DNB Working Paper
No 823/December 2024

Residual-debt insurance and mortgage repayments

Yeorim Kim and Mauro Mastrogiacomo

DeNederlandscheBank
EUROSYSTEEM

Residual-debt insurance and mortgage repayments

Yeorim Kim and Mauro Mastrogiacomo *

* Views expressed are those of the authors and do not necessarily reflect official positions of De Nederlandsche Bank.

Working Paper No. 823

December 2024

De Nederlandsche Bank NV
P.O. Box 98
1000 AB AMSTERDAM
The Netherlands

Residual-debt insurance and mortgage repayments

Yeorim Kim [a, b, c], Mauro Mastrogiacomo [a, b, c]

a: VU Amsterdam, *b:* Netspar, *c:* De Nederlandsche Bank

December 2024

Abstract

Mortgagors insured against negative home equity are less likely to partially prepay their mortgage debt compared to those without the insurance. We identify the effect of insurance on prepayments combining two strategies. First we use a regression discontinuity design, enabled by the acceptance criteria of the Dutch insurance which is only accessible for houses below a legislated threshold. After that we add information on (unexpected) intergenerational transfers to the borrowers. We find that insured borrowers make 22.8% lower prepayments relative to their original debt, and we propose that this could be explained by moral hazard. As this insurance was an 'offer you cannot refuse', this is a more likely explanation than adverse selection.

Keywords: moral hazard, mortgage insurance, mortgage prepayment

JEL codes: D14, G21, G51

1. Introduction

Excessive mortgage debt-holding has been linked to a number of negative externalities (Mian et al., 2013). In this paper we discuss this focusing on the Netherlands, the country with the relatively largest mortgage debt in the world. We discuss how borrowers reduce it when they are differently exposed to the risk of residual mortgage debt upon selling their house.

Mortgage owners can reduce their mortgage debt in different ways. Typically one does that contractually by using amortizing loans, such as annuity or linear mortgages, where debt periodically is reduced. Also popular in several markets are saving or investment loans, where saving deposits are accumulated in dedicated accounts that will be used upon maturity to repay the loans. In several cases there is no specific debt reduction, like in the case of interest-only loans. On top of these options it is often possible to prepay debt partly, whereby a borrower voluntarily reduces mortgage debt by transferring some private savings to the mortgage provider. In return, the lower debt grants lower monthly premiums for the residual duration of the mortgage contract, or a shorter duration, depending on specifics in the contract. In this study, we focus on these voluntary actions, thus on partial prepayments.

In western countries, partial prepayments have risen since the early 2000s (e.g. McCollum et al. (2015) for US, and Groot and Lejour (2018) for the Netherlands) and became more popular after the Great Recession (Di Maggio et al., 2017; Groot and Lejour, 2018), along with the drop in the interest rate on savings (Green and Shoven, 1986) that recently followed expansionary monetary policy. Partial prepayment is important not only for understanding mortgagors' behavior or predicting mortgages' performance but also as a financial stability tool for some countries where there is a large share of low repayments (for instance due to a large share of interest-only loans) or highly leveraged loans, such as the Netherlands or Denmark. Compared to the literature devoted to full prepayment, studies on partial prepayments are limited, among others, because of lack of data. As full prepayments are related to mobility (Hassink and Van Leuvensteijn, 2011) or refinancing, the goal of most related literature is predicting either mortgage pricing or default/termination (Titman and Torous, 1989, Kau et al., 1992, Kau et al., 1994, Stanton, 1995, Deng et al., 2000, etc.)[1].

[1] The determinants of the full prepayment in the literature are for example Interest rates (Charlier and Van Bussel, 2003), borrower credit score, LTV ratios (Green and Lacourt-Little, 1999), and age of contract (Charlier and Van Bussel, 2003).

There are several empirical studies investigating partial prepayments in response to financial incentives (interest rate changes for instance). In some studies interest rate changes are considered as positive income shocks. Di Maggio et al. (2017) find, using data on US (automatic) adjustable rate mortgages, that households first increase their consumption, and then voluntarily prepay their mortgage debt. They also show that highly leveraged households spend twice as much but deleverage less. Low income households also show the same pattern. More recently, Kuang et al. (2021) also study the same effect for Chinese mortgagors, where no partial prepayment penalty exists. They show that female, older, married, high-income, high education borrowers, and households with larger mortgage debt respond more to changes in the interest rate. Groot and Lejour (2018) study the Dutch case and notice that the difference between mortgage interest rates and interest rates on saving deposits, the deduction of mortgage interest payments and some specific tax exemptions on *inter vivos* explain most of the prepayments, but typically only for wealthy households.

There are more studies investigating other determinants of partial prepayments. Abrahams (1997) shows that borrower's liquidity, loan age, and available funds are associated with partial prepayments. Also, previous prepayments (Fu, 1997; Lin and Yang, 2005), or higher propensity to save and liquidity risk (Adelman et al., 2010) are important determinants of the decision to partially prepay. Behavioral explanations also exist. Amromin et al. (2007) study optimal portfolio allocation and show that borrowers prepay too much compared to how much they save for their retirement in accounts that are tax-deferred. McCollum et al. (2015) propose that households' attitudes towards debt should account for prepayments that cannot otherwise be explained by financial reasons (for instance arbitrage between mortgage interest rates and interest rate on saving deposits). House price developments might play a role too, because of their effect on the LTV. For the Netherlands, Van Beers et al. (2015) find that households with negative home equity deleverage mortgage debt when house prices decrease. Suari Andreu (2015) on the contrary finds no significant effect on savings. Other relevant institutional details are the presence of a prepayment penalty fined by banks (Groot and Lejour, 2018), the fiscal treatment of inheritances or gifts (Li and Mastrogiacomo, 2023), and retirement options (Amromin et al., 2007). It is on top of this that institutions such as mortgage insurance could play a significant role in voluntary deleveraging.

In the Netherlands a residual debt insurance (Nationale Hypotheek Garantie, NHG in Dutch) exists, that covers residual debt after selling one's home. Conditional on insurance, the bank will offer NHG participants their lowest available interest rates. The implied lower mortgage

premium, combined with prices fluctuations after the Great Recession, made the insurance an offer you cannot refuse (De Haan and Mastrogiacomo, 2020), with take-up rates around 90%, leaving little scope for adverse selection and the potential role of selection on unobservables (e.g. risk attitude).

Prepayments matter in several western countries that allow interest-only loans or longer-term mortgages (Scanlon et al., 2008; European Central Bank, 2009; Karpestam and Johansson, 2019;) as it is the only way to reduce debt. These concerns are crucial in the Netherlands, where most loans are interest-only. Prepayments then can be seen as a tool to reduce the financial stability risks related to interest-only loans, such as higher default risk (Brueckner et al., 2016), higher loss give default (Cunha et al., 2013), ex-ante adverse selection (Garmaise, 2013) also in terms of collateral risk (Edelberg, 2004), incidence in regions with single recourse (Amromin et al., 2018), interest rate risk (Scanlon et al.,2008; Mastrogiacomo, 2019) and house price shocks (Scanlon et al.,2008).

