U.S. ranked as the weakest on both counts, while the Netherlands was ranked as "very strong" on the legal right of recourse and its application in practice, the highest ranking in either category (NVB 2014). The result is that foreclosure rates in the U.S. at their peak were almost a hundred times higher than in the Netherlands, even though a higher proportion of households had negative equity in the Netherlands in their relative Great Recession troughs.
[16] In practice these linked accounts could be more complex with households able to invest their funds in investment accounts, though the majority were still more standard deposit or life-insurance linked accounts. These life-insurance linked accounts could be used to pay down the mortgage, but would also be eligible for use in the case of the death of associated party.

50%, requiring at least half the house purchase repaid on a standard amortization schedule by the life of the mortgage – which must be less than 30 years. This did not prevent households from amortizing mortgages via linked accounts, so following this regulation the vast majority of mortgages originated had 50% IO and 50% linked accounts. In addition, the revised CCM set maximum origination LTVs at 106%, with 1% reductions each year afterwards until it finally reached 100% in January of 2018[17]. Just as they did with the limits for interest-only mortgages, households tended to borrow up near 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 1/5th of all mortgage offers were at exactly the limit.

1.2. The 2013 Regulation

For new home purchase contracts initiated 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 same year, these new regulations required all new mortgages to be fully amortizing over 30 years in order to retain mortgage-interest deductibility and conforming eligibility. During most of 2012 though there was substantial uncertainty about exactly whether the plan 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."[18] In the end, the measured was passed, applying completely to first-time homebuyer (FTHB) mortgages and partially grandfathering in for previous homeowners. These rules did not allow any of the mortgages for FTHBs to be IO or to amortize via a linked savings account. As can be seen in Figures A4 and A5, as of the beginning of 2012 less than 5% of mortgage offers were for standard fully amortizing mortgages, while by the beginning of 2013 almost 95% would be, causing a dramatic increase in the % of the mortgage balance expected to be repaid under the standard amortization contract. Interestingly, a comparison of FTHBs standard amortizing mortgage expected repayments (Figure A5) and actual observed repayments (Figure A10 Panel A black line) reveal a rise of 1.5% for both over 2014. This suggests households undo little-to-none of the treatment of the regulation via differential voluntary repayment or home equity withdrawals.

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[17] See Struyven (2015) and van Bekkum et al. (2019) for more discussion of this regulatory change and its effects. One of the reasons we do not study this period is that the initial implementation of these LTV limits may have had significant selection and perhaps treatment effects which could confounding our analysis.

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

In 2012 about 90% of the loans that were not fully standard amortizing mortgages were combination mortgages that were partially interest-only and partially amortizing linked mortgages accounts, with the vast majority approximately 50/50 each. This is consistent with borrowers typically having their interest-only component as high as possible while still under the limit imposed by the 2011 CCM requirements. As must be the case, given the massive and sudden rise in standard amortizing mortgages, the interest-only and linked account mortgages among FTHBs largely vanished by early 2013. With more than 40% of mortgage balances interest-only prior to the regulation this constituted a substantial shift in the amortization schedule for homes going under contract following the regulation.

Examining this regulation has a number of benefits. First, almost all FTHBs were compliers both before and after the regulation. This gives us confidence that our estimates are likely to apply to the broader population, rather than a particular subset (as would be the case for households who endogenously chose IO mortgages in the U.S.). Second, while changes in the amortization schedule would clearly increase monthly payments, it did *not* mechanically alter regulatory maximum PTI limits. Even prior to this regulation the National Budget Institute (NBI) would compute PTI limits as if the mortgage was a standard fully amortizing 30-year fixed rate loan, regardless of the actual mortgage type/terms. Third, mortgages were already partially amortizing, and had been for some time prior to the regulation. This means we contribute effects to increases in amortization, not the introduction of amortization at all for the first time. Fourth, we don't see evidence of other dramatic changes in mortgage macroeconomic conditions surrounding the regulation. As can be seen in Figures A2 and A3 house prices, average mortgage interest rates, and average origination loan-to-values varied smoothly around this regulation. A lack of a substantial change in interest rates, despite the shorter duration implied by the increased amortization, may reflect the lower concerns about default discussed previously, as well as the propensity for Dutch households to have relatively short fixed rate periods – 85% had rates that become floating within the first 10 years.

While, not as clean a group to examine as FTHBs, previous homeowners were also somewhat affected by this regulation and are another advantage of looking at this event. Previous homeowners not refinancing or moving were fully grandfathered in and weren't required to alter their existing mortgage contracts. By contrast, those buying a new home were obligated to meet the conditions of the August 2011 requirements, but conditional on meeting those requirements could otherwise carry-over their existing interest-only or linked-savings mortgages as is. That meant they were not, however, allowed to change any terms of these mortgage contracts. They couldn't increase the maturity, lock-in a lower interest rate, or increase the mortgage balance. That meant any homeowner buying a new home that is more expensive and hoping to borrow more to finance it would need to get an additional 30-year fully amortizing mortgage as a 2nd lien on top of the prior mortgage(s). In practice, this often meant that for previous homeowners hoping

to buy a new house the regulation had some effect on their amortization, despite the attempt to partially grandfather them in. These rules also meant that non-FTHBs with longer prior housing spells typically had shorter remaining life on their existing mortgages and subsequently a smaller long-run ability to be grandfathered in under the prior regime.

2. Data description

2.1. Datasets and Sample

Our primary analysis takes advantage of administrative datasets from The Statistics Netherlands (CBS), with individual-level financial information on every person living in the Netherlands from 2006-2016. The datasets are the transactions of the existing purchase dwellings registry (*Bestaande Koopwoningen*), the universes of individual address- (*Adresbus*) and family structure (*Huishoudensbus*) spells, the 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 (e.g. married without children) as well as the position of the individual in the household (e.g. 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 child, death of 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 surrounding the house purchase. Simple summary statistics on these households are provided in Table 1. As we noted in Section 2, the strict recourse laws and enforcement of those laws in the Netherlands is associated with high initial loan-to-value (LTV) ratios, usually in excess of 1, and this is true for our buyers as well, who have a median LTV of about 1.05. We also find that just like the population of homeowners overall, mortgage liabilities are by far the largest component of the average Dutch households' debt. For our group the median mortgage balance is €187k, while their overall liabilities, including the mortgage, are €193k. As would be expected for first-time home buyers these households tend to be fairly young, with a median age of 36 years old for the oldest member of the household, and fairly high income, which is why they are able to buy a house, with a median household gross income in 2014 of about €54k.

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2.2. Variability in Liquid Financial Assets

Perhaps not surprisingly given their relatively high income, these households also on average have a non-negligible amount of liquid savings and variability in those savings. We measure liquid savings as the combination of all their deposits (money in all their checking and savings accounts) and their non-housing financial assets, including stocks, bonds, etc. The presence of a wealth tax in the Netherlands means information on financial assets are collected comprehensively at the household level and verified by financial institutions. In Table 1 we show that the median household liquid assets as of 2014 is ~€8k and more than ¾ of them have liquid assets of greater than €2.6k. We also compute separately, not shown, using data from 2006-2016 the variation within a given household and find substantial variation across time. The within household year-over-year standard deviation in liquid savings over the whole time period is about €14k. In our summary statistics table, we also show that in just 2014 the standard deviation of the year-over-year change in liquid assets was about €9k. These variations appear to be driven by economic conditions facing the households in a manner that might be expected. In appendix Figure A1 we plot yearly changes in liquid financial assets in the years surrounding a year when a household experiences any decline in their gross household income, after including household fixed effects and time fixed effects. As would be expected, there is a substantial decline in the year of the income decline as households likely use their liquid financial assets as a savings buffer for consumption smoothing purposes. This provides some validation of the administrative data collected and verified by the Dutch government, and shows that households appear to have non-negligible stocks, flows, and variability in their liquid financial assets.

2.3. Measuring Wealth Accumulation

One of the advantages of exploring this regulation in the Netherlands is the presence of detailed administrative data on wealth and its components at a 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 we can obtain detailed and accurate information on from administrative records at CBS and that represent wealth accumulation decisions of the household. These includes all liquid assets, as discussed in Section 2.2, as well as implied voluntary pension contributions[19].

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[19] In the Netherlands, most pension contributions are mandatory and collected by employers (unfortunately, these payments are unobservable to us). 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

The 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 within our analysis[20]. Our measure is meant to capture wealth accumulation decisions by the household, not their total wealth, so it does not include the value of human capital (ex. income), mandated pension contributions, implicit guarantees, etc. For our purposes there is no reason to be believe any of these things, besides income (which we will examine in detail), are likely to change systematically around the regulation, and we explore income separately within our analysis. Liabilities include the outstanding mortgage balance and all other non-mortgage liabilities. Non-mortgage liabilities are provided by CBS and are based on national credit registry data merged to the household level. Outstanding mortgage liabilities are based on administrative tax records from CBS filed by households and verified by banks. As discussed in Section 2, mortgages with linked savings/insurance accounts allowed households to avoid taxes meaning CBS's administrative tax records do not include information on the amortization that occurs within those accounts. To overcome those issues, we supplement this data with information provided by the Mortgage Data Network or Hypotheken Data Netwerk (HDN) dataset of mortgage offers. This data covers around 75% of mortgage request 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,..). As we noted previously, prior to the 2013 regulation new mortgages had to be at least 50% amortizing to be eligible for interest deductibility and be conforming. We verify that most were approximately 50% amortizing, though often via these linked accounts. Therefore, if in CBS we observe a mortgage that appears to have no change year-over-year in its mortgage balance we make the assumption that the household has a linked savings account for 50% of their mortgage[21]. We then impute the amortization the household effectively made within the linked account, assuming these mortgages amortize as an annuity, plugging in an interest rate of 4.50%. In other words, any funds in linked accounts are treated as repayments of the mortgage (i.e. effectively a lower mortgage balance), not as part of financial assets, since these sinking funds are only accessible for mortgage repayment. Using an aggregate matching method to combine these datasets across groups of buyers we can verify that for our first-time homebuyers around the regulation this approach accurately measures their linked accounts. In our analysis we also show that our elasticities of interest are unchanged using just the aggregate matched datasets or alternative assumptions.

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contributions. We verify that that these contributions are positively correlated with household income and are generally distributed in way consistent with maximum contribution cut-offs providing validity for our calculations.

[20] 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 including real estate changes as well in our measure of wealth.

[21] In our robustness checks we show that our estimated elasticities are virtually unchanged changing these assumptions.

2.4. Life Events

Another benefit of examining this regulation in the Netherlands is that they collect and have made available for research accurate and timely information available on household life circumstances. For our purposes, we are able to get detailed information on the number of members of a household over time. This allows us to limit our sample to 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 members of the household 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 by driven by the timing of these life-events and so unlikely to be timed strategically to take advantage of any regulations. We verify that it is indeed the case that the timing of life-events appears to be a valid instrument for the timing of home purchase. We proceed by using the timing of their life-events, not their home purchase, as instrument for the mortgage amortization driven by the regulation.

3. Theoretical Predictions and Empirical Design

For the purposes of this paper we can simplify the discussion by considering the following decomposition:

$$\textbf{W}\text{ealth Accumulation}_{i,t,t+1} \equiv \text{Mortgage } \textbf{A}\text{mortization}_{i,t,t+1} + \text{Net Other } \textbf{S}\text{avings}_{i,t,t+1} \qquad (1)$$

where wealth accumulation, or $\Delta Net\ Worth$, for household *i* from date *t* to *t+1* is equal to the mortgage amortization, including all mortgage debt repayment, plus any net non-mortgage savings over that same period. Net non-mortgage savings includes all other components of household wealth accumulation except mortgage repayment, including the build-up of deposits, stocks, bonds, etc. or reductions in non-housing liabilities, such as consumer loans. What should be clear is a change in amortization requirements, which increase the speed of mortgage debt repayment, only increases wealth accumulation if they are not offset by changes in other savings. For example, if a household was forced to amortize their mortgage an additional €1k in a given year, but did so by selling stocks worth €1k, savings less into their savings account by €1k, or by paying down their other liabilities by €1k less, there would be no effect on wealth accumulation. In other words, the question is to what extent households act in a way consistent with the fungibility of these separate accounts.

Our paper therefore boils down to a provide one estimate which defines the value of the following two elasticities:

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

and

$$Wealth\,Elasticity\,(\epsilon_{WA}) := \frac{\partial W}{\partial A} := 1\text{-}F \tag{3}$$

where F is the fungibility between mortgage repayment induced by amortization and net non-mortgage savings, while ϵ_{WA} is the change in wealth for a change in mortgage amortization. We have defined F so that if mortgage repayments and non-mortgage savings are treated as perfect substitutes, aka perfectly fungible, then this value will be 1 and $\epsilon_{WA} = 0$. In that case any changes in mortgage repayments are offset by changes in non-mortgage wealth, leading to no change in wealth accumulation. On the other hand, if $F=0$ then households don't alter behavior in their other accounts which means increased debt repayments speed up wealth accumulation.

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

$$MR_{Jan-Dec\,2015,i} = \sum \delta_C \times 1_{C,i} + \eta_i \tag{4}$$

$$WA_{Jan-Dec\,2015,i} = \sum \beta_C \times 1_{C,i} + u_i \tag{5}$$

where *MR* and *WA* are mortgage repayment (amortization) and wealth accumulation, respectively. in each regression the only independent variable is the cohort (month) when a household closed on their house, $1_{C,i}$. We consider households who closed after March 2013 as "treated" (intent-to-treat), since the regulation binds for those who went under contract after January 2013, which typically takes at least 2 months. From these estimates, we calculate $\hat{\epsilon}_{WA} = \frac{\bar{\beta}_{treated} - \bar{\beta}_{control}}{\bar{\delta}_{treated} - \bar{\delta}_{control}}$ using just these simple averages.

We estimate this elasticity more formally within a two-stage least squares estimation, using the timing of home purchase as an instrument for mortgage repayments. In particular we estimate the following 1st stage:

$$MR_{Jan-Dec\,2015,i} = \delta_{treated}\,1_{treated,i} + \lambda_r + X_i'\beta + \eta_i \tag{6}$$

Where *MR* is the increased 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 2013, λ_r are geographic fixed effects, and X_i are household controls in the years prior to home purchase (ex. 2010 household gross income). If the regulation increased mortgage repayment, we would expect $\delta_{treated}$ to be positive and highly statistically significant. The second stage takes the predicted mortgage repayments from equation (6) and looks at the effect on wealth accumulation (we run this using 2SLS to obtain the correct standard errors):

$$WA_{Jan-Dec\ 2015,i} = \gamma_{treated}\ \widehat{MR}_{Jan-Dec\ 2015,i} + \lambda_r + X_i'\beta + u_i \tag{7}$$

where we are using the timing of house purchase as an instrument to see the effect of mortgage repayments driven by mortgage amortization on household i wealth accumulation over 2015.

One concern is that the timing of home purchase may be correlated with household preferred wealth accumulation, in which case that selection could bias our estimates. To alleviate that, we run the same two stage least squares specification from equations (6) and (7), but we restrict ourselves to only 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 purchase, to determine whether a household is treated or not.

Wealth accumulation cannot arise out of nowhere. By definition:

$$WA_{2015,i} \equiv I_{2015,i} - E_{2015,i} \tag{8}$$

where I is after-tax income and E is expenditures of household i. Therefore, if household wealth accumulation rises, because they do not alter non-mortgage savings, then it must be the case that either they increase their income or reduce their expenditures. We will able to use our detailed administrative data to analyze changes in income, interpreting the remaining variation as changes coming from expenditures, likely in large part a reduction in what we would typically label consumption.

4. Results

4.1. Mortgage amortization and wealth accumulation

To examine the effect of this amortization regulation on mortgage repayment we examine mortgage repayments and wealth accumulation among first-time home buyers who closed on their house at different times around the regulation. Following the methodology outlined in Equation (4) we start by comparing the amount of mortgage repayment from January to December 2015 cohort-by-cohort in Figure 1. The earliest most households who went under contract after the regulation would close by is March 2013. The solid black line is the estimated amount of mortgage repayment for each cohort relative to the omitted cohort of February 2013 (the last pre-treatment month). We do not include any other controls and use the full sample of first-time home buyers. Relative to the February 2013 buyers, those who bought in each month in 2012

and in January of 2013 appear to have similar amounts of mortgage repayment in 2015. By contrast for buyers who close on their houses March 2013 and later there is a sudden and persistent rise in the amount of mortgage repayment, quickly flattening out at about a €2k increase, which, as can be seen on the second y-axis, is around 3% of household gross income (or about 5% of their net income). It therefore appears, as would be expected, that the regulation did in fact lead to an increase in mortgage debt repayments caused by the increased amortization requirements.