In this study, we investigate prepayments as means to voluntarily reduce mortgage debt, and how these differ for borrowers with or without residual mortgage debt insurance in the presence of an (unexpected) intergenerational transfer. Insurance qualification threshold are exogenously set, and unexpected transfers qualify as an instrument that addresses the possible endogeneity between housing wealth and prepayments as saving decisions. To preview our results, we find that insured mortgagors tend to prepay 22.8% less than the non-insured group, and even do so when they receive (unexpected) inheritances/gifts. When we look at this insurance effect among inheritors, the relative decrease becomes larger (25.4%). This difference still stands after controlling for all confounding factors mentioned above. More in detail, borrowers without the NHG insurance prepay 12.8 % of their original loan balance while those with the insurance prepay 3 percentage points less than that. This is compatible with the presence of moral hazard among the insured group. This might be one of the significant factors that explains the (lack of) deleveraging of less wealthy mortgagors in the Netherlands, and to the best of our knowledge, our study is the first to consider this.

Di Maggio et al. (2017), Groot and Lejour (2018), and Kuang et al. (2021) find a much smaller incidence, if any, of prepayments among borrowers in relatively poorer households, but do not link this to the difference in risk-bearing between insured (with lower income/wealth) and uninsured (typically richer). This is in line with the literature on the marginal propensity to consume of low income/ low housing wealth households in the presence of positive income

shocks (Zeldes 1989, Carroll and Kimball 1996, and Carroll 1997). While Groot and Lejour (2018) find no significant effect of prepayments in reducing debt for poorer households, we add an explanation for their finding. Moral hazard among insurance participants (who are housing poorer) could induce one to prepay less as the insurance moves the risk of residual debt on the insurer.

Our investigation of the role of insurance relies a.o. on the unexpected nature of bequests as only unexpected income/wealth shocks should unlock behavioral responses. Elinder et al. (2018) show that poorer heirs in Sweden consume a larger part of their inheritance. Also in other studies, this finding is not different (e.g., Weil, 1994, Joulfaian, 2006, Zagorsky, 2013). Investments also respond to inheritances, such as housing equity (Martinello, 2014) and stock market participation (Andersen and Nielsen, 2011). Christelis et al. (2019) show that households in the Netherlands use about 15% of an unexpected positive income shock for debt reduction, in line with US evidence (Mian and Sufi, 2010; Dynan et al., 2012)[2]. Since we have access to administrative data, we will use age of death to proxy unexpected inheritances, where the bequeather 'unexpectedly' dies at ages younger than one's cohort average age of death. Mirowsky (1999) finds that subjective life expectancy is close to the actuarial mortality estimates for each age group.

We focus on the effect of the insurance on differential behavior between (un)insured borrowers. Whether insured prepay less than they would if uninsured (moral hazard), or whether they are those who would normally prepay less (adverse selection) is here a matter of secondary relevance. But ruling out adverse selection for the reasons mentioned above, we can ascribe differences in prepayments to the explanations above (financial incentives, fees, wealth etc...) and to moral hazard. This is a rare opportunity for a study on debt insurance. The literature has delivered mixed results on this issue. Park (2016) finds adverse selection problems within the US private mortgage insurance market, where loans had higher defaults compared to the uninsured ones, but no difference in defaults rates between FHA (Federal Housing Administration) mortgage insurance participants and the non-insured. In Canada and Hongkong, mortgages with insurance have lower arrears (Grace et al., 2015). Tam et al. (2010) instead find no association between defaults and the Mortgage Insurance Program in Hong Kong. These different results could depend on the interplay between adverse selection and

[2] Unexpected inheritance can be elicited from survey data (Brown et al., 2010; Suari Andreu 2018) or from the medical records on the cause of death (Andersen and Nielsen, 2011; Elinder et al., 2018).

moral hazard, so it is particularly valuable that we can separately focus on the last[3].
The rest of the paper is organized as follows. In Section 2, we discuss the most relevant institutions. Section 3 presents the data and descriptive analysis, while Section 4 the empirical results are shown. In the last section, the policy discussion is followed by summary and conclusions.

2. Institutional background

High LTV ratios and interest-only loans are a distinctive feature of the Dutch mortgage portfolio, which is therefore considered a high macro risk (ESRB, 2016). The maximum LTV ratio has been reduced to 100% in 2018, it was still 106% in 2013 and much higher before (NVB, 2014). A generous tax deduction on mortgage interest payments applies, but it was disincentivized since 2013 for non-amortizing loans, yet there is still a large legacy from the past, about 60% of all outstanding mortgage loans as of 2014 (Mastrogiacomo and Van der Molen, 2015). Lenders have a full recourse, including not only their assets (except pension wealth), but also their (future) income. Currently about 27% of all mortgages are covered by the NHG, with peaks of more than 90% in the new production during the Great Recession. Dutch mortgages are typically composed by multiple loans, two on average. This can be done at origination, in order to mix loan types with different characteristics. But also over time borrowers can buy additional loans when purchasing a more expensive home. At that point one might lose NHG coverage, if the price of the new home exceeds the insurance threshold.

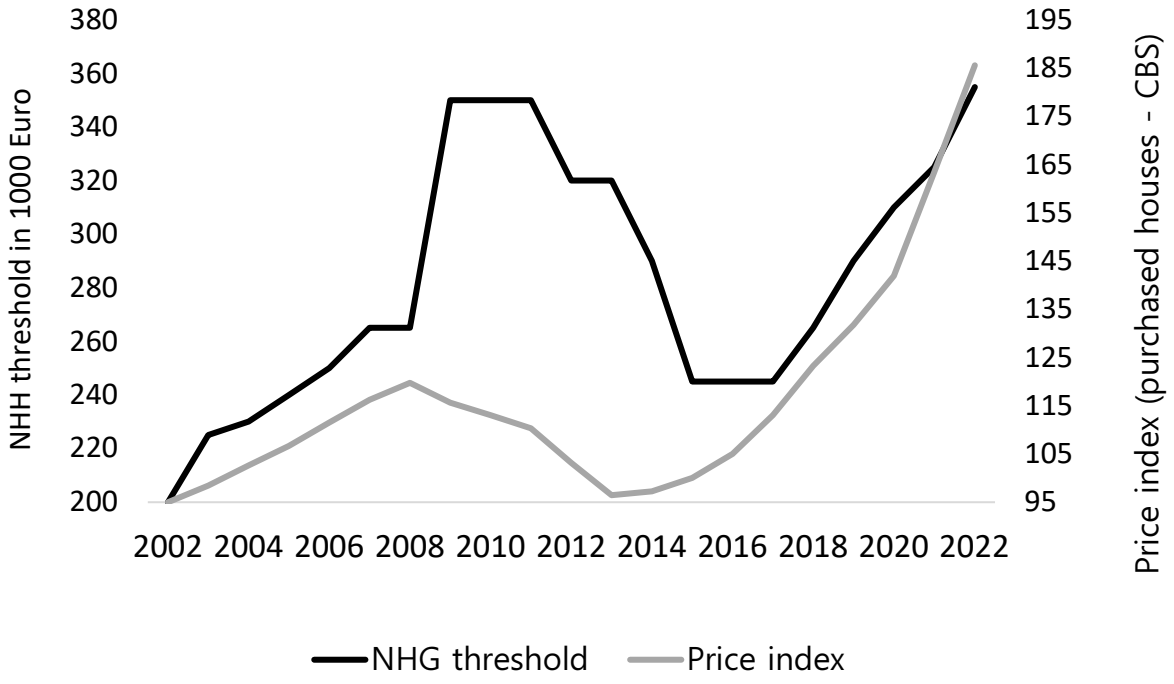
NHG has several specific features. The one-off entry fee is not correlated with risk (LTV) and there is lenders' copayment, contrary to insurances in most other countries that require periodic premiums and the borrower to copay in order to avoid moral hazard (Blood, 2009, and Clauretie and Herzog, 1990).

The insurance can be bought for properties with transaction price up to a ceiling, which is key to our identification strategy. This threshold moves yearly and it is set exogenously and cannot be affected by the borrowers. During the period of our analysis it was also independent of the

[3] To be more precise, these different results show that documenting differences between the insured and uninsured is not enough to identify a behavioral effect, unless one can make insurance participation exogenous to the choice of borrowers. We claim that this is the case of the Dutch NHG, where borrowers are assigned the option to buy depending on house prices, and they did not buy the NHG to receive insurance, but to receive the reduction in interest rate that comes with it, which is mostly relevant for the exogeneity.

development in house prices (see Figure 1). It offers a point of discontinuity that allows us to measure the effect of insurance participation on prepayments. During our observation period, in some years it followed house prices while in some years it did not. The figure shows that during the financial crisis, the threshold was first increased from 265k to 350k in 2009, to be then reduced to 245k in 2015.

Figure 1: NHG threshold and house price index in the Netherlands



Explanatory note: NHG thresholds and house price development in the Netherlands from 2002 to 2022. Source, Statistics Netherlands (CBS), NHG annual report (NHG), own computations.

3. Data and descriptive statistics

3.1 Data

The Dutch central bank (DNB) has requested mortgage lenders in the Netherlands who engage in securitization to provide details about their mortgage portfolios using loan-level data (LLD). This encompasses nearly all mortgages held by banks (approximately 95%) in the country, as well as those held by a few prominent insurers. It includes both loans and borrower characteristics with a quarterly frequency since 2012 (See Mastrogiacomo and Van der Molen (2015) for more information).

We merge the 2016 LLD with the income and wealth data of individuals/households provided by Statistics Netherlands (CBS) for the year 2016[4]. To include positive wealth shocks, we add several transfer-related data on inheritances and gifts in the period 2007-2016 also provided by Statistics Netherlands.

Our sample contains 60% of all households with a mortgage[5]. The absent 40% comprises around 800,000 loans, which are primarily associated with smaller insurers and pension funds, and are not held with banks. Additionally, there are roughly 500,000 loans held by banks that cannot be integrated into other administrative records due to the lack of information concerning the borrowers' birth year, a key variable for the secure identification of the households associated with these loans. Nonetheless, our analysis confirms that borrowers linked with these banks do not exhibit any discernible differences in terms of risk compared to those with other banks.

We construct two proxies of prepayments relative to original debt, namely cumulative prepayments and per-period prepayments. The first is a cumulative measure based on originating information for the last quarter of 2016, while the second is a per-period measure calculated by comparing current changes to debt with those in five consecutive waves of the LLD (2015q4 to 2016q4). The construction of the two proxies will be explained more in detail in the next paragraph. These additional requirements reduce our sample to 679,463 households with cumulative prepayments, and 672,701 households with per-period prepayments. For more details about our data selection process see the Appendix.

3.2 Descriptive analysis and summary statistics

We aggregate first all loans at borrower level and look at their present debt outstanding, relative to the original debt position. A cumulative prepayment of mortgage debt is defined as

$$PP1_{i,2016} = Original\ debt_{i,\tau} - Current\ debt_{i,2016} - Contractual\ repayments_{i,2016}$$

where τ is the origination date, and i stands for the borrower (or the mortgage, which are equivalent here). As we are interested in prepayments, we only look at the situation with

[4] This is for the moment the only year for which CBS provides a merging table that allows linking the income of individuals (INPATAB)/households (INHATAB) and the wealth of households (VEHTAB) to the LLD.

[5] Combining all datasets mentioned above and keeping one household head in each indebted household, we have an initial sample with the size of 1,970,762 households.

$Original\ debt_{i,\tau} - Current\ debt_{i,2016} > 0$ and discard all cases of additional mortgage take up. Current debt and original debt are observed as of the last quarter of 2016 in this specific definition. Contractual repayments are calculated as the cumulative sum of all scheduled repayments from debt origination till 2016. Initially, this calculation is performed on an at loan level, considering that the contractual amortization varies based on the type of loan (annuity, linear, interest-only, or saving/insurance/investment loans), to be then aggregated at borrower level too. Evidently, in this definition, we do not know in which quarter the prepayment has taken place but only its' cumulative value by the end of 2016. In this definition, we exploit the retrospective nature of the data, so only one cross section is enough to carry out this computation.

The first indicator we are interested in is thus cumulative prepayments relative to original debt, that is the prepaid share:

$$PPS1_{i,2016} = \frac{PP1_{i,2016}}{Original\ debt_{i,\tau}}$$

capturing the percentage of original principal being already prepaid. As PP1 and PPS1 are accumulated prepayments, the analysis based on those measures compares borrowers with different origination dates at the same point in time (2016q4 here). Notice that the term PPS1 is different from the per-period maximum prepayments share (most often about 10% in the Netherlands) allowed by banks without levying any interest penalty. That maximum is set annually on the basis of original debt or current debt, but our measure above instead is a cumulative one and relative to original debt.

As the LLD is a panel data for continuing borrowers, we can also reconstruct the schedule of prepayments for all quarters in 2016, above the contractual repayments. This boils down to:

$$PP2_{i,2016} = \sum_{q=2016q1}^{2016q4} Current\ debt_{i,q-1} - Current\ debt_{i,q} - Contractual\ repayments_{i,q-1}$$

With this variable, we can obtain the prepaid share relative to original debt for the year 2016:

$$PPS2_{i,2016} = \frac{PP2_{i,2016}}{Original\ debt_{i,\tau}}$$

As the most comprehensive measure of prepayments are $PP1$ and $PPS1$, in this section we show descriptive evidence based on these indicators. $PP2$ (and $PPS2$) instead will be used again when we perform robustness checks in Section 4.2 and when we carry out a descriptive

study of penalty fees on extra repayment by banks in Section 5. So, our data provides a more precise definition of prepayments relative to the proxy for prepayments in Groot and Lejour (2018) because, by observing the loan type and its' characteristics, we can isolate contractual repayments and solely attribute to prepayments any further reduction in outstanding debt over time.

We link *PPS1* directly to intergenerational transfers. We use inheritances and gifts data from 2007 to 2016, to distill an indicator that shows whether or not a household received a transfer during this period[6]. When we refer to a 'transfer', we mean the sum of inheritances and gifts. We consider three cases, where transfers include either inheritances or gifts, or both of them. As transfers could be expected and thus not qualify as exogenous wealth shocks, we address this endogeneity problem by proxying for unexpected inheritances using deaths at an early age, as discussed above. So, when one receives a transfer from a wo(man) who died before the age of 70 (65), we consider this death as unexpected. Figures 2.1 and 2.2 show the prepaid share by age of the household head for households in which we observe at least one debtor/receiver.

We plot 2 different graphs, depending on different definitions of (unexpected) inheritances/gifts/transfers. In all graphs, older heads accumulated larger prepayments. This could either depend on the fact that the cumulative process takes time, but also on a differential need/willingness across cohorts to make prepayments. For example, before 2013, most mortgages originated with interest-only loans, while afterwards younger buyers were pushed with fiscal incentives towards amortizing loans, which might justify higher prepayments in the older age group as well.

The figure also shows that, in all definitions, prepayments are significantly larger for those who receive any sort of wealth shock. The effect is largest for gits and for unexpected inheritances. This could indicate the presence of either a wealth effect or a cohort effect, suggesting that individuals passing away at a younger age might also have relatively younger (potentially more indebted) inheritors.

[6] We keep all borrowers in our baseline estimation but we will perform a robustness check excluding borrowers originating their loans before 2007 in order to align the time periods in both datasets.