How did households respond with their non-mortgage savings and therefore the effects on wealth accumulation? The yellow dotted line in Figure 1 is the change in non-housing wealth - including deposits, financial assets, and non-mortgage liabilities – over 2015 for these same buyers, again relative to the omitted February 2013 cohort. Again, we find little systematic differences in non-housing savings by cohort in the months leading up to the regulation, but in contrast to the change in mortgage repayment, we find little evidence of a change in non-housing savings for the households who bought after the regulation either. Households do not appear to act as if these accounts are fungible ($F \sim 0$), since they do not pay for the increased regular debt repayments by reducing wealth accumulation in their other accounts. The net results, as can be seen with the dashed gray line in Figure 1, is a near 1-for-1 increase in wealth accumulation with increased mortgage repayment driven by the amortization changes $\epsilon_{AW} \sim 1$. By contrast, in appendix Figure A6 we illustrate what the response of a hypothetical set of households treating their accounts as perfectly fungible would look like. In this case non-housing savings would fall (yellow dotted line), perfectly offsetting the rise in mortgage repayment (black solid line), and there would be no change in wealth accumulation (gray line).

In Table 2 we formalize this analysis following the two-stage least squares procedure outlined in Equations (6) and (7). This table includes the subset of ~42k first-time home buyers who bought near the regulation after October 2012 and before September 2013, but not during the months when households experienced only partial treatment (March and April 2013). It should be apparent from Figure 1 that elasticities are unchanged for other subsets. In Column 1 we show that being part of a cohort that almost surely bought after the regulation is associated with a ~€2k higher mortgage debt repayment in 2015. This is our first stage estimate. In Column 2 we find a nearly identical effect on wealth accumulation, so it is not surprising that in Column 3 we find an estimate of the mortgage amortization-wealth accumulation elasticity, ϵ_{AW}, of 0.993, which is statistically different from 0, but not from 1 (95% confidence intervals of 0.88 to 1.10). In other words, consistent with Figure 1, it appears that about each dollar of increased mortgage repayment increases wealth by approximately one dollar. Consistent with this, in Column 4 we find no statistically significant effect on non-housing savings for these same households over 2015. In appendix Figures A7 and A8, we separate our effects for non-housing savings into the components coming

from changes in financial assets (A6) and non-mortgage liabilities (A7). In both cases, just like we saw overall, there is no offsetting of the mortgage repayment increases. Households do not appear to reduce the funds in their checking/savings/investment accounts or their annual flow into those accounts in order to pay for their increased mortgage repayments[22]. Given that, how then do households finance this increased wealth accumulation?

In Table 3 we examine exactly how households adjust to the increased mortgage repayment. For this we use the framework of the income/consumption/savings identity highlighted in Equation (8). In Column 1 we show that within a given household, gross income rises by about ~€1,270k from 2012 to 2015 for those who bought after, relative to before the regulation. 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 information on hours worked to show that these changes in household gross labor income come from changes in hours worked by the household[23]. In appendix Tables A5 we show an increase in the number of wage earners in the household, both overall (column 1) and for those households with at least two working age members (column 2). This results in a reduction in the probability a house has only a single earner from around 27% of households to only around 25% (column 3) and also holds conditioning on households that experience a change on single earner status (column 4). Perhaps not surprisingly then in Table A6 column 3 and 4 we find increases in total household hours worked from 2012 to 2015. In fact, in column 5 we show that controlling for changes in hours worked the magnitude of the effect of buying after the regulation on income growth falls dramatically in magnitude and is no longer statistically significant. This suggests that virtually all of the future rise in household gross income for those buying after the regulation, relative to before, comes via increased hours worked, consistent with an increase in a labor supply.

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[22] As we show in appendix Table A1, these results are robust to including an alternative measure of voluntary pension contributions that includes all imputed values (column1), including the appraised value of real estate in our measure of wealth (column 2), both (column 3), running the analysis over the sum of all mortgage repayment and wealth accumulation for 2014 and 2015 (column 4), 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. These findings are also unchanged including 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 aren't buying a house for the first time (column 3). These findings are also qualitatively and quantitively unchanged when 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). This includes just using the raw observed data itself (Table A3 column 1) without any amortization adjustments.
[23] 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 (ex. UI insurance, et al.) and are linked into the primary data sources via unique person-level identifiers by CBS. We validate in columns 1 and 2 of appendix Table AVI that in levels and changes hours worked are highly correlated with household income.

When then we formally run the two-stage least squares estimate to look at effects on household gross income without any controls in Column 2 of Table 3 and we get an estimate of 0.621. Since marginal tax rates are about 42% in the Netherlands for our group of buyers, this would suggest that approximately 0.621 x (1-42%) ~ 36% of the increase in wealth accumulation can be paid for by an increase in after-tax household income, with 95% confidence intervals of 22-51%. We obtain similar estimates in Column 3 after controlling for household financial circumstances well before the regulation, in particular the log of gross household income and financial assets in 2011, and municipal fixed effects. In Columns 4 and 5, we show that the increase in income is caused by an increase in income in 2015, not a decrease right before the regulation in 2012. Consistent with these results, in Column 6 we show that our initial findings for the elasticity on wealth are unchanged including those same set of pre-regulation and geographic controls. The estimate in Column 5 would suggest that around 26% of the increase in debt repayment could be paid for by a rise in after-tax household income. Taken together, our point estimates suggest that household compensated around 1/4 -1/3 of the rise in mortgage amortization and wealth accumulation by increasing after-tax household income. By omission, the remainder must be driven by reductions in household expenditures.

4.2. Addressing selection concerns

While our findings so far are consistent with a large response of wealth accumulation to mortgage amortization, since the timing of home purchases is not randomly assigned it is possible that our estimates are confounded by selection concerns. If households who mostly want to save less are able to systematically buy before the regulation, leaving only those that do not mind saving more to buy after, then this would bias our estimates upwards. In appendix Figure A9 we examine the number of home purchase closings per month for our group of buyers and do not find any evidence consistent with bunching around the regulation[24]. That being said, it is theoretically possible (though not ex-ante obvious) that sorting could occur across time, without any variation in the total level of transactions in a way that causes systematic bias in our estimates. Evidence presented in appendix Figure A10, though suggests such concerns are unlikely to be a major factor in this setting. We conduct the same analysis, based on the purchase cohort month, carried out previously, but break-out our analysis into FTHBs and all other homebuyers around the 2013 regulation. FTHBs are treated more than all other homebuyers (Panel A), so if there is selection

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[24] The spike in transactions in June of 2012 is driven by concerns about an increase in the transaction tax for new house purchases. This stands in stark contrast to the lack of any spikes or dips around the 2013 regulation, suggesting households do sometimes respond to changes in mortgage regulation, but clearly did not appear to do so for this regulation.

around the regulation cut-off, that keeps the total number of buyers smooth, but systematically sorted, we would expect to see a sudden non-linear change in FTHBs confounding co-variates right around the regulatory change, matching Panel A. We would also expect to see a similar non-linear movement but likely smaller change 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) or pre-regulation financial assets (Panel E), gross household income (Panel F), financial asset accumulation (Panel G), or income growth (Panel H) we see no evidence of sharp non-linear changes in the co-variates of FTHBs, all other buyers, and the differences between them in the cohorts surrounding the regulation[25]. For both groups we also see no evidence of crowding out of financial asset accumulation (Panels C and D) again consistent with a large effect of mortgage amortization on wealth accumulation with is pervasive across multiple buyer types. Now while it may not be likely it is still possible these buyers differ suddenly on dimensions we don't examine. To alleviate even these concerns, we use a novel feature of our setting and data – life-event changes.

In particular, we focus on the subset of our original sample of first-time home buyers who experience a life-event (changes in the number of members of their household) during the same period when they purchase their homes (2012-2013). The high-quality nature of the administrative data collection in the Netherlands lets us identify the exact month where there are changes in the number of members of the household, such as the birth of a child. In Figure 2 we re-estimate the exact same analysis from Figure 1, focusing on this subset of buyers and plotting the effects by the quarter of their life-event, not the actual purchase quarter of their home. In Figure 2 we show that relative to the omitted quarters - Q4 of 2013 and Q1 of 2013 – mortgage amortization is similar for quarters in 2012 (gray points). By contrast, in 2013 we find substantially higher mortgage repayments in 2015 for those with life-events in 2013. These sort of life-events, perhaps not surprisingly, are significant drivers of first-time home purchase decisions, so those with later life-events are more likely to be affected by the regulation, leading to more debt repayment, even 3 years later. Just like we saw in Figure 1, the increases in mortgage amortization are matched nearly one-for-one with increases in in wealth accumulation for the same households over 2015.

We rerun this analysis formally in Table 4 using the two-stage least squares methodology implemented previously, now on the subset of buyers with life-events and using the month of their life-event, not closing of home purchase, as the source of variation in the first stage. In Columns 1-3 we first

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[25] The key here is that there isn't evidence of non-linear changes right around the regulation in all three: FTHBs, non-FTHBs, and the differences between them, that is consistent with selection altering wealth accumulation. For example, in Panel F there is a slow downward trend in FTHBs that is steeper than for non-FTHBs, but there is little evidence of a sharp non-linear change around the regulation and the lack of any difference at all for those non-FTHBs buying before vs. after (and no differential future financial asset accumulation for either group) supports our identifying assumptions.

show no difference in pre-regulation household income, financial asset accumulation, or overall wealth accumulation in 2010 for households experiencing a life event before or after the regulation. Despite this, in Columns 4 and 5 we do find significant increases in the amount of mortgage repayment and wealth accumulation. In Column 6 we show 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. In Column 7 we look at all households with a life-event between 2012 and 2013 and don't have a home at the end of 2011, but don't require them to become a first-time homebuyer during this period. We then regress a dummy variable equal to one if they own any real estate by the end of 2016, which occurs 16.9% of the time, on a dummy variable equal to one if their life-event occurs after the regulation. We find no effect of having a life-event after the regulation on the probability of owning any real estate at all 4 years later, suggesting little evidence of the regulation altering homeownership rates. This also shown even more clearly in Figure A11 where the same estimation is done month-by-month and we so no evidence of any movement surrounding the regulation. As before, in the IV specification in Column 8, we find an estimate of the mortgage amortization-wealth accumulation elasticity, ϵ_{AW} , of 0.864, which is statistically different from 0, but not from 1 (95% confidence intervals of 0.54 to 1.19)[26]. One potential concern is that municipal records for household members might be more likely updated when there is a housing transition. 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 ϵ_{AW} close to 1 (0.931, with 95% confidence intervals of 0.41 to 1.45).

To sum up: we find little evidence for bunching of transactions before the regulation, 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 regulation, as a different source of variation. We obtain very similar estimates with ϵ_{AW} close to 1. This suggests that the overall findings are not contaminated by selection effects.

4.3. Addressing confounded treatment concerns

Given the analyses in the previous sections it seems plausible to interpret our findings as a causal effect on those who bought their house after versus before the regulation. Nevertheless, it is still possible that concurrent treatments confound the results. For example, households who bought later have

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[26] As we show in Table A1 column 6 we also 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.

by definition a different amount of time passed since they moved into their new home. Since we compare household behavior over the same period (2015), this might matter. We make this explicit in Figures 1 and 2 which include the "age" (months since their respective events, house transfer or life-event) on the horizontal axis, which ranges between 12 and 32 months since we compare behavior over 2015. The figures clearly indicate that that age is not a likely confound in our setting. There appears to be no effect of age on wealth accumulation for those with house transfers prior to the regulation, with wealth accumulation for those ages basically flat for each month from 29 till 23 months, relative to the omitted age of 22 months (which is the February 2013 cohort). There is then a sudden rise, concurrent with the timing of the regulation, from ages 21 to 20 months, at which point the curve flattens out again at a higher level, and remains flat from ages 19 to 12 months. We observe a similar pattern using variation based on life-events in Figure 2. Along similar lines, the results in Figure 1 do not suggest that our effect is driven by seasonal factors. For age to be a confound, it would need to be the case that the concurrent treatment takes place in exactly the same month as the regulation. Given our discussion of the general economic environment in Section 1, this seems implausible.

One potentially confound might come from differences between groups at year-end that arise from the date of the house transfer (rather than purchase) occurring before or after year-end. One such candidate is the wealth tax that we discuss in Section 1. There were no changes in the wealth tax from 2012 to 2013. However, those households who had a house transfer after January 1st 2013 might have had more non-housing savings on that date than those who had a house transfer earlier, and therefore had to pay a higher wealth tax (at 1.2%). It is unlikely that this had effects lasting until 2015. Nevertheless, in our setting there is a straightforward way but to address this issue (and other issues arising from similar year-end effects). In Table 5 Column 1, we re-estimate our primary specification focusing on households who had a house transfer in 2013, either in January and February or March and April. The regulation is based on the time of going under contract, not the date of the house transfer, with typically takes place at least 2 months later. Therefore, the former group is unlikely to be affected by the regulation, while the latter group is. At the same time, the two groups are similar in age since purchase. Results are virtually the same as before, suggesting that age is not a likely confound. Furthermore, this particular exercise also suggests that any policies which tend to occur at year-end and that were based on the house transfer date (rather than contract date) are unlikely to drive results.

Another potential confound in our setting are other effects from the regulation itself. In particular, households who purchased their homes under the new regulation lose part of the mortgage interest deductibility (MID). The regulation stipulates that only standard 30-year amortizing mortgages qualify for interest deductibility. Mortgages with linked savings account and interest-only mortgages are ineligible.

Moreover, these non-conforming mortgages lose access to the national mortgage insurance. That means that households purchasing after the regulation can expect larger tax payments, all else equal, since faster repayment reduces the euro MID amount, especially later in the life of the mortgage. This would affect 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 liquidity effect will be small in the first few years of the mortgage, amounting to substantially less than the €2000 baseline effect we find. The life-time wealth effect is potentially larger. However, because most of the differences in tax deductibility accrue later in the mortgage this depends on homeowners' expectations of what would happen with MID in the future. Prospects were highly uncertain. The *Raad van State*, the Dutch Council of State, was highly skeptical of the regulation marinating full MID for existing home owners and restricting it for new time home buyers, given that one of the main reasons to have MID in the first place is to stimulate home ownership. According to the Council this would lead to an equal treatment under the law which was hard to validate.[27] This might have raised expectations that existing homebuyers would lose part of the MID as well. Moreover, the regulation was the first substantial change in the Dutch MID regime in decades, suggesting more restrictions were to follow.[28] As we discussed previously in appendix Figure A9, we find no evidence of bunching around the 2013 regulation, but we do around an expected increase in the transaction tax in June of 2012. This confirms that households do not appear to respond as if they expect the 2013 regulation to constitute a substantial wealth shock similar to the June 2012 transaction tax[29].

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 ϵ_{AW} are similar if we estimate it for 2014 or 2016 (rather than 2015). If liquidity effects from tax differences were important, we would expect to see substantial differences between those years. Also, in 2014, tax differences should not have been more than

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[27] 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.

[28] In fact, starting in 2014, the maximum marginal tax rate at which you 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.

[29] This lack of bunching or evidence of more general selection around the regulation may represent uncertainty about the future variability in the MID from buying before vs. after either because of regulatory change or limits on the grandfathering highlighted earlier (ex. no ability to extend maturity or increase balance). It may also be that the MID had relatively little effect on household decisions in this setting. 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.

tens of euros since an annuity mortgage hardly amortizes anything in the first few months after origination. Nevertheless, we still find an effect not different from an elasticity of 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 regulation would be expected to differentially increase their savings (reduce their borrowings), 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, letting us use the increase amortization amount over time as our endogenous variable of interest instead of the average increase in debt repayment. Again, in column 4 we find an elasticity of about 1. This suggests it is unlikely our effects are confounded by time-invariant shocks occurring at the time of initially going under contract on the mortgage driven from differential wealth shocks caused by the MID. Also, our finding of consistent effects even 4 years after the regulation (2016), suggests our results are also unlikely to be driven by a lack of familiarity with these new regulations or slow adjustment. This is especially true since households do appear to adjust along other dimensions, such as their labor income, and that mortgages were amortizing, just partially, even prior to the regulation[30].