Figure 2.1:
Prepaid share by age: with or without inheritance/ unexpected inheritance

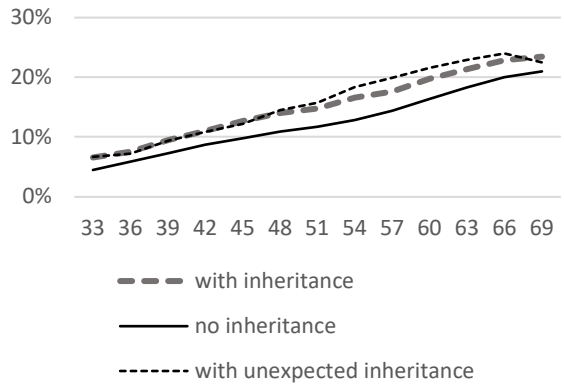
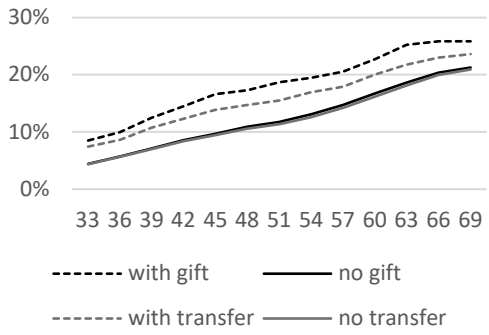


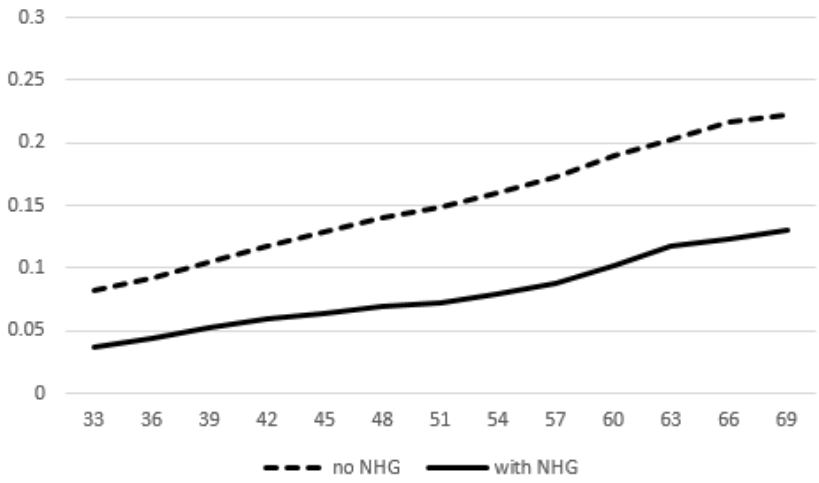
Figure 2.2
Prepaid share by age: with and without gift or transfers (transfer= inheritance + gift)



Explanatory note: Source: LLD (DNB), and income and wealth data of individuals & households (CBS).

Figure 3 shows a distinct difference in prepayments relative to debt between those with residual debt insurance and without. Borrowers covered by NHG insurance tend to make a comparatively smaller portion of debt prepayment. This observation could suggest that the group under insurance pays off a lesser amount not necessarily due to their insurance coverage, but potentially due to having lower wealth or inheriting fewer assets compared to the other group. Alternatively, it s plausible that this group selects more affordable homes initially because they anticipate receiving fewer financial transfers from their parents in the future.

Figure 3: Prepaid share by age with or without NHG



Explanatory note: Source: LLD (DNB), and CBS microdata, own computations.

Formulated differently: the choice for NHG and (expected) inheritances could be endogenous. One concern in the analysis above is the fact that the probability to receive intergenerational transfers is different across the treated and control groups, even prior to their home purchase; this disparity could affect the participation of receivers in NHG. For instance, wealthy parents might be more likely to assist their children in purchasing more costly homes (thus without NHG) while also bestowing larger inheritances. In order to look at this potential selection problem, we show how the probability to receive inheritances/gifts changes across two groups identified by their (ex-ante) propensity score to participate in NHG (rather than by the observed and potentially endogenous choice) both before and after purchasing a house. When we do this, we look at the probability among borrowers with similar propensity scores to buy NHG. The scores are projected based on the parental financial status, taking into account factors such as housing wealth and disposable income.

Figure 4.1 focuses on first-time buyers, while Figure 4.2 focuses on all homeowners. These figures indicate a greater likelihood of inheritances occurring for non-NHG borrowers after their home purchase (Figure 4.2) compared to before (Figure 4.1), especially as the recipients age. Since the majority of first-time buyers are under 40, Figure 4.1 suggests a low inheritance probability prior to home purchases, which does not vary significantly based on NHG status. While there could be instances where children acquire expensive homes (hence without NHG) with the expectation of potential future inheritances, the examination of propensity scores indicates that this problem is of limited empirical concern.

Instead, Figure 4.3 and Figure 4.4 display the probability of receiving gifts before and after home purchases. Once again, non-NHG borrowers more frequently receive gifts after buying a home (Figure 4.4) as opposed to before (Figure 4.3). However, this time, a noticeable divergence in gift receipt also emerges for younger first-time buyers (Figure 4.3). To address this potential endogeneity – the likelihood of insurance being lower for children of affluent parents – we have compared in these figures groups with similar propensity scores (determined by parental wealth) for NHG participation. This motivates some of our robustness checks later on (for example replacing the NHG indicator with an indicator for wealthy parents).

Figure 4.1:

Probability to receive inheritances of starters (before their purchase) among similar propensity scores to have or not to have NHG

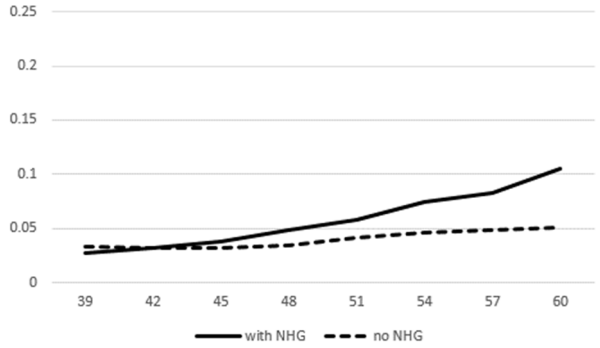


Figure 4.2:

Probability to receive inheritances of homeowners among similar propensity scores to have or not to have NHG

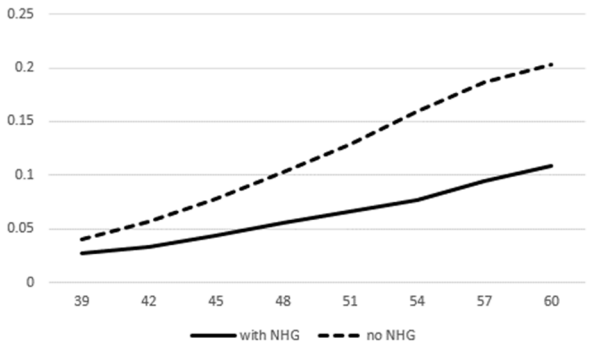


Figure 4.3:

Probability to receive gifts of starters before (their purchase) among similar propensity scores to have or not to have NHG