4.5. Not just non-savers and the young

For the correct interpretation of our findings it is important to pin down what type of households drive our results. We investigate whether our effect is isolated to non-savers and the young, or whether it holds more generally.

Households without liquid savings might just be forced to wealth accumulate 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 would likely only hold for households with very few liquid assets. As we discuss in Section 2.2 most households in our sample have more than enough liquid

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[30] We also show wealth effects from expectations of reduced future MIDs are unlikely to be a significant confound in appendix Figure A12 by comparing non-FTHBs with different previous residence housing spell lengths. In particular, we compare buyers who lived in their prior residence for more versus less than 10 years, where on average those in the latter group lived in the residence for almost 16 years. Given how much longer the second group lived in their prior residence it seems likely they would have shorter maturities remaining on their mortgage and therefore less to gain or lose from any potential perceived future loss of the MID. Despite this, even though they face similar short-run changes in mortgage debt repayment (Panel A), they show no differences in house size purchased (Panel B), or non-mortgage wealth accumulation (Panels C and D). This again suggests expected far in the future wealth benefits of the MID are unlikely to be biasing our estimated elasticity in a meaningful way.

financial assets to pay for the increased mortgage amortization using just the money in their checking or savings accounts. In Table 6 we show that our results are consistent for households that do not appear to be financially constrained. In Columns 1 and 2 we show that our effects are similar for 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 appear less likely that they are liquidity constrained[31]. In Column 3 we find similar results limiting ourselves to the subset of households who either have at least €10k in liquid financial 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 financial assets, suggesting they are unlikely to be up against their financial constraints. That being said, it is possible that high liquid assets signal a high need for precautionary savings (or a high demand for some sort of indivisible consumption). In that case, this group would still be unwilling to reduce its liquid savings in response to increased mortgage debt repayments. To address that concern in Columns 4-6 we show that a household's liquid financial assets are highly persistent and use their lagged financial assets as the source of variation. In particular, requiring that all households have at a minimum €10k in liquid financial assets at the end of 2011 means that group on average has liquid financial assets ~€43k more than the average of the other households in the sample in 2011 (column 4) and ~€26k higher by the end of 2015. In Column 6, we find that our elasticity is unchanged even focusing on the subgroup with higher liquid financial assets in 2011, and therefore still higher financial assets in 2015. It still possible these households have a large need for liquid financial assets, which they are unwilling to reduce in response to increased mortgage debt repayments. Nevertheless, the results in Tale 6 do suggest our elasticity estimates are likely to apply to a broad set of households, not just those with no or very low liquid savings.

Next, we explore whether our estimates simply represent young households far away from retirement. In Table 7 we show that our results appear to be pervasive across age groups. In Columns 1-4 we re-run our primary analysis but focus on households where the maximum age in a household is above 30, 40, and 50 respectively (after excluding those older than 75). In all cases we find elasticities in line with our prior estimates. In the Netherlands there are few multi-generation households and 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 occupant older than 50, excluding any households where any members of the household differ in age by more than 20 years. Our results are consistent.

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

Finally, our effect might be driven by less experienced buyers. The previous finding that buyers act the same regardless of age seems to speak against this, as long as age and experience are correlated. In Column 5, we show no statistically significant difference for the subset of households who resell their home during our period, and who we might think are better aware of the different regulations. These findings, in addition to our finding of similar estimates even four years after home purchase, suggest a degree of persistence consistent with long-run effects on wealth accumulation that are not driven by inexperience or inattention around the introduction of the regulation.[32]

While we can't test explicitly for this persistence without waiting several decades and reanalyzing our cohorts, aggregate statistics are at least suggestive that this is likely. As can be seen in appendix Figure A13, homeowners without high levels of liquid assets in the U.S. and Netherlands both typically finance house purchases with mortgages, as can be seen for individuals ages 25-30 in Panel B. Also, if anything Dutch households tend to be homeowners a bit earlier in their lives allowing them more time to repay their loans (Panel A). Despite that, older Dutch homeowners are much more likely to still have mortgages than their U.S. counterparts. In their early-to-mid 60s only about 59% of U.S. homeowners still have mortgage debt, of which many will be those who bought for the first time slightly later in life, while about 90% of Dutch homeowners in their early-to-mid 60s still have a mortgage. This dramatic difference in the life-cycle of mortgage repayment is clear in Panel B and the fact that Dutch mortgages at this time were predominantly non-fully amortizing, while U.S. mortgages were, is certainly suggestive that amortization could have long-lasting effects. In particular, it appears that Dutch households who bought prior to the regulation typically refinanced their mortgage rather than repaying it, not consistent with them suddenly building up their own substantial financial assets and repaying the mortgage just after the period we examine[33]. Also, in a setting like the U.S. where home equity withdrawals are feasible, we still see most households pay-off their mortgage balance and mostly don't "undo" the repayment plan implicit in the amortization schedule. Both of these are consistent with longer run effects on wealth accumulation of mortgage amortization. It also likely explains why even though Dutch households have far less revolving credit, auto-loans, and student loans than U.S. households, as of 2013 the OECD shows that household debt to net disposable income is 116% in the U.S., but 270% in the Netherlands[34].

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[32] This could stand in contrast to other mechanisms that have been show to help encourage long-run savings. For example, default autoenrollments for pension contributions appear to substantially raise short-run contributions (ex. Madrian and Shea 2001), but may be at least partially offset by lower contributions later on (Choukhmane 2019).

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

[34] https://data.oecd.org/hha/household-debt.htm

Taken all together it appears that our results appear to be generally applicable, not just for liquidity constrained or young and inexperienced households, suggesting mechanisms at play are likely to be more pervasive.

5. Discussion

5.1. Plausible mechanisms

If our finding of an ϵ_{AW} close to 1 is not driven by non-savers and the young, then there must be other mechanisms that induce households to act as if there is little-to-no fungibility between mortgage repayments and savings in their other accounts. Here we discuss (rational) liquidity wedges, and a number of behavioral factors, such as mental accounting, default settings and pre-commitment.

One possibility could be a liquidity wedge. Extracting home equity involves non-trivial cost and time. In addition, if house values fall during economic downturns, then funds paid into reducing mortgage debt could be inaccessible (ex. negative home equity, Campbell et al. 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 actually be states when constraints make it costly or impossible to access home equity. In that case, households might rationally treat mortgage repayment and liquid assets as not fungible from a short (or medium)-term consumption smoothing standpoint, even if they have similar effects for 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 the elasticities are high for this group as well.

While a liquidity wedge undoubtedly plays a role in our findings, it is not clear it supported by the full set of our findings. First of all, the finding of virtually no response of liquid financial assets to the increased debt repayment suggests an elasticity of intertemporal substitution (EIS) that does not appear consistent with the prevailing literature. Households appear willing to cut consumption substantially today in order to avoid *any* increased risk in the need to cut that 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 to engage in exactly that. Another possibility is that household behaviors may deviate slightly from strictly fully rational models, overestimating their one need for liquidity in the future. D'Acunto et al. (2020) for example, find that when given access to an overdraft facility even very liquid users act as if they are severely constrained, something they call the "perceived

precautionary savings motive". This is consistent with the large observed consumption response of individuals to randomized credit line expansions, even if they have substantial liquidity (Aydin 2019), and to income payments (Olafsson and Pagel 2017).

Apart from this "perceived precautionary savings motive", there is a wider range of behavior models that could explain our findings. Kovacs and Moran (2019) and Vihriala (2019) both provide theoretical arguments supporting the idea that mortgages could act as a commitment device for present-biased households. Beshears et al. (2015) find in an experimental setting that people appear to value commitment devices. Yet Laibson (2015) notes that, based on the empirical evidence, there are much fewer commitment devices than one would expect. Our works suggests that amortizing mortgages might be just such a commitment device. Vihriala (2019) finds that in Finland most households do not take the free option to have the ability suspend mortgage amortization for a period of time and, among those that do, 46% of households choose a shorter period than allowed, even though it is explicitly costless to replicate the shorter period with a longer one by simply paying the full amortizing payment each month whenever the household decides. Vihriala argues that this suggests that some households are aware of the value of amortization as a commitment device and purposefully choose their mortgage contract with that intent in mind. Indeed, consistent with our findings, though of course confounded by the endogenous selection of the timing, Vihriala (2019) finds that consumption falls when households' non-amortization period ends. In our setting we find that even households with substantial liquid assets respond similarly, which suggests that if a commitment device mechanism is at work it is only needed to help households accumulate long-run wealth, not shorter-run precautionary savings.

Another plausible mechanism is that households may, for mental accounting reasons, treat mortgage repayments as bills instead of wealth accumulation. Di Maggio et al. (2017) look at downward interest rate resets in the U.S. as an anticipatable increase in liquid wealth and find substantial increases in consumption. Zator (2019) looks at similar rate changes in Poland and find that increases in rates lead to increases in labor income. In those settings, just like ours, the authors look at changes in required monthly mortgage payments. Our analysis, though, focuses on changes in mortgage payments coming from increased debt repayment, which are instantaneously net worth neutral, while these papers look at changes in mortgage payments driven by changes in interest rates (fees) paid, which actually change the net worth of the household. These are fundamentally different shocks since in the long-run amortization in essence just moves money around within a household's accounts, from one pocket to another, while changes in interest rates remove or add funds from household accounts completely. In fact, it is surprising that Zator (2019) finds that about 30-45% of the increase in mortgage payment comes from a change in labor supply, which is similar to what we find for our increase in mortgage payments, despite completely different shocks

at play. This would be consistent with what Camanho and Fernandes (2018) call the "mortgage illusion". In an experimental setting they show that households compare rental payments to mortgage payments when deciding to own or rent and 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 the amortizing component of the payments are higher making the regular monthly payment higher. These findings are also consistent with Argyle et al. (2019) who document that auto loan borrowers have substantial sensitivity to changes in maturity, likely reflecting strong sensitivity to monthly payment sizes, even if they are just amortizing payments. Just like in our setting these results hold even among unconstrained borrowers, which may suggest liquidity constraints by themselves may only offer a partial explanation.

Another possible behavioral mechanism could be that amortization constitutes a "default" setting to wealth accumulate. A broad literature in multiple countries has shown evidence that default settings with passive choice, such as 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). These substantial wealth accumulation effects are rarely seen for policies that require active choice (opt-in policies) such as voluntary debt repayments (Kuchler and Pagel 2019). In our setting, after 2013 amortizing mortgages become the default contract, and with them increased mortgage repayments. As we have noted before, if households don't offset these increased default mortgage repayments, by reducing their net non-mortgages savings, then they will wealth accumulate. Though the authors don't call it this, Beshears et al. (2019) implicitly examine debt-savings fungibility when they explore whether default settings that increase U.S. individual pension contributions are offset by increased liabilities. In their main regression specification, they find no effect of increased retirement contributions on credit scores or debt balances, which would be consistent with the little-to-no debt-savings fungibility, just like we find in our setting[35]. In our setting, we also observe a more complete picture of the household balance sheet, including home equity, financial assets, and other liabilities, allowing us to provide increased support for the plausibility of the low fungibility observed in that literature[36]. While this is suggestive that low levels of debt-savings fungibility might be possible, it isn't necessarily obvious ex-ante that households treat this fungibility as symmetric.

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[35] In their settings they do have alternative specifications with some limited evidence of offsetting initial mortgage and auto-loan liabilities, but they note that those are not their preferred empirical designs and they can't observe all household assets. The authors conclude that there is therefore some risk that these alternative findings could be driven by changes in the size of say the house purchased, meaning higher first mortgage balances might not actually be offsetting retirement pension contributions. This seems even more likely given the lack of evidence of any increased cash out refinancing or mortgage prepayment speeds. This would be another mechanism, they could observe more cleanly, where households could offset pension contributions in their home equity, and they don't find anything.

[36] Garcia et al. (2020) also don't find any evidence that savings nudges increase high-interest unsecured borrowing in a setting where the observe a higher frequency picture of household balance sheets.

Even if default settings for pension contributions don't alter liabilities, increased amortization requirements could still alter net non-mortgage savings decisions. As noted by Chetty et al. (2014), the mechanism by which households are encouraged to wealth accumulate has critical implications for its effectiveness. As we have noted, there are multiple channels by which amortization might encourage households to wealth accumulate, with default settings being one. All we can say with more certainty is that it appears that our evidence is consistent with low fungibility for the effects of mortgage amortization on net non-mortgage savings, leading to substantial effects on wealth accumulation.

All this being said, while amortization may drive wealth accumulation due to behavioral factors such as commitment devices or mental accounting it is also possible that more rational channels could be at play. It could even be some combination, where households act overly concerned about liquidity differences between these accounts. Within this particular design it may be hard to disentangle exactly which of these potential drivers are at work. What we can say is that households do not seem to treat mortgage debt repayments and other more liquid accounts as substitutes, even among those with large liquid assets and the old, causing increases in mortgage amortization to substantial raise wealth accumulation.

5.2. External Validity and Implications

While there is no silver-bullet to verify external validity outside the Netherlands, prior work looking at other countries finds results broadly consistent with our results. This provides support that even if exact elasticities may vary in other mortgage environments or populations, 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 reductions 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 net worth/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 just the effects of mortgage amortization on wealth accumulation.[37] The closest

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[37] Households in sufficient distress to file for mortgage assistance might be especially financial constrained. Households about to pay-off their mortgage appear to increase bank loans and already start to reduce their labor income even in the years prior to the run-off making it difficult to assign all effects to just the amortization component. It may be that households for whatever reason treat fully paying off their house as something special or, since effects begin prior to the full run-off, that reduced interest payments or life-cycle events matter in addition to amortization effects in this setting.

work to our own 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 use IO mortgages, appear to have higher consumption growth. This is driven by those who refinance into IO mortgages. Larsen et al. (2018) find that those who choose to take out IO mortgages tended to be older or younger (rather than middle age), and consumed more afterwards, but did not alter their financial assets[38]. Households who choose to use IO mortgages in Denmark at this time tended to have ex-ante lower savings rates and higher loan-to-value ratios (Kuchler 2015). Moreover, the timing of refinancing into an IO mortgage is almost certainly related to household time-varying conditions. It is therefore plausible that these findings are a combination of the effects of reduced mortgage amortization and selection. Although these papers do not look at wealth accumulation, the correlations they find between amortization and consumption/financial asset accumulation are at least consistent with our findings.

Another closely related literature that is broadly consistent with our findings, is evidence on default and alternative mortgage products (AMPs) such as interest-only (IO) loans and option adjustable-rate mortgages (ARMs). Option ARMs give households the option to pay down the principal as a fully amortizing mortgage or not at all, like an interest-only loan. Under some conditions, especially a lack of self-control problems, this can be a useful feature. Rather than being forced to put wealth into mortgage repayments that are costly to reverse, option ARMs provide the flexibility to allow households to retain savings in liquid assets when they need them (Piskorsi 2010). Cocco (2013) shows that households taking out AMPs did so to relax financial constraints and tended to have higher average future income growth allowing them to transfer consumption from the future to the present. At the same time, Amromin et al. (2018) show that when you control for the fact that people who took out AMPs in the U.S. had higher incomes and credit scores, default rates were twice as high as those for normal mortgages. Default is not sufficient statistic to judge whether option ARMs are good or bad (Piskorski 2010). Moreover, the choice of mortgage type is endogenous. Nevertheless, these results suggest that U.S. households do not use the extra funds available from option ARMs to increase their non-mortgage savings that they could use to prevent a costly default. This is consistent with our results that changes in mortgage amortization do not lead to a change in non-mortgage savings. This raises the question whether option ARMs are as attractive in reality as some models might suggest.

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[38] De Stefani and Moertel (2019) aggregate these consumption effects using regional variation in financial constraints as a proxy for areas more likely to use interest-only mortgages. Backman and Lutz (2020) find no effect on homeownership rates.

Aggregate statistics on retired households also provide support for our findings holding in other settings. Households appear to use their home equity as a primary form of savings, with real estate accounting for over 70% of U.S. households assets (Campbell 2006). Even among retirement age households, real estate is by far their 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 (PRA) such as individual retirement accounts (IRA) or 401(k) plans (Poterba 2011). 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 wealth. Households may to prefer to invest in housing as an asset, either because they believe it has a higher risk adjusted return (Kaplan and Violante 2014), or acts as a hedge for local rental rates. 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. Alternatively, as our results suggests, households simply treat home equity and liquid savings differently.