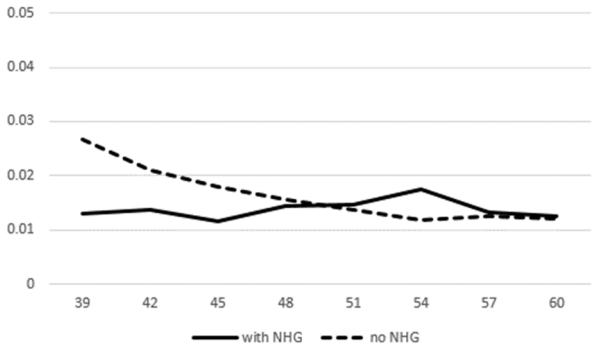
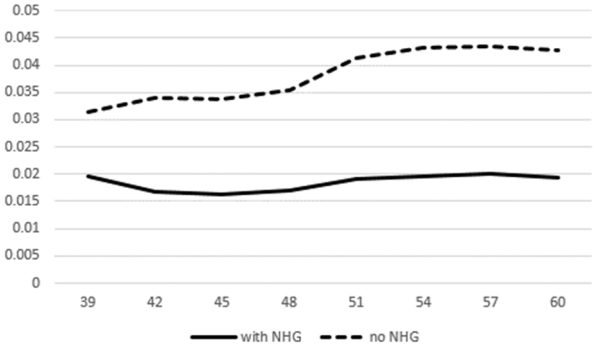


Figure 4.4:

Probability to receive gifts of homeowners among similar propensity scores to have or not to have NHG



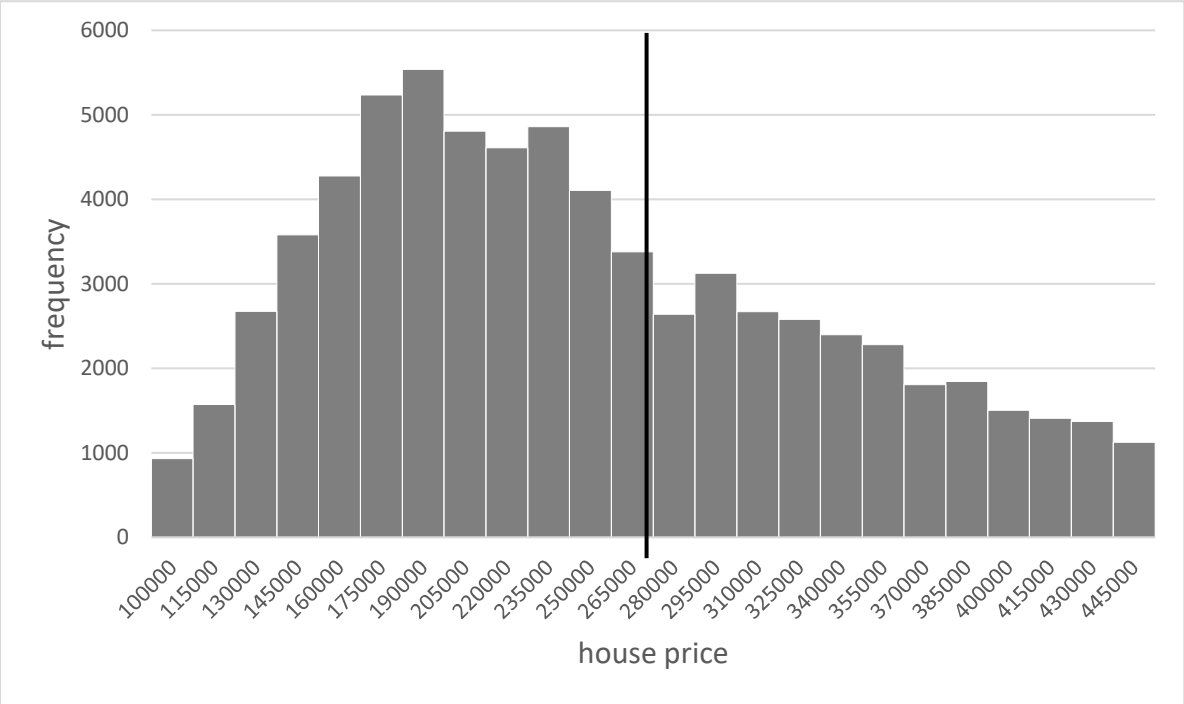
Explanatory note: All figures are based on the mortgage owners in 2016. Among those, in the left figure, we define "starters" as new owners that bought a house in t (all years) and look at their received transfers before that (thus currently tenants at the time of receiving transfers). In the right figures we look at homeowners in general, and the transfers they receive after having purchased their house. In all figures we only look at the mortgagors with similar propensity scores to have NHG, which is between 0.4 and 0.6, where most propensity scores overlap among two groups (with or without NHG). Source: LLD (DNB), and income and wealth data of individuals & households (CBS), own computations

As similarity between NHG and non-NHG participants is key to our identification strategy, we have additionally used propensity score matching by matching various borrower/ loan characteristics including mortgage duration. We emphasize this, because PPS1 is a cumulative variable, so it inherently relies on mortgage duration. Therefore, we use it in our analysis of propensity scores for NHG participation (also including duration-related characteristics such as age, disposable income, and financial assets) alongside the existing factors – parental

financial situation – that could all influence insurance decisions[7]. For sake of brevity, we do not show here the full results of the PSM analysis, which confirms that the matching achieved is well-balanced and effectively yields an effect of 6 percentage points less prepaid share by NHG participants, in line with the evidence above.

Informative to this discussion is a test of continuity of the assignment variable around the cutoff. Figure 5 shows the distribution of house prices around the NHG limit (vertical line). Here we only look at the houses within a price window of ± 180k of the NHG threshold. More houses are concentrated on the left of this distribution, but further from the vertical line, while near the threshold there is no discontinuity, bouncing or jumps.

Figure 5: Distribution of house prices and NHG threshold



Explanatory note: House prices at purchase within a window of ± 180k of the NHG threshold. Source, LLD (DNB), own computations.

[7] Upon conducting statistical tests (details available upon request from the authors), we find that incorporating mortgage duration (classified into five groups) helps mitigate standardized bias between the NHG and non-NHG groups. Specifically, the difference in duration between these two groups no longer significantly contributes to their imbalance. However, parental housing wealth still plays a significant role in the matching imbalance between the groups. However, the level of this disparity is generally negligible (usually below 0.5%), which is well within the acceptable margin of 5%.

We combine insurance and transfers indicators in Figures 6.1-6.4, where we see that the borrowers with insurance make lower prepayments relative to their debt at any age in the presence of each type of transfer, particularly so for those unexpected.

Figure 6.1:

Prepaid share with inheritance by age

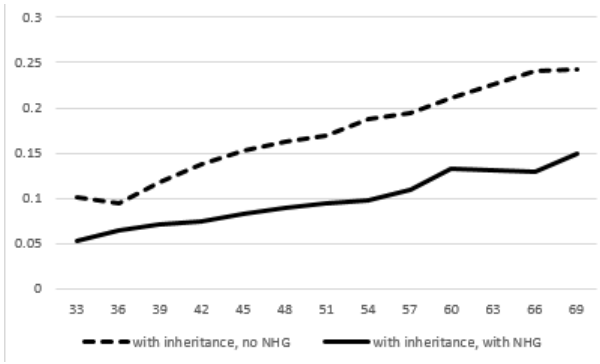


Figure 6.2:

Prepaid share with unexpected inheritance by age

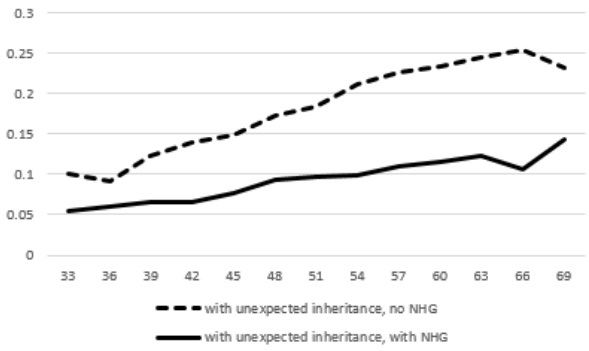


Figure 6.3:

Prepaid share with gift by age

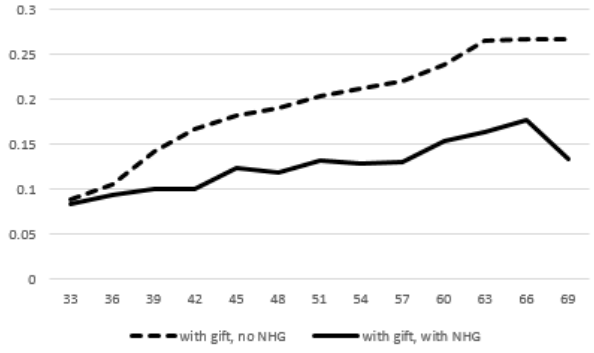
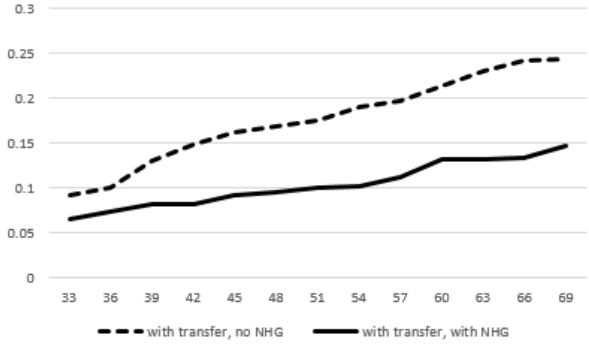


Figure 6.4:

Prepaid share with transfer (inheritance + gift) by age

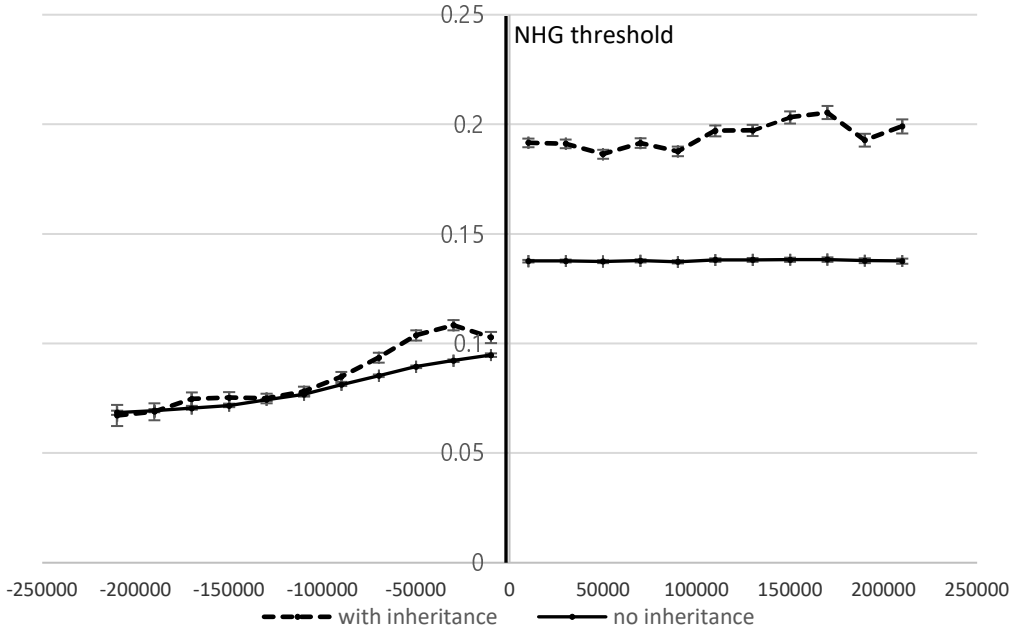


Explanatory note: Source: LLD (DNB), and income and wealth data of individuals & households (CBS), own computations.

Next we order prepayments by the distance from the NHG threshold (from 1991 to 2015), taking inheritances into account in Figure 7, looking at deleveraging around the threshold. Inheritances should not respond to the legislated threshold of NHG and prepayments should – in the absence of behavioral responses – not change around the threshold.

We see a discontinuity in the prepaid share between NHG holders (left) and non-insured borrowers (right). Prepayments are abruptly discontinued around the threshold. Additionally, we observe that only on the right of the point of discontinuity borrowers are systematically affected by having received an inheritance. Borrowers without insurance tend to make comparatively larger prepayments, particularly when they receive inheritances. As discussed above, several confounding factors could be responsible for this. This motivates us to estimate a regression discontinuity model later on, and therefore also present supplementary tests to validate the soundness of the underlying assumptions.

Figure 7: Prepaid share with and without inheritance and NHG threshold



Explanatory note: different prepayments around the NHG threshold (-220,000 to 220,000 euro). In this figure we pool all years between 1991 and 2015, accounting for the different NHG thresholds in each year. Source, LLD (DNB), income and wealth data of individuals & households (CBS), own computations.

Finally, in Table 1, we show descriptive statistics for our sample. As seen in Figure 2.1 and Figure 3, $PPS1$ is on average lower for NHG participants and non-inheritors.

The majority of mean observed characteristics, regardless of NHG participation, exhibit similarities, although a few exceptions exist. Financial assets of the household, age of household head, the share of interest-only (IO) loans in the mortgage, and mortgage duration are all larger at the right-hand side of the threshold, and must be controlled for in the empirical

model later on. As these variables are related to borrowers having different elapsed durations and experienced different life-cycle events in the past, we will later on add mortgage duration and its higher order terms as a regressor explaining prepayments.

Table 1: Summary statistics of NHG (non) participants and (non-) inheritors in the estimation sample: means and standard deviations