Our results have important implications for macroprudential policies more broadly. In the context of the specific policy we study in this paper, the Dutch government was hoping that amortizing mortgages would improve financial stability. If households responded to the regulation by transferring liquid assets, such as deposits, into mortgage repayment, with no change in net-wealth, it is unlikely such a policy would improve financial stability. By contrast, our findings confirm that increased amortization has increased household home equity, with potential benefits from reduced housing lock (Bernstein and Struyven 2019), without reducing liquid savings. In other words, the regulation has worked as intended. 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. Our findings also suggest that proposed macro-prudential policies, such as changing amortizing mortgages into interest-only during recessions (Campbell et al. 2019), 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 meaning. This suggests that countercyclical amortization reductions could have consumption/labor responses similar to those seen for mortgage designs with countercyclical interest rates (ex. Guren et al. 2019), but with very different costs/wealth implications.

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 2010). The evidence to support that view though 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. Barrot et al. (2019) show that households tend to extract and "spend" substantive house price appreciation via changes in expenditures and income[39]. Sodini et al. (2017) study condos, for which mortgages in the period under study 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 suggest that a critical mechanism for homeownership to lead to wealth accumulation is the coupling with amortizing mortgages.[40] This would also be broadly consistent with the aforementioned observed aggregate statistics, with households at retirement having substantial illiquid wealth in the form of housing and few liquid financial assets. On the flip-side, reducing homeownership either overall or for specific groups could help to explain historical differences in wealth accumulation (Charles and Hurst 2002; Krivo and Kaufman 2004; Stein and Yannelis 2020).

Finally, our results have potential implications for optimal design of retirement programs and the types of mechanisms likely to lead them to achieve their goals. Beshears et al. (2019) argue that the social optimal mandatory contribution plan with tastes shocks and present bias is one with three accounts including one liquid account, one illiquid account, and one with costly early liquidation. To the extent these assumptions hold, homeownership with mortgage amortization is an account with costly liquidation that might be a critical component of the optimal policy illiquidity mix of long-run household wealth accumulation.

6. Conclusion

In this paper we provide the first empirical evidence on the effects of mortgage amortization on wealth accumulation by examining a 2013 amortization regulation in the Netherlands for first-time home buyers. Using detailed individual-level administrative data and variation in the timing of purchases by first-time home buyers surrounding the regulation, we find that even four years later there is no observable change in non-housing savings, leading to a near 1-for-1 rise in net worth with the rise in mortgage debt repayments. The effects occur suddenly, and only for cohorts who are exposed to the regulation. We find no evidence of bunching and results are unchanged using the timing of life-events (ex. birth of a child) as

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[39] By contrast Leth-Petersen (2010) find relatively little effect of allowance of home equity withdrawals for consumption purposes when they were first allowed in Denmark among households with relatively little house price appreciation. Kovacs and Moran (2020) find some more evidence in the U.S. setting, but do not clearly separate out the effects of house price appreciation from mortgage repayment.

[40] 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, and potentially even wealth accumulation depending on long-run labor/consumption responses (ex. Goldsmith-Pinkham and Shue 2020).

an instrument for buying before vs. after the regulation. The rise in wealth accumulation is achieved through an increase in labor income and reduction in expenditures among the group of buyers exposed to the regulation. Our findings hold looking at households with substantial liquid assets, suggesting results are not caused by just non-savers, and across a broad range of ages, suggesting a general applicability of our estimated elasticity. Our results can either be consistent with a relatively atypical rational model of household liquidity preferences or behavioral models that have been shown to help drive wealth accumulation in other settings (such as pension contributions), including commitment devices, mental accounting, and default settings.

Regardless of the underlying mechanism, the finding of a substantial effect of mortgage amortization and debt repayment on wealth accumulation has important economic implications. Our estimated elasticity suggests that ex-ante macroprudential polices aimed at building up household home equity through faster amortization are effective and do not significantly reduce household liquidity. Ex-post macroprudential policies that reduce principal repayments during recessions are also likely to have larger effects than might be expected in more standard models of the household. More broadly, our results suggest that homeownership can be a critical driver of household wealth accumulation when financed with an amortizing mortgage. Therefore, variation in homeownership and amortization rates across time and groups, are likely important factors drivers of differential wealth accumulation. For many households, the home is the single largest asset they have available in retirement. It may not be surprising then that our findings suggest that mortgage design may play a key role in our understanding of household retirement savings and long-run wealth accumulation.

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

These are summary statistics for 2014 for the CBS administrative datasets of 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. This is the sample that is used for Figure 1 and a sub-sample of these for cohorts closer to the 2013 regulation is used in the subsequent regression tables, but under the same criteria.

	Mean	Median	Stdev	25th	75th	N
Mtg LTV '14	1.02	1.05	0.19	1.01	1.09	111,523
Mtg Origination Balance '14 (€)	203k	187k	88k	151k	234k	111,523
Total Liabilities '14 (€)	211k	193k	97k	155k	242k	111,523
Income '14 (€)	73k	66k	36k	49k	88k	111,523
Liquid savings '14 (€)	18k	7.8k	34k	2.6k	21k	111,523
ΔLiquid savings '14 (€)	1.3k	0.3k	8.6k	-1.2k	3.4k	111,523

Table II. 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 regulation. Column 1 regresses the amount of mortgage repayment from January-December 2015 for a given household 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 – February 2013, while the treated are those who closed from May-Sept 2013. Since the regulation applied to the timing of going under contract, not closing, which typically takes at least 2 months, but is uncertain for those who closed in March and April those months are excluded. Relative to Figure 1 and the sample included in the summary statistics this is the same identified group of first-time home buyers, but focusing more narrowly on those buying closer to the regulation change. Column 1 becomes the 1st stage of our two-stage least squares regression. Column 2 is the same 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, but 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 looking at the effects of wealth accumulation over the same period. Column 4 is the same as column 3, but looking at only non-housing wealth accumulation 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%.

	1st Stage (1) MTG Repaid '15	RF (2) ΔWealth '15	IV (3) ΔWealth '15	IV (4) ΔNon-Home Wealth '15
Post	2045.0*** (19.22)	2030.8*** (14.34)		
MTG Repaid '15			0.993*** [0.88,1.10] (17.62)	-0.0053 [-0.12,0.11] (-0.09)
IV	-	-	Post	Post
F-Stat	-	-	369.3	310.9
Obs	42,468	42,468	42,468	42,468
Adj. R^2	0.020	0.011	0.331	0.002

Table III. "Paying" for Wealth Accumulation

This table shows how households alter labor supply in order to pay for the increase in wealth accumulation caused by rise in mortgage amortization. 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. The control group are all buyers who closed on their homes from October 2012 – February 2013, while the treated are those who closed from May-Sept 2013. Since the regulation applied to the timing of going under contract, not closing, which typically takes at least 2 months, but is uncertain for those who closed in March and April those months are excluded. Relative to Figure 1 and the sample included in the summary statistics this is the same identified group of first-time home buyers, but focusing more narrowly on those buying closer to the regulation change. 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 carried out in Table 2, but where the dependent variable is the change in gross household income from 2012 to 2015. Column 3 is the same as 2, but after including fixed effects for municipality and controls for the 2010 natural log of household income and financial assets. Column 4 is the same as column 3, but where the dependent variable is 2012 gross household income. Column 5 is the same as column 3, but where the dependent variable is 2015 gross household income. Column 6 is the same as column 3, but where the dependent variable is 2015 household wealth accumulation. 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) ΔIncome '15-'12	(2) ΔIncome '15-'12	(3) ΔIncome '15-'12	(4) Income '12	(5) Income '15	(6) ΔWealth '15
Post	1270.1*** (5.08)					
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. R²	0.001	0.001	-0.046	-0.015	-0.005	0.319

Table IV. 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 regulation. Life-events are defined to be months with changes in the number of members of a household (ex. birth of a child). Columns 1-3 are covariate balance tests to show that the timing of the life-event does not appear correlated with pre-regulation household characteristics. In particular, Column 1 regresses gross household income in 2010 on *Post(life event)* dummy variable equal to 1 if they had a life-event after March 1st, 2013. The control group are all buyers with a life-event from November 2012 – February 2013, while the treated are those with a life-event from March-Sept 2013. Relative to Table 2 and the sample included in the summary statistics this is the same identified group of first-time home buyers, but focusing only on those that experience a life-event in 2012 or 2013 and more narrowly on those buying closer to the regulation change. Unlike in Table 2 we are less concerned with making sure all households are clearly before or after the regulation, since we are relying on an intent-to-treat from life-events in this case anyway. Column 2 is the same as column 1, but with the change in financial assets in 2010 (financial asset savings) is the dependent variable. Column 3 is the same as column 1, but where wealth accumulation in 2010 is the dependent variable. Column 4 is the same as column 1, but the amount of mortgage repayment from January-December 2015 is the dependent variable and is the first stage of the primary two-stage least squares regression in this table. Column 5 is the same as column 1, but the dependent variable is wealth accumulation in 2015. This is the reduced form regression of the primary two-stage least squares regression in this table. Column 6 is the same as column 1, but the dependent variable is the percent increase in the assessed value of the house over 2015. Column 7 is the full sample of all life-event buyers (not just those that buy a house) without a house at the end of 2011. The dependent variable is a dummy variable equal to 1 if the household owns real estate by December of 2016 regressed on a dummy variable equal to 1 if the life event occurs after, relative to before the regulation change. Column 8 is the formal two-stage least squares regression using life-event post the regulation as the instrumental variable, just like in the first stage regression of column 4, and then regressing the instrumented mortgage repayment in 2015 on wealth accumulation over the same period. Column 9 is the same as column 8, but excludes any life-events in the same month as the households move month and excludes life-events in March since the ambiguity of treatment in that month reduces the power of the 1st stage to a level more open to concerns about weak instruments. 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			1st Stage	RF			IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	HH Income '10	ΔFinancial Assets '10	ΔWealth '10	MTG Repaid '15	ΔWealth '15	%ΔHome Value '15	Have Real Estate '16	ΔWealth '15	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)	-0.000016 (-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. R²	-0.000	-0.000	-0.000	0.003	0.001	-0.000	-0.000	0.355	0.360

Table V. Persistence of Effects & Convexity of MTG Amortization Schedule

This table shows persistence in the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 regulation. Column 1 runs the two-stage least squares regression in Table 2 column 3, but focusing only on the sub-set of buyers who closed on their properties in the first 4 months of 2013. In particular, dependent variable is wealth accumulation over 2015 and the endogenous variable is mortgage repayment over the same period, instrumented for using a dummy variable, *Post*, equal to 1 if they closed on their house after March 1st, 2013. The control group are all buyers who closed on their homes from January–February 2013, while the treated are those who closed from March-April 2013. Relative to Figure 1 and the sample included in the summary statistics this is the same identified group of first-time home buyers, but focusing more narrowly on those buying closer to the regulation change and only closing in 2013. Column 2 is the same as column 1, but the dependent variable and endogenous variable of interest are for 2016. Column 3 is the same as column 1, but the dependent variable and endogenous variable of interest are for 2014. Column 4 is the same as column 1, but the dependent variable is the wealth accumulation from January-December 2016 minus wealth accumulation from January-December 2014 and the endogenous variable is mortgage repayment from January-December 2016 minus mortgage repayment from January-December 2014. 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) ΔWealth '15	(2) ΔWealth '16	(3) ΔWealth '14	(4) ΔΔWealth '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			0.940*** [0.88,1.01] (28.64)	
ΔMTG Repaid '16-'14				1.083*** [0.38,1.78] (3.03)
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	5/13-9/13	5/13-9/13
IV	Post	Post	Post	Post
F-Stat	37.4	428.9	572.6	11.5
Obs	15,223	38,741	41,395	38,741
Adj. R^2	0.259	0.326	0.581	0.340

Table VI. Not Driven by Non-Saving Households

This table shows that even for households with significant savings what the effect is of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 regulation. Column 1 runs the two-stage least squares regression in Table 2 column 3, but focusing only on the sub-set of buyers with a loan-to-value ratio below 90% as of the end of 2014. Column 2 is the same as column 1, but instead on the subset with a loan-to-gross household income ratio below 4 at the end of 2014. Column 3 is the same as column 1, but instead on the subset of households with at least €10k in liquid financial assets at the end of 2015 or who increased their liquid financial assets by at least €3k in 2015. Column 4 looks at the same sub-group of buyers as in Table 2 column 3, but the dependent variable is household liquid financial assets at the end of 2011 regressed on a dummy variable equal to 1 if the household has more than €10k in liquid financial assets at the end of that same year. Column 5 is the same as column 4, but the dependent variable is liquid financial assets at the end of 2015. Column 6 is the same as column 1, but instead on the subset of households with at least €10k in liquid financial assets at the end of 2010. 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%.

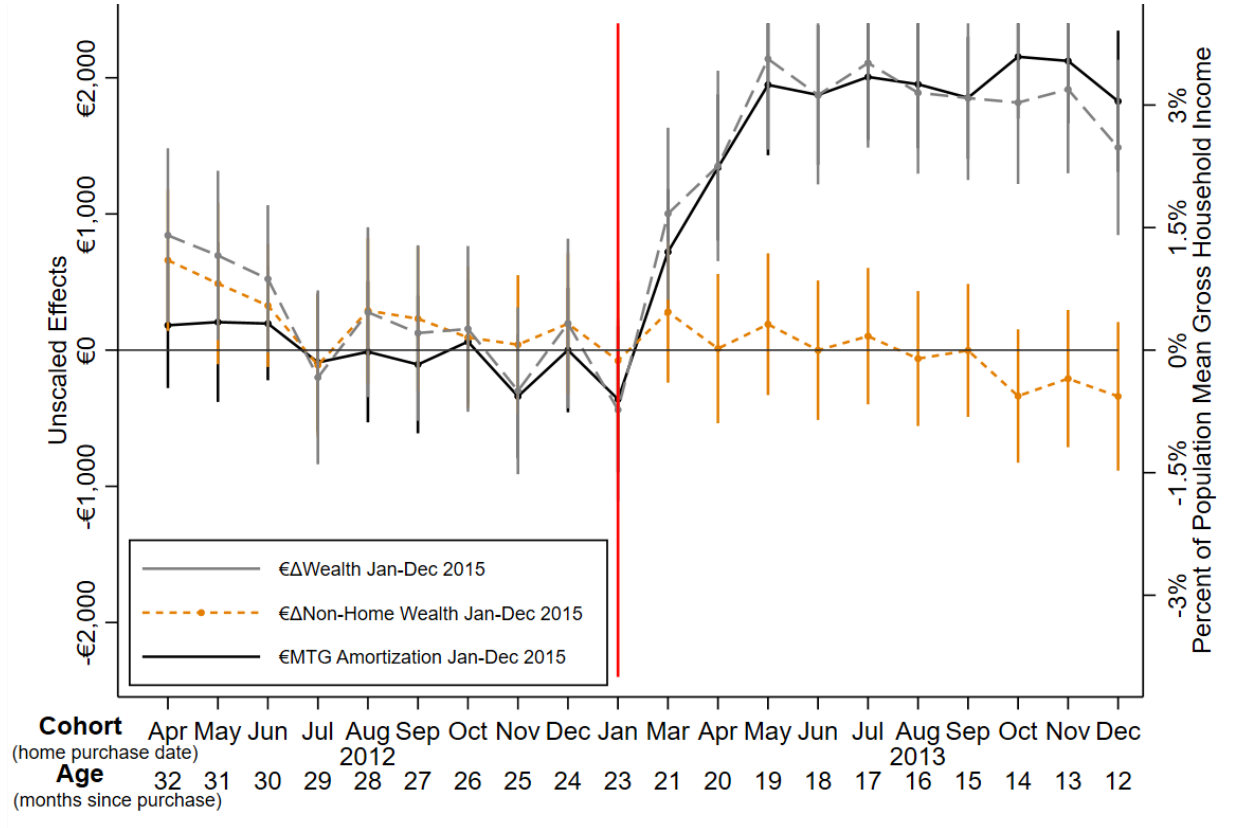|	(1) ΔWealth '15	(2) ΔWealth '15	(3) ΔWealth '15	(4) Fin. Asset '11	(5) Fin. Asset '15	(6) ΔWealth '15
MTG Repaid '15	1.315***	0.959***	0.997***			0.956***
	[0.91,1.72]	[0.82,1.10]	[0.84,1.15]			[0.84,1.07]
	(6.37)	(13.76)	(12.80)			(15.87)
FinAsset'11>10k				43,445***	26,486***	
				(96.66)	(81.06)	
LTV '14	<0.9	-	-	-	-	-
LTI '14	-	<4	-	-	-	-
FinAsset'15	-	-	>10k|>3k	-	-	-
FinAsset'11	-	-	-	-	-	>10k
IV	Post	Post	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 VII. Pervasive Effects

This table shows that even for older households and those that have resold their home what the effect is of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 regulation. Column 1 runs the two-stage least squares regression in Table 2 column 3, but focusing only on the sub-set of buyers with the oldest member of the household (excluding those over 75 years old) is older than 30 years old as of the end of 2015. Column 2 is the same as column 1, but for the oldest in the household greater than 40 years old. Column 3 is the same as column 1, but for the oldest in the household greater than 50 years old. Column 4 is the same as column 3, but excludes households where anyone in the household differs by more than 20 years, in order to avoid multi-generation households. Column 5 is the same as the two-stage least squares specification in Table 5 column 2, but the instrument and the endogenous variable includes interaction with a dummy variable equal to 1 if the household resold their home by the end of 2016. 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) ΔWealth '15	(2) ΔWealth '15	(3) ΔWealth '15	(4) ΔWealth '15	(5) ΔWealth '16
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)	
MTG Repaid '16					1.214*** [0.84,1.59] (6.34)
MTG Repaid '16 x Home Resold					-0.149 (-1.25)
Age	>30	>40	>50	>50	-
GParentFilt	N	N	N	Y	-
IV	Post	Post	Post	Post	Post
F-Stat	274.2	105.0	40.6	25.2	261.9
Obs	34,185	15,668	6,416	5,268	38,741
Adj. R^2	0.327	0.301	0.289	0.177	0.290

Figure I. 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 regulation, following equations 4 and 5 in the paper. In particular, we regress mortgage repayment from Jan-Dec 2015 (solid black line), wealth accumulation from Jan-Dec 2015 (gray dashed line), and non-housing wealth accumulation from Jan-Dec-2015 (yellow dotted line) 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 and we use the full set of all first-time home buyers in the Netherlands over this period. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The second (left) y-axis simply takes these estimates and scales them by the sample's mean gross household income, to provide some idea of the relative magnitudes of the effects. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level.

Figure II. 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 regulation. Life-events are defined to be quarters with changes in the number of members of a household (ex. birth of a child), following equations 4 and 5 in the paper. In particular, we regress mortgage repayment from Jan-Dec 2015 (black) and wealth accumulation from Jan-Dec 2015 (gray) 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 and we use the full set of all first-time home buyers in the Netherlands over this period who have a life-event in 2012 or 2013. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The second (left) y-axis simply takes these estimates and scales them by the sample's mean gross household income, to provide some idea of the relative magnitudes of the effects. The x-axis includes the cohort (quarter of life-event) and the age (quarters from life-event till the beginning of 2015). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level.
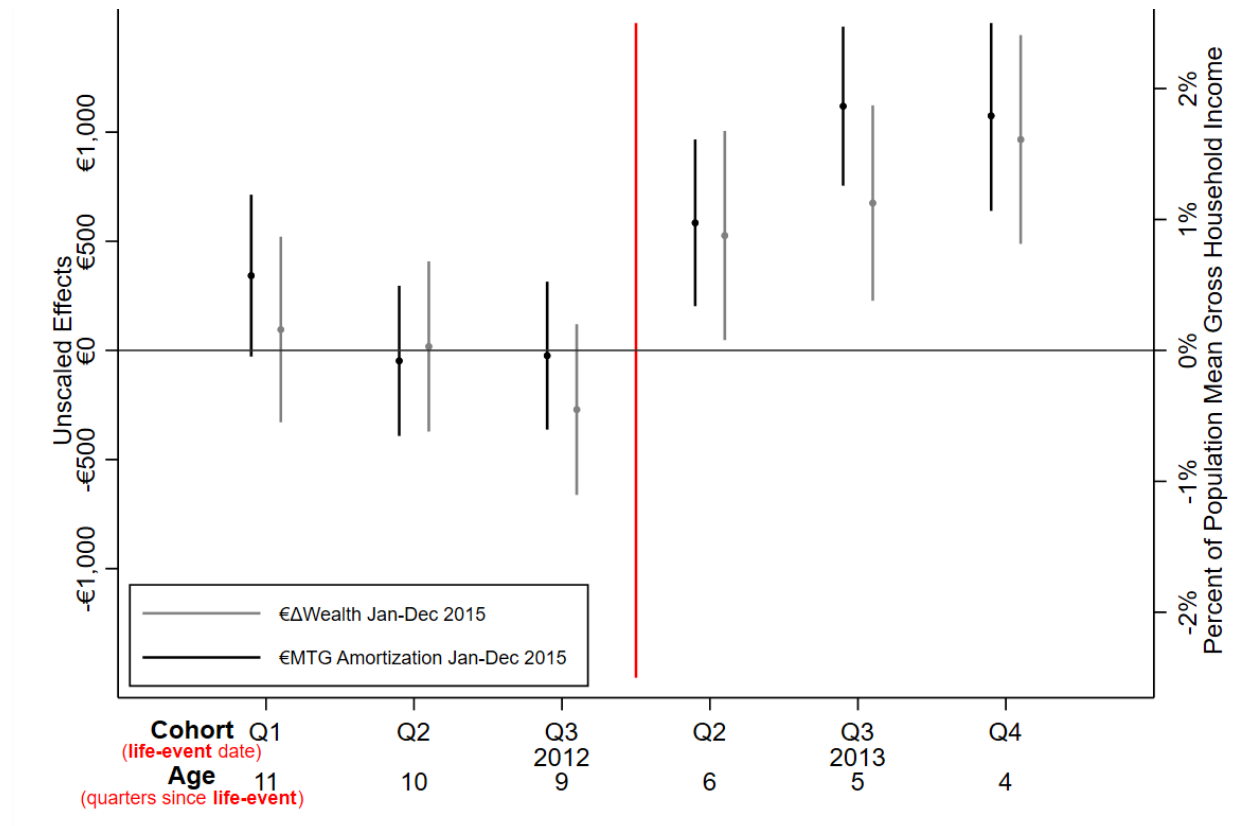
APPENDIX

Table AI. Robustness: Alternative Wealth Measures

This table shows that the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 regulation is robust to the specific measure of wealth used. Column 1 runs the two-stage least squares regression in Table 2 column 3, but including 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 is the same as Table 2 column 3, but includes appraisal values for real estate in the measure of wealth. Column 3 is the same as column 2, but also includes the alternative pension measure from column 1. Column 4 is the same as Table 2 column 3, but is the wealth accumulation (and mortgage repayment) over 2014 and 2015 combined. Column 5 is the same as Table 2 column 3, but is the household's net worth (all assets – liabilities) and home equity as of December 2015 in levels. Column 6 is the same as Table 4 column 8 (life-event IV), but just like column 5 is the household's net worth (all assets – liabilities) and home equity as of December 2015 in levels. 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) ΔWealth '15	(2) ΔWealth '15	(3) ΔWealth '15	(4) ΔWealth '14+'15	(5) Net Worth '15	(6) 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				0.955*** [0.89,1.03] (26.85)		
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(buy)	Post(life)
F-Stat	378.0	369.3	378.0	687.1	472.5	27.0
Obs	41,559	42,468	41,559	42,468	42,468	16,581
Adj. R²	0.316	0.126	0.119	0.467	0.663	0.656

Table AII. Robustness: Alternative Samples

This table shows that the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 regulation is robust to the sample used. Column 1 runs the two-stage least squares regression in Table 2 column 3, but includes all wealth changes (not just those <+/-€100k as in the main analysis of the paper). Column 2 is the same as Table 2 column 3, but 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 is the same as Table 2 column 3, but includes all households and observations regardless of size as long as they purchase a home during the period of interest. This means including all the observations in columns 1 and 2, but will also include non-first-time homebuyers since there are no restrictions on having real estate/mortgages (or not) in the periods prior to the regulation. 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) ΔWealth '15	(2) ΔWealth '15	(3) ΔWealth '15
MTG Repaid '15	1.013***	0.976***	1.000***
	[0.87,1.12]	[0.85,1.12]	[0.92,1.08]
	(13.65)	(14.82)	(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 AIII. Robustness: Alternative Amortization Assumptions

This table shows that the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 regulation is robust to the amortization assumptions used for linked mortgage accounts. Column 1 runs the two-stage least squares regression in Table 2 column 3, but just uses the raw data (no adjustment for potential unobserved linked mortgage accounts). Columns 2-5 are the same as Table 2 column 3, but assume that for those mortgages that are unchanged in 2015, 30%, 40%, 60%, and 70% are actually amortizing via linked mortgage accounts (the assumption in the main analysis is 50%). Column 6 and 7 are the same as Table 2 column 3, but assume that for those mortgages that are unchanged in 2015 the annual interest rate used for amortization of unobserved linked mortgage accounts is 6% and 3% respectively. 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) ΔWealth '15	(2) ΔWealth '15	(3) ΔWealth '15	(4) ΔWealth '15	(5) ΔWealth '15	(6) ΔWealth '15	(7) ΔWealth '15
MTG Repaid '15	0.996***	0.994***	0.994***	0.993***	0.993***	0.994***	0.994***
	[0.92,1.07]	[0.90,1.09]	[0.90,1.09]	[0.87,1.12]	[0.85,1.13]	[0.90,1.10]	[0.90,1.09]
	(27.03)	(21.37)	(19.49)	(15.74)	(13.88)	(19.62)	(15.25)
% unobserved assume amortizing	0	30%	40%	60%	70%	50%	50%
interest rate assumed	4.5%	4.5%	4.5%	4.5%	4.5%	6.0%	3.0%
IV	Post(buy)	Post(buy)	Post(buy)	Post(buy)	Post(buy)	Post(buy)	Post(buy)
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 AIV. Robustness: Standard Errors

This table shows that the effect of mortgage amortization on wealth accumulation using variation in the timing of home purchase for first-time home buyers around the 2013 regulation is robust to method of computing standard errors. Column 1 runs the two-stage least squares regression in Table 2 column 3, but computes heteroskedasticity robust standard errors without any clustering (main analysis clusters at the household-level). Column 3 and 4 are the same as Table 2 column 3, but cluster at the level of 4-digit postal code and municipality (gemeente) respectively. Column 4 is the same as column 3, but observations are collapsed to include only the 1 observation per household per year (the household head). 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) ΔWealth '15	(2) ΔWealth '15	(3) ΔWealth '15	(4) ΔWealth '15
MTG Repaid '15	0.993***	0.993***	0.993***	0.978***
	[0.92,1.07]	[0.89,1.11]	[0.88,1.11]	[0.87,1.09]
	(25.97)	(17.29)	(17.26)	(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 AV. Labor Supply: # of HH Earners

This table shows how households alter the number of household members working in order to pay for the increase in wealth accumulation caused by rise in mortgage amortization. Column 1 regresses the change in the number of household members who are reported as working at least an average of 10 hours per week over a given year 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. The control group are all buyers who closed on their homes from October 2012 – February 2013, while the treated are those who closed from May-Sept 2013. Since the regulation applied to the timing of going under contract, not closing, which typically takes at least 2 months, but is uncertain for those who closed in March and April those months are excluded. Relative to Figure 1 and the sample included in the summary statistics this is the same identified group of first-time home buyers, but focusing more narrowly on those buying closer to the regulation change. Column 2 is the same as column 1, but focused on the subset of households with at least 2 working age people living the household as of 2012. Column 3 is the same as column 2, but looking 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 focusing on only those 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%.

|	(1) Δ#HH Earners '15-'12	(2) Δ#HH Earners '15-'12	(3) ΔSingle Earner HH '15-'12	(4) ΔSingle Earner HH '15-'12
Post	0.0239***	0.0299***	-0.0223***	-0.146***
	(3.36)	(2.65)	(-2.60)	(-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 AVI. Labor Supply: Hours Worked

This table shows how households alter the amount of hours worked to pay for the increase in wealth accumulation caused by the rise in mortgage amortization and that this 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 the administrative record of their total household hours worked in 2012. Column 2 is the same as column 1, but looks at changes in gross household income and household hours worked from 2012 to 2015. Column 3 is the same as Table 3 column 1, but where the variable of interest is the change in total household hours worked from 2012 to 2015. Column 4 is the same as column 3, but is the change in the natural log of total household hours worked from 2012 to 2015. Column 5 is the same as Table 3 column 1, but includes a control for the change in total household hours worked from 2012 to 2015. T-statistics with heteroskedasticity robust standard errors clustered at the household level are shown in parentheses. P-Values: * 10%; ** 5%; ***1%.

	(1) Income '12	(2) ΔIncome '15-'12	(3) ΔHrs Worked '15-'12	(4) ΔHrs Worked '15-'12 (ln)	(5) ΔIncome '15-'12
Post			86.12***	0.0492***	364.2
			(8.35)	(3.22)	(1.59)
ΔHrs Worked '15-'12		10.54***			10.52***
		(40.79)			(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

Figure AI. Variability in Liquid Wealth Accumulation

For the full sample of first-time home buyers from Table 1 for all years 2006-2016 we compute the yearly change in liquid financial assets and regress those on dummy variables for years since a year with a decline in household gross income, after including household fixed effects and year fixed effects.
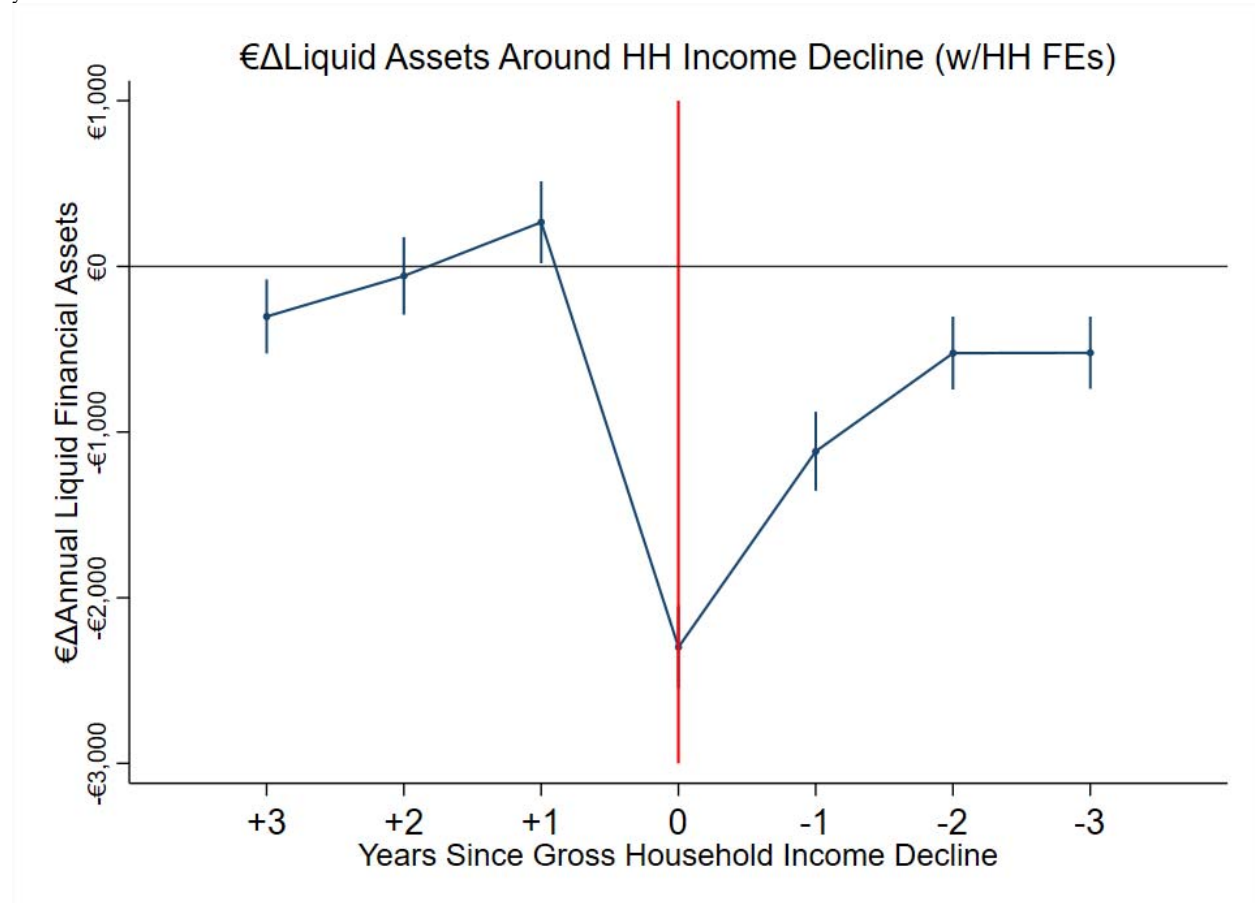
Figure AII. Dutch Macroeconomic Housing Statistics '07-'16