Variables	NHG participants	NHG non-participants	Inheritors	Non-Inheritors
PPS1	0.06 (0.10)	0.16 (0.18)	0.17 (0.18)	0.11 (0.15)
Age of household head	41.06 (10.81)	54.18 (12.22)	54.24 (11.32)	47.57 (13.36)
Disposable income household ($\times 10^5$)	0.49 (0.20)	0.65 (0.36)	0.61 (0.35)	0.57 (0.30)
Financial Asset household ($\times 10^5$)	0.26 (0.42)	0.64 (0.91)	0.76 (1.02)	0.44 (0.71)
Non-Adult Child (0/1)	0.45 (0.50)	0.38 (0.48)	0.32 (0.47)	0.42 (0.49)
Adult Child (0/1)	0.10 (0.30)	0.19 (0.40)	0.20 (0.40)	0.14 (0.35)
Couple (0/1)	0.70 (0.46)	0.84 (0.36)	0.72 (0.45)	0.78 (0.41)
Number of household member	2.62 (1.30)	2.83 (1.26)	2.58 (1.28)	2.75 (1.28)
Mortgage duration	4.83 (3.52)	10.33 (6.35)	9.54 (6.27)	7.66 (5.87)
Share of IO mortgage	0.20 (0.40)	0.64 (0.48)	0.58 (0.49)	0.43 (0.49)
Share of linear/annuity mortgage	0.32 (0.47)	0.11 (0.31)	0.13 (0.33)	0.21 (0.41)
Share of saving/insurance/investment mortgage	0.46 (0.50)	0.24 (0.43)	0.28 (0.45)	0.34 (0.47)
Share of other mortgage	0.02 (0.13)	0.01 (0.12)	0.02 (0.12)	0.02 (0.12)
Interest rate revision date[8]: b/w 2019 till 2021	0.25 (0.43)	0.20 (0.40)	0.22 (0.41)	0.22 (0.42)
Interest rate revision date: after 2021	0.64 (0.48)	0.55 (0.50)	0.55 (0.50)	0.59 (0.49)
Live in four largest cities (0/1)	0.12 (0.32)	0.08 (0.27)	0.08 (0.28)	0.10 (0.30)
Interest rate difference from origination	4.23 (0.86)	4.75 (1.18)	4.71 (1.12)	4.49 (1.08)
Underwater at origination	0.51 (0.50)	0.16 (0.36)	0.19 (0.39)	0.33 (0.47)
Underwater at current year	0.17 (0.38)	0.04 (0.21)	0.05 (0.22)	0.11 (0.31)
GAP ($\times 10^5$)	-1.05 (0.58)	1.36 (1.17)	0.82 (1.52)	0.22 (1.51)
Parents' housing wealth household	1.8 (1.6)	1.8 (1.8)	2.2 (1.7)	1.8 (1.7)
Parents' disposable income household	0.4 (0.2)	0.3 (0.2)	0.3 (0.2)	0.3 (0.2)
Parents' age (household head)	69.2 (10.2)	79.2 (10.6)	82.1 (10.5)	73.0 (11.2)
Parents born in NL (0/1)	0.9 (0.3)	0.9 (0.2)	0.96 (0.2)	0.93 (0.3)
Number of Observations	293,501	362,658	72,503	583,656

Explanatory note: The number of observations presented here does not correspond to that in regressions including parental variables. This discrepancy arises because parental variables cannot be obtained for 40% of the observations. Standard deviations in parentheses, Source, LLD (DNB), and income and wealth data of individuals & households (CBS), own computations

[8] In case of the revision date from 2016 and 2019, both dummies of interest rate revision date are indicated as 0.

We also measure the relative difference between the house value at purchase and the NHG threshold by the variable GAP. By construction, this variable is negative for NHG participants and positive for the rest. The last four variables at the bottom are parents-related and do not differ by NHG status, except the age of parents. In order to test whether NHG insurance is associated with parental characteristics, we include them as additional controls later on in model (3) in Table 2. In Table 1, we also show background characteristics of the estimating sample for the group of inheritors and non-inheritors. Most borrower's characteristics are similar except financial assets, age, and mortgage duration; while the parents of inheritors are obviously somewhat older and wealthier.

4. Empirical results

4.1 Empirical test of moral hazard

We conduct a formal examination to determine if participation in NHG led to reduced prepayments among couples who were recipients of transfers. We will estimate the following regression discontinuity model:

$$PPS1_i = \beta_0 + \beta_1 NHG_i + \beta_2 Transfer_i +$$

$$\beta_3 (NHG_i * Transfer_i) +$$

$$\beta_4 GAP_i + \beta_5 GAP_i^2 + \beta_6' X_i + \varepsilon_i \quad (1)$$

Here i stands for borrower. NHG_i and $Transfer_i$ are dummies for NHG participation and for whether or not households receive transfers respectively. In Table 2, we have carried out all estimations using inheritances as the only element within transfers, but robustness checks will follow. As controls, we include in X_i mortgage and borrower characteristics. Among these, we also add parental variables, as this might be related to NHG participation. The variable GAP captures the distance from the NHG threshold.

Following this, we show three different estimation models in Table 2. The first one only captures the effect of inheritances, NHG participation and their interaction with the prepaid share along with the GAP variables. The second model is our baseline, which adds mortgage and borrower characteristics to model (1). For model (3), we additionally include parental controls to the baseline (2). While matching parental variables with the children, we lose about 40% of the sample (see the number of observations at the bottom of Table 2).

All models show that inheritors have higher prepaid shares relative to the non-inheritors. Furthermore, all models suggest that borrowers who have insurance have lower prepayment rates in comparison to individuals without insurance, as well as those without insurance or inheritance. In all our models, the estimated coefficients of the interaction term (β_3) is negative and highly significant**.** The marginal effect of the NHG indicator is about -0.03 in our baseline, which indicates that NHG participants tend to make prepayments that are 3 percentage points lower of their original debt relative to non-NHG participants. As borrowers tend to prepay about 11.5% of their original debt, this effect is sizable. Back of the envelope computations, using our baseline results, show that uninsured participants prepay 12.8% of debt while NHG participants only 9.9%, when making a prepayment around the threshold. This boils down to a relative decrease of the prepaid share of 22.8%, which is induced by NHG (that is (12.8%-9.9%)/12.8%). When we specifically look at inheritors, this drop is even larger: 25.4%. In other words, the insurance leads to a reduction in prepayments relative to the initial debts for households with insurance, particularly noticeable among inheritors. This observation aligns with explanations rooted in moral hazard. Furthermore, this is true even after controlling for parental wealth/income (model (3)).

A positive and significant coefficient of the GAP in model (1) suggests that borrowers with more expensive houses are more likely to make an extra prepayment, but the coefficient becomes very small in other models and near zero in our baseline. For NHG participants the GAP value is negative (by construction), meaning that their pre-paid share gets smaller as their house values are far away from the threshold. The estimation results of most controls (mortgage/borrower characteristics) are in line with our expectations. The disposable income and financial assets of the households are positively associated with the prepaid share. Compared to households without children, all those with adult child, non-adult child, and both adult and none-adult children show lower prepaid share.

Table 2: OLS estimation results of prepayments

Dependent variable: prepaid share ($PPS1$)	(1) basic	(2) baseline	(3) add parental controls
NHG (0/1) (β_1)	-0.0747***	-0.0285***	-0.0259***
Inheritance (0/1)	0.0351***	0.0167***	0.015***
NHG (0/1) × Inheritance (0/1) (β_3)	-0.0025**	-0.0079***	-0.0077***
GAP ($\times 10^5$)	0.0062***	0.0005***	0.0016***
NHG (0/1) × GAP ($\times 10^5$)	0.015***	0.0043***	0.0029***
Log of age		-0.806***	-0.7334***
(Log of age) squared		0.1152***	0.105***
Disposable income household ($\times 10^5$)		0.0045***	0.0033***
Financial Asset household ($\times 10^5$)		0.028***	0.028***
Non-Adult Child		-0.0038***	-0.0034***
Adult Child		-0.0140***	-0.0122***
Non-Adult Child × Adult Child		0.0034***	0.0008
Couple (0/1)		-0.0052***	-0.0058***
Number of household member		-0.0005*	-0.0004
Mortgage duration		0.0024***	0.0026***
Share of linear/annuity mortgage		-0.053***	-0.045***
Share of saving/insurance/investment mortgage		-0.030***	-0.022***
Share of other mortgage		-0.028***	-0.022***
Interest rate revision date (b/w 2019 till 2021)		-0.0063***	-0.004***
Interest rate revision date (after 2021)		-0.0195***	-0.013***
Live in four largest cities (0/1)		-0.0072***	-0.0063***
Interest rate difference from origination		-0.0025***	-0.0022***
Underwater at origination		-0.011***	-0.011***
Underwater at current year (2016)		-0.051***	-0.046***
Parental housing wealth household			0.0013***
Parental disposable income household			0.0023***
Parental age household head			0.0004***
Parents born in NL (0/1)			0.005***
Constant	0.1505***	1.54***	1.37***
Number of Observations	656,159	656,159	390,279
Number of Households	656,159	656,159	390,279

Explanatory note: Our sample is limited to those borrowers who held mortgage loans as of 2016. We also condition on loan origination after 1991, start date of the NHG program. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Mortgage duration[9] is positively related to the prepayment. Mortgage composition affects the prepaid share too. In comparison with the share of interest-only mortgage (reference case), a higher share of amortizing mortgages (linear and annuity) decreases the frequency of prepayments. The timing of the interest rate revision also plays a role. As the revision date approaches, there is a corresponding increase in prepayments. Lastly, we confirm that in the time of low interest rates on mortgages, mortgagors tend to initiate more prepayments (Kau et al., 1993).