This figure demonstrates general aggregate Dutch housing trends surrounding the January 2013 regulation of interest. House prices (black line) are normalized to be 100 in 2005 and plotted on the left y-axis, while average residential mortgage interest rates (gray line) are plotted on the right y-axis. All data come from aggregate statistics publicly available from aggregate (not micro-level) CBS data.
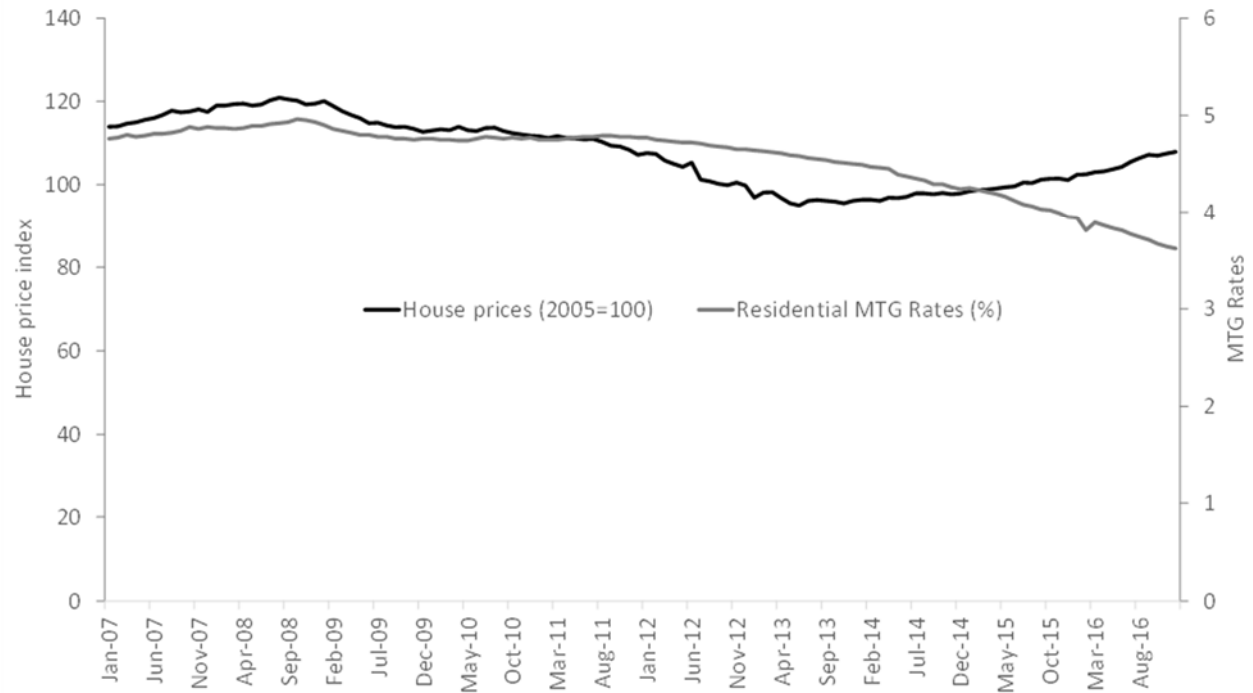
Figure AIII. Origination Loan-to-Value (mean)
by Mortgage Offer Date for First-Time Homebuyers 2012-2013

This figure depicts the average (mean) origination loan-to-value of mortgage offers for first-time homebuyers from 2012-2013 by mortgage offer dates. 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 labeled as clearly for first-time homebuyers for the purchase of a new home, for those age 30 and up, where the mortgage product type is at least partially known. The new mortgage regulations of interest for this paper begin for mortgages originated after December 2012 (vertical red dashed line).
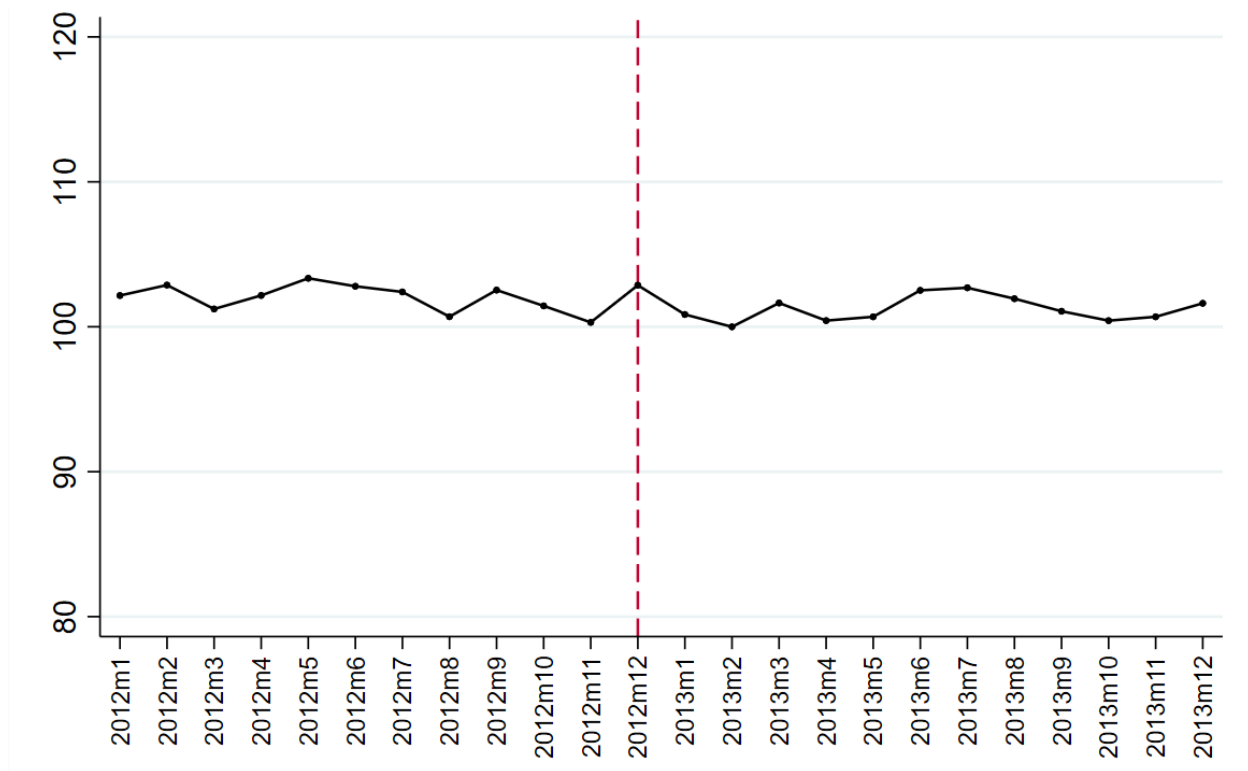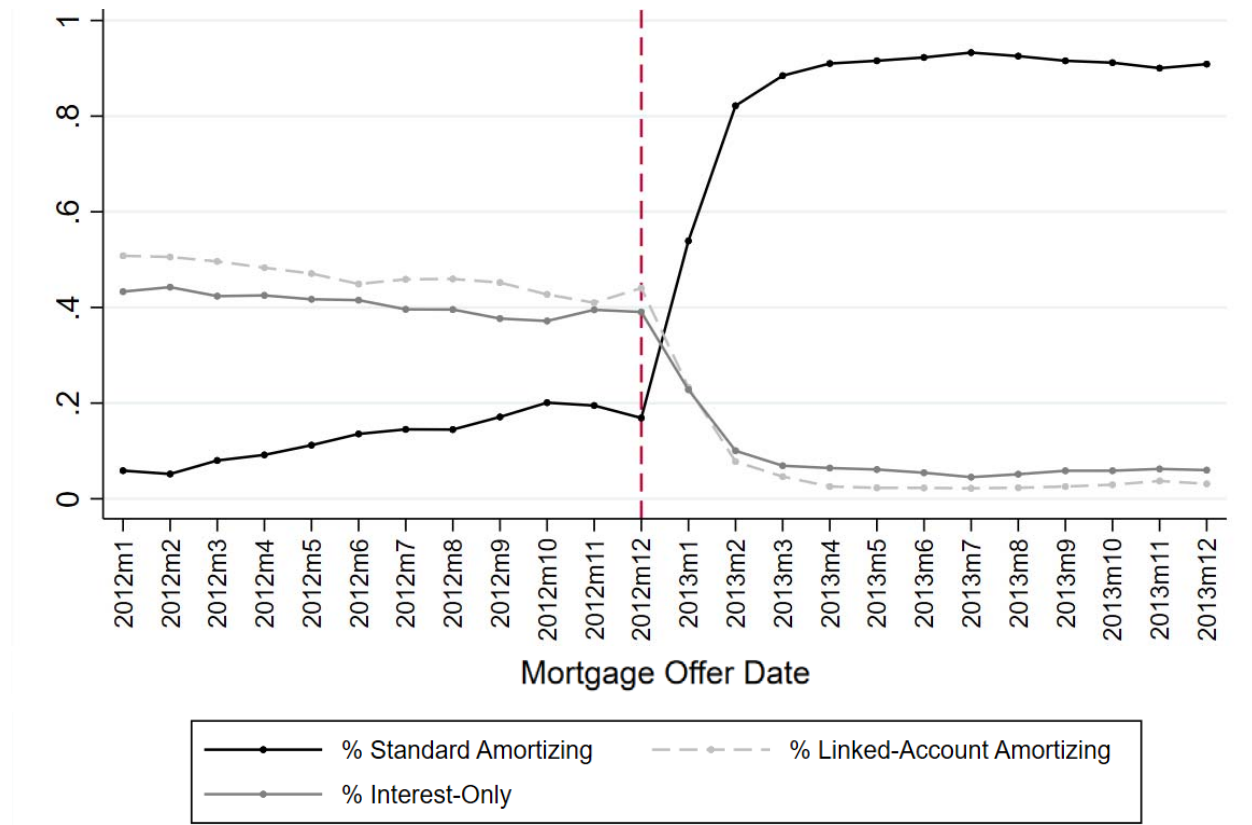
Figure AIV. Average Proportion of Mortgage Balance by Contract Type
by Mortgage Offer Date for First-Time Homebuyers

This figure depicts the average proportion of mortgage offer balances by offer month (not date of closing) by type of product from 2012-2013 for first-time-homebuyers. 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 labeled as clearly for first-time homebuyers for the purchase of a new home, for those age 30 and up, where the mortgage product type is at least partially known. The mortgage types are standard amortizing (black line), linked-accounts that are amortizing, but whose amortizing is not observable in administrative data since it is held as the equivalent of a sinking fund (dashed light gray line), and interest-only (solid dark gray line). The new mortgage regulations of interest for this paper begin for mortgages originated after December 2012 (vertical red dashed line).

Figure AV. Portion Mortgage Balance Expected to Observably Amortize in 2014 by Mortgage Offer Date for First-Time Homebuyers

(relative to August 2012 mortgage offer cohort)

This figure depicts the expected portion of the outstanding mortgage balance expected to be reported as repaid in 2014 according the initial mortgage contract terms by offer month (not date of closing) by type of product from 2012-2013 for first-time-homebuyers. This is driven by the proportion of mortgage balances that are standard amortizing mortgages (since this doesn't include amortization in any linked accounts). 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 labeled as clearly for first-time homebuyers for the purchase of a new home, for those age 30 and up, where the mortgage product type is at least partially known. The new mortgage regulations of interest for this paper begin for mortgages originated after December 2012 (vertical red dashed line).

Figure AVI. Mortgage Amortization & *Hypothetical* Non-Mortgage Wealth under Prefect Fungibility (*F=1*) in 2015 by Date of Home Purchase: 1st Time Home Buyers '12-'13

This figure shows a *hypothetical* effect of mortgage amortization on wealth accumulation under the assumption of fungibility between mortgage repayment and non-mortgage savings, using variation in the timing of home purchase for first-time home buyers around the 2013 regulation, following equations 4 and 5 in the paper. In particular, we regress actual mortgage repayment from Jan-Dec 2015 (solid black line), a hypothetical wealth accumulation from Jan-Dec 2015 (gray dashed line), and hypothetical non-mortgage savings from Jan-Dec-2015 (yellow dotted line) 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 and we use the full set of all first-time home buyers in the Netherlands over this period. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The second (left) y-axis simply takes these estimates and scales them by the sample's mean gross household income, to provide some idea of the relative magnitudes of the effects. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard 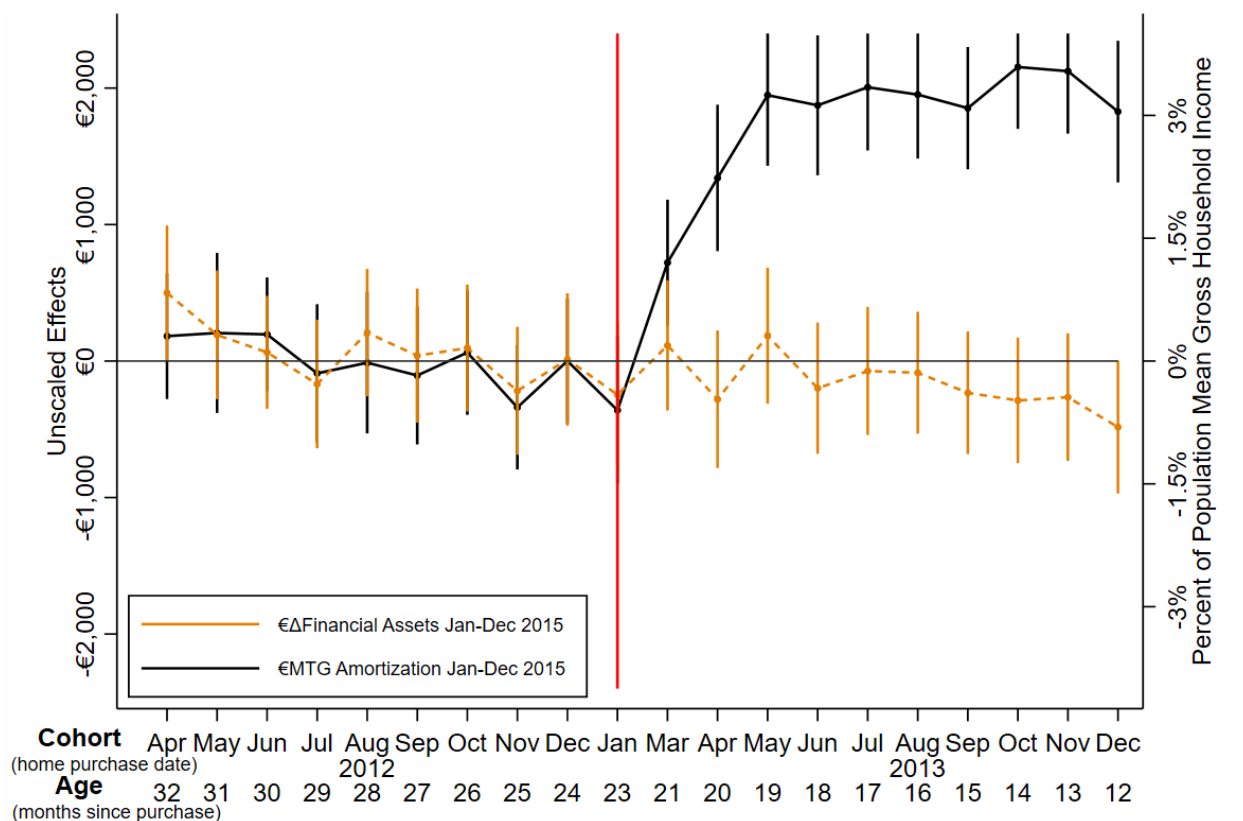errors clustered at the household level. For hypothetical graphs, not using actual data (yellow and gray) no standard errors are included for obvious reasons.
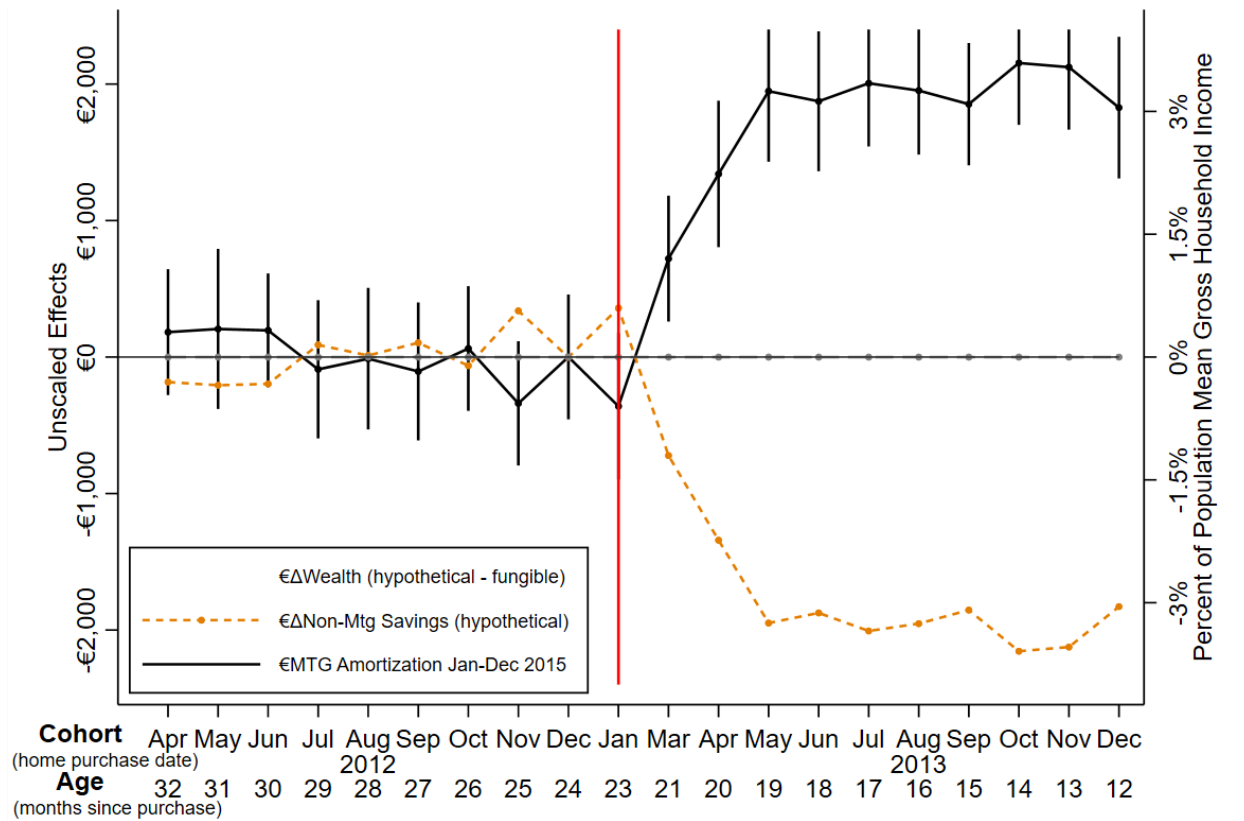
Figure AVII. Mortgage Amortization in 2015 & ΔFinancial Assets
by Date of Home Purchase: 1st Time Home Buyers '12-'13

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 regulation, following equations 4 and 5 in the paper. In particular, we regress mortgage repayment from Jan-Dec 2015 (solid black line) and Δfinancial assets from Jan-Dec-2015 (yellow dotted line) 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 and we use the full set of all first-time home buyers in the Netherlands over this period. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The second (left) y-axis simply takes these estimates and scales them by the sample's mean gross household income, to provide some idea of the relative magnitudes of the effects. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level.
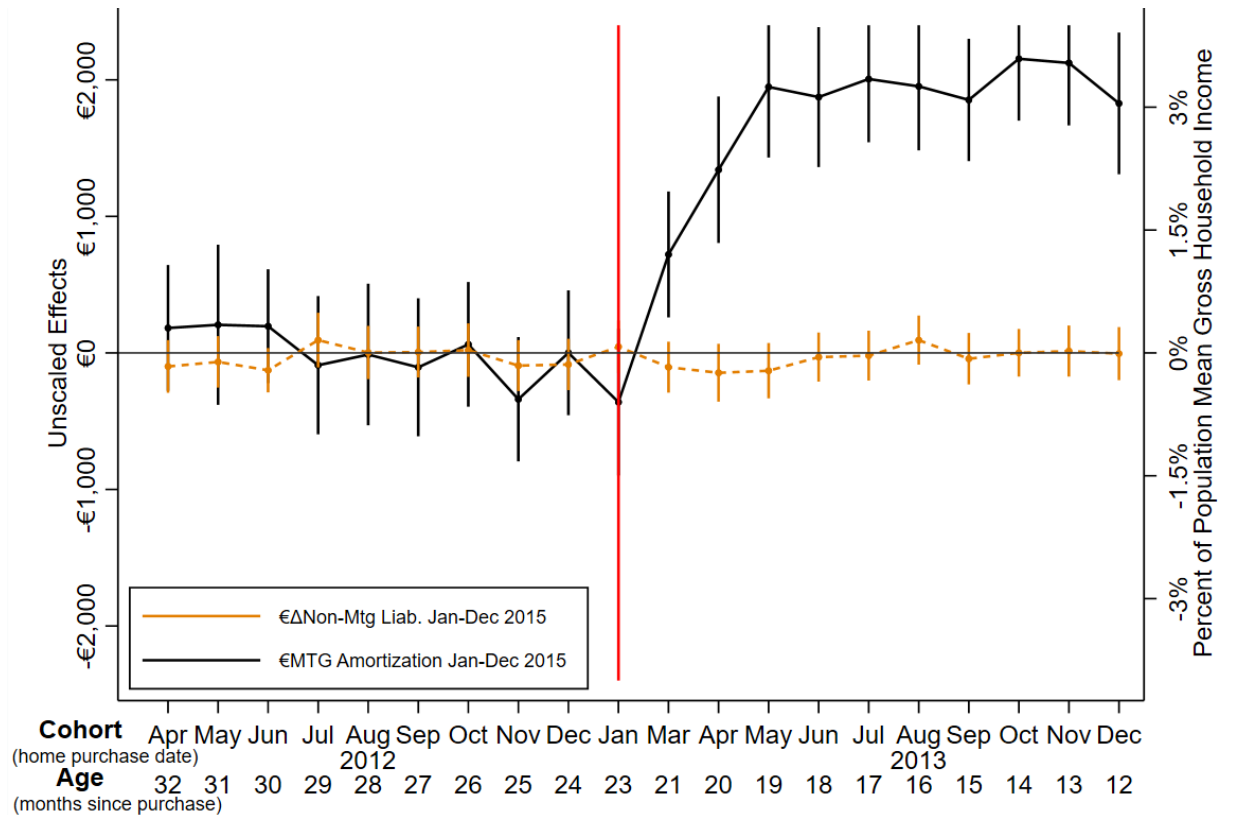
Figure AVIII. Mortgage Amortization in 2015 & ΔNon-MTG Liab.
by Date of Home Purchase: 1ˢᵗ Time Home Buyers '12-'13

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 regulation, following equations 4 and 5 in the paper. In particular, we regress mortgage repayment from Jan-Dec 2015 (solid black line) and Δnon-mortgage liabilities from Jan-Dec-2015 (yellow dotted line) 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 and we use the full set of all first-time home buyers in the Netherlands over this period. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The second (left) y-axis simply takes these estimates and scales them by the sample's mean gross household income, to provide some idea of the relative magnitudes of the effects. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level.

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Figure AIX. Mortgage Amortization in 2015 & # of Transactions 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 regulation, following equations 4 and 5 in the paper. In particular, we regress mortgage repayment from Jan-Dec 2015 (solid black line) from Jan-Dec-2015 (yellow dotted line) on categorical dummy variables for each cohort (month of closing on the house), where February 2013 is the omitted month. We also plot just the number of transactions for each cohort (brown solid line) in each month. No other control variables are included and we use the full set of all first-time home buyers in the Netherlands over this period. Each dot is the estimate for the relative effect each month, with 95% confidence intervals plotted for each point. The second (left) y-axis simply takes these estimates and scales them by the sample's mean gross household income, to provide some idea of the relative magnitudes of the effects. The x-axis includes the cohort (month of closing) and the age (months from closing till the beginning of 2015). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level.
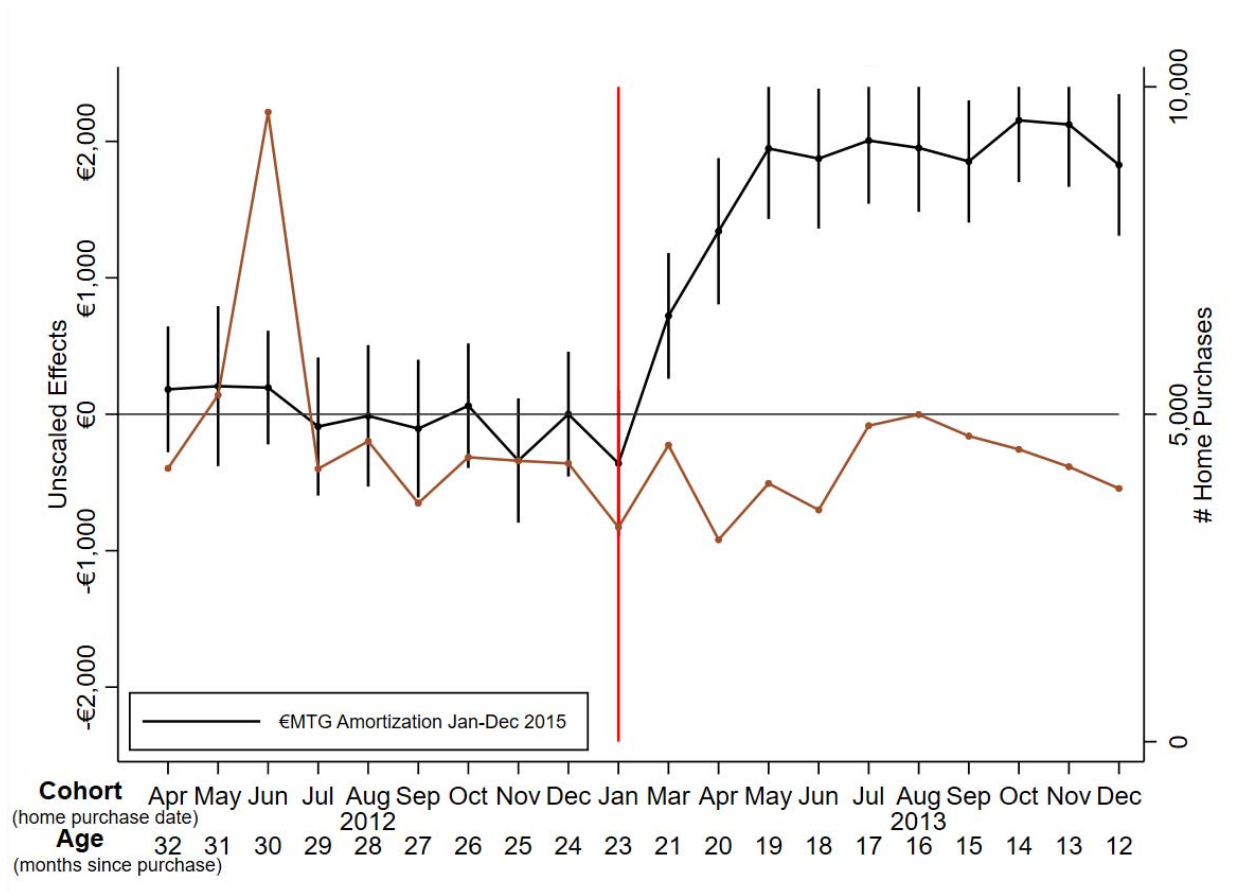
Figure AX. First-Time Homebuyers vs. All Other Buyers

This figure shows differential treatment for first-time homebuyers (FTHBs) vs. all other buyers of the 2013 regulation based on purchase cohorts (months), but no sharp jumps (in absolute terms or relative to each other) in other variables around that date. FTHBs (solid black lines) are defined to be those that purchase a home with a mortgage during the depicted month, but didn't have real estate or a mortgage in the 2 years prior to that. By contrast All Other Buyers (dashed yellow lines) also purchase a house with a mortgage during the depicted month, but do have real estate and a mortgage in the 2 years prior. In panel A we regress the % of the mortgage balance repaid from Jan-Dec 2014 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 and we use the full set of all first-time home buyers in the Netherlands over this period. 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). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level. Panels B-H are the same as panel A, but where the variable of interest are the initial home purchase price in thousands of euros (B), the change in the natural log of financial assets over 2014 (C), the change in financial assets over 2014 divided by the mortgage balance at the end of 2013 (D), the natural log of financial assets as of Dec 2010 (E), the natural log of household gross income as of Dec 2010 (F), the change in the natural log of financial assets over 2011 (G), and the change in the natural log of household gross income over 2011 (H).
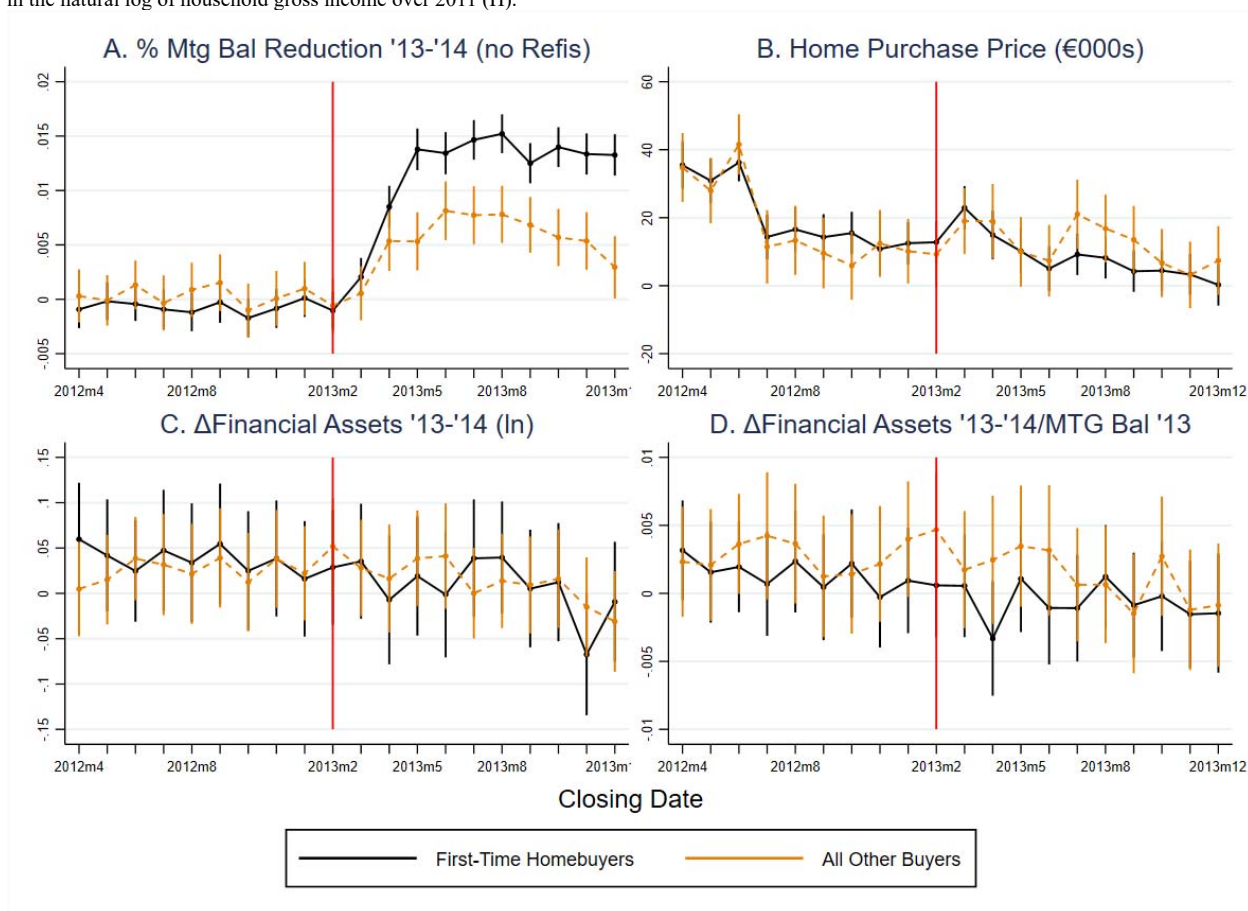
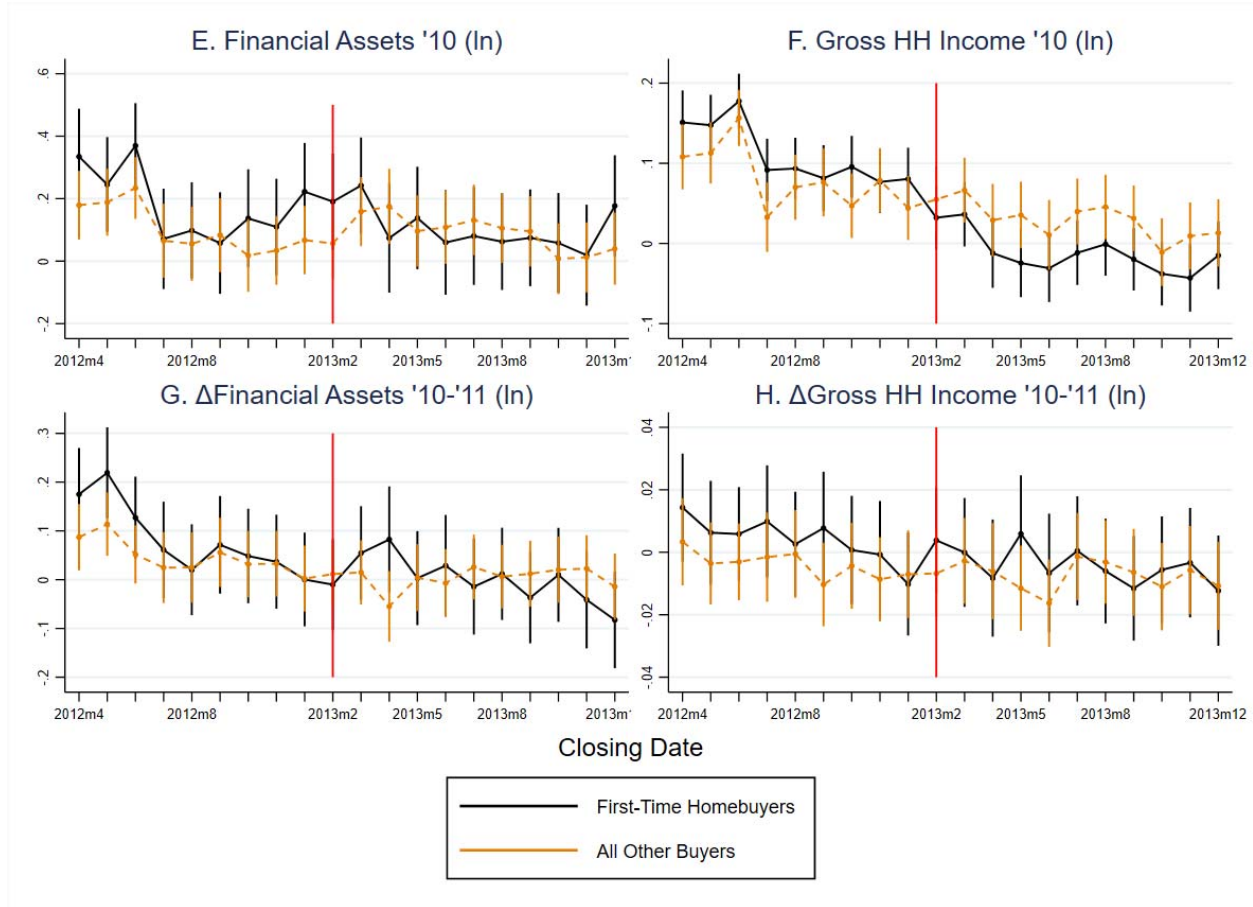# Figure AX. First-Time Homebuyers vs. All Other Buyers (Cont.)

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

This figure shows that there is 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 and don't have a home at the end of 2011, but don't require them to become a first-time homebuyer during this period. We then regress a dummy variable equal to one 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). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level
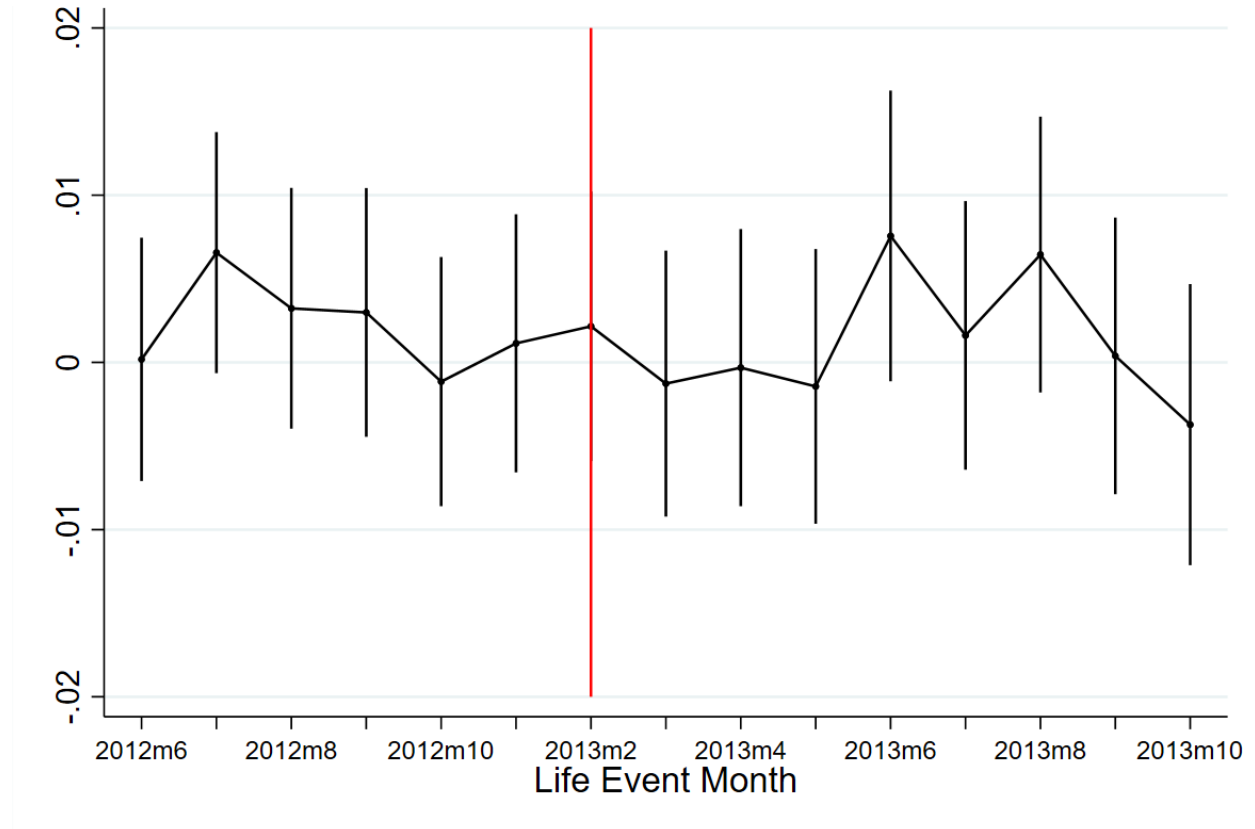
# Figure AXII. Non-First-Time Homebuyers by Previous Housing Tenure Length

This figure shows differential treatment for non-first-time homebuyers with shorter previous housing spells (<10 yrs – solid black lines) vs. those that were longer (>=10 yrs – dashed yellow lines) of the 2013 regulation based on purchase cohorts (months), but no sharp jumps (in absolute terms or relative to each other) in other variables around that date. In panel A we regress the % of the mortgage balance repaid from Jan-Dec 2014 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 and we use the full set of all first-time home buyers in the Netherlands over this period. 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). T-statistics (and 95% confidence intervals) with heteroskedasticity robust standard errors clustered at the household level. Panels B-D are the same as panel A, but where the variable of interest are the initial home purchase price in thousands of euros (B), the change in the natural log of financial assets over 2014 (C), the change in financial assets over 2014 divided by the mortgage balance at the end of 2013 (D).
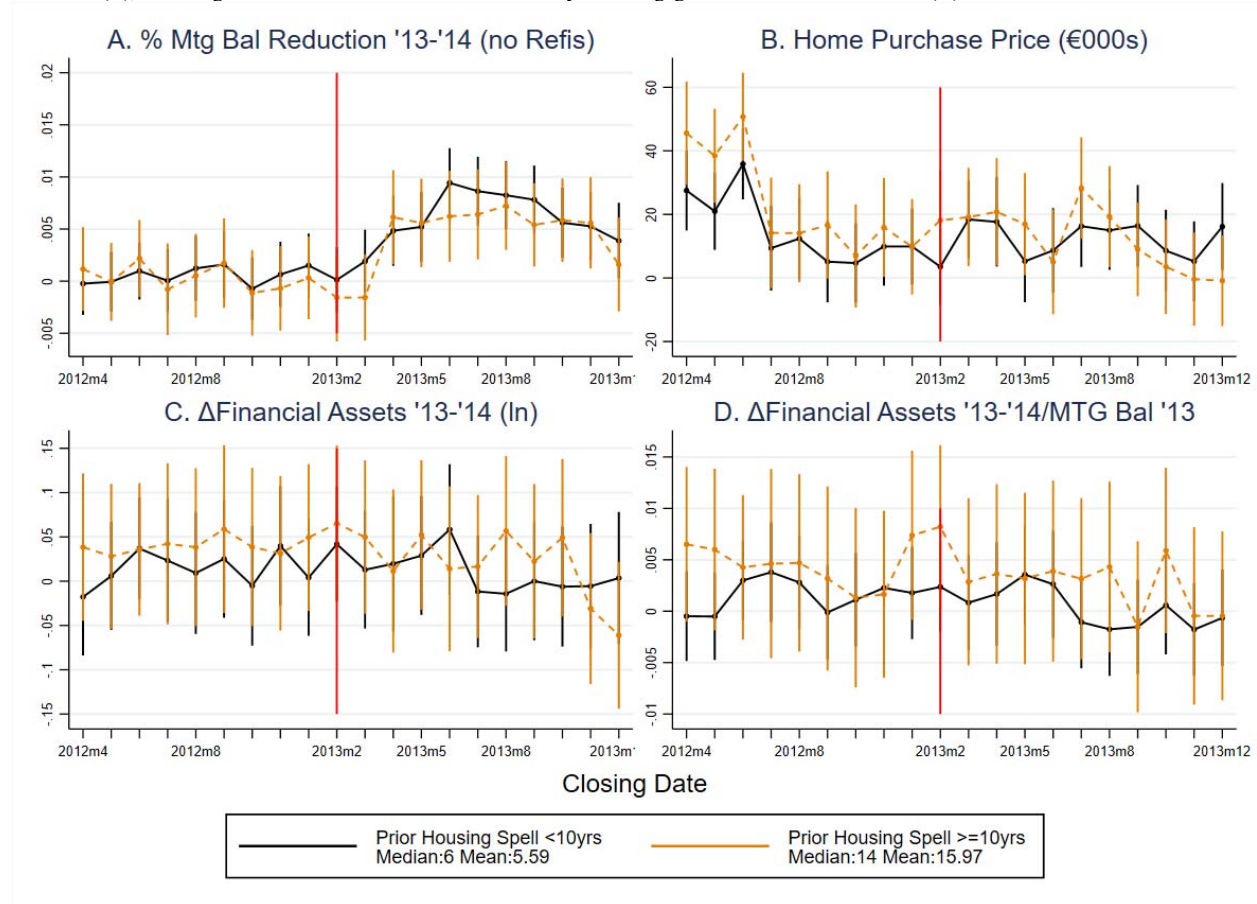
Figure AXIII. Dutch vs. U.S. Homeowners by Age

Panel A depicts the percent of household heads who report having real estate by 5-year age group categories for typical working ages from 20-25 up till 60-65. Panel B reports the percent of homeowners without large levels of liquidity (<$100 USD/€73K) who have any outstanding mortgage balance remaining by the same age groups as Panel A. Data on U.S. households (solid black line) comes from the 2016 Survey of Consumer Finances, while those for Dutch households (dotted gray line) comes from 2012 CBS.

Panel A. Homeownership Rate
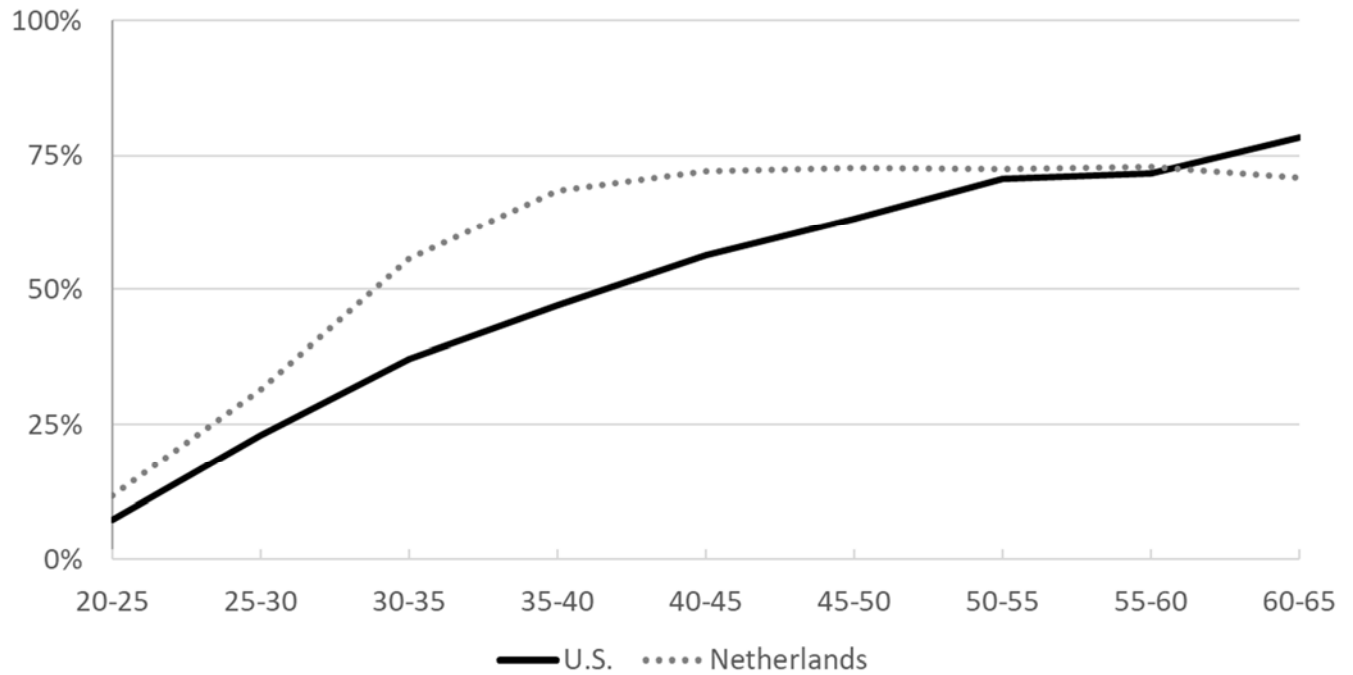
Panel B. % of Homeowners w/ an Outstanding Mortgage Balance Remaining