In model (3), parental variables are also added along with other controls. Parental housing wealth and income are positively associated with the pre-paid share by their children, although with modest impact. The age of the parents and being born in the Netherlands are also positively related to children's prepayments. For the other controls, signs and magnitudes are the same as the baseline (2), with a few non-noteworthy exceptions. For a cleaner comparison, we also re-estimate model (2) based on the sample used in model (3). The results are very similar to those in model (3) and (2), also with similar and highly significant β_1(=-0.026) and β_3(=-0.0074). What is worth mentioning is that this additional estimation shows a coefficient that is similar to model (2) but smaller than model (3) for the inheritance dummy (0.166).

4.2 Robustness checks

As previously indicated, our intention here is to challenge the existing finding by subjecting it to a series of tests. We explore whether wealth shocks remain relevant even when considering diverse forms of transfers, encompassing unexpected inheritances, such as those arising from individuals leaving bequests at varying earlier ages. Also, we test that the control group, assigned to those not eligible for insurance, is not endogenously defined. To address this concern, we substitute the NHG indicator with a binary variable identifying individuals with rich parents.

We also test the hypothesis of a constant treatment effect by analyzing different subsets of borrowers. This entails examining borrowers who are more comparable based on their house

[9] As a sensitivity analysis, we replace the mortgage duration variable with log of mortgage duration and its square and perform the same regression. The results are very similar to our baseline (-0.026*** for β_1 and -0.0079*** for β_3). We have a positive coefficient for log of mortgage duration (0.043***) and a negative coefficient for its square term (-0.004***), both highly significant, though they do not change our main results.

prices or specific sample periods. In this analysis we exclude loans that originated before the receipt of intergenerational transfers or prior to the introduction of fiscal incentives for full amortization in 2013.

Lastly, we incorporate inheritance amounts (so no longer the dummy only) or a bank-related binary variable (aiming to encompass all bank-specific peculiarities, including varying maximum free-fee prepayment thresholds) into the baseline model. Through this addition, we aim to determine whether these factors induce any changes in the primary results. Table 3 shows these robustness checks, where we also use the alternative definition of prepayments ($PPS2$) discussed above.

Table 3: Robustness Checks

Coefficient of NHG (β_1)	$PPS1$			$PPS2$		
Coefficient of NHG × wealth shocks (β_3)	$\boldsymbol{\beta_1}$	$\boldsymbol{\beta_3}$	**N**	$\boldsymbol{\beta_1}$	$\boldsymbol{\beta_3}$	**N**
Baseline Estimation (NHG × Inheritance)	-0.0285***	-0.0079***	656,159	-0.0083***	-0.0046***	650,263
Panel A1 NHG × Unexpected Inheritance: (Fe)male giver died before 65(60)	-0.0283***	-0.018***	592,169	-0.0084***	-0.0078***	594,567
Panel A2 NHG × Unexpected Inheritance: (Fe)male giver died before 70(65)	-0.0283***	-0.013***	597,795	-0.0084***	-0.0052***	599,610
Panel A3 NHG × Unexpected Inheritance: (Fe)male giver died before 75(70)	-0.0283***	-0.009***	604,966	-0.0083***	-0.0036***	606,152
Panel B1 NHG × Transfer	-0.0285***	-0.0050***	656,159	-0.0082***	-0.0037***	650,263
Panel B2 NHG × Inheritance after buying houses	-0.0288***	-0.0048***	635,481	-0.0083***	-0.0029***	630,290
Panel B3 NHG × Gift	-0.0291***	-0.0004	656,159	-0.0086***	-0.0012	650,263
Panel C Rich parents dummy × Inheritance	0.0099***	0.0018	390,279	0.002***	-0.0002	416,261
Panel D: close to the threshold (difference b/w ȷ 200,000)	-0.0251***	-0.0097***	552,717	-0.0077***	-0.0047***	556,806
Panel E1: loan condition (loans that originates since 2007)	-0.0279***	-0.0076***	433,998	-0.0097***	-0.0054***	453,551
Panel E2: loan condition (loans that originates since 2013)	-0.0217***	-0.0051***	192,773	-0.0093***	-0.0042***	196,131
Panel F1: add amount of inheritances	-0.0286***	-0.0072***	656,159	-0.0083***	-0.0043***	650,263
Panel F2: add bank dummy	-0.0267***	-0.0085***	656,159	-0.0071***	-0.0046***	650,263

Explanatory note: β_1 is a coefficient for the effect of the NHG dummy while β_3 refers to the interaction between NHG and transfer dummy. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10

The estimated coefficients of NHG indicator (β_1) for both definitions of prepaid share ($PPS1$ and $PPS2$) are highly significant with very similar magnitudes as in our baseline (-0.0285 and -0.0083 respectively) in all specifications (A to F), except Panel C where we replace the NHG indicator with a dummy for rich parents. There we find a more than twenty times smaller effect. This is reassuring as it suggests that having rich parents and not meeting the criteria for insurance qualification are distinct facts. No matter how we modify our models, the effect of insurance in lowering prepayments stays the same.

We now look at the estimates of β_3. For $PPS1$, we see negative coefficients with high statistical significance for all different definitions of wealth transfers (from Panel A1 to B3) with exception of one, that is not significant. This is the case where we use inter vivos gifts instead of inheritances. Evidently with gifts it is more difficult to claim that they are unexpected. Most importantly, in Panel A we have three cases of unexpected inheritances depending on the age of death for the giver; whether a (fe)male person died before 65(60), 70(65), or 75(70). All those coefficients (β_3) for unexpected inheritances are significant, and somewhat larger than the baseline. Also in terms of the magnitude, the younger (more unexpected) the death, the stronger the effect (lower prepaid share of NHG participants relative to non-participants). This suggests again that after addressing some of the potential endogeneity problems surrounding inheritances, our baseline results still stand.

We also limited the inheritances to those that took place after house purchases (see Panel B2), the same group as in Figure 4.2 (but this time for mortgagors with all propensity scores). The results are still qualitatively similar to those in Tables 2 though the magnitude is somewhat smaller. In Panel C, we replace the NHG indicator with a dummy for wealthy parents. The dummy is equal to 1(0) if the parents of borrowers have more(less) than 500,000 euro of assets. Then the PPS1 model, β_3 is not statistically significant, which again suggests that there is no correspondence between not having NHG and having rich parents. We also see a similar β_3 when we restrict the distance from the threshold between $\pm$200 thousand euro (Panel D), and when we only consider the mortgage loans which originated since 2007 (Panel E1). So, in the latter, we dismiss loans that were originated before 2007 but received inheritances after 2007 and made prepayments between 2007 and 2016. In Panel E2, we again have negative and highly significant β_3, suggesting less concern that the effect might be driven by more amortizing loans for the insured borrowers compared to the non-insured. The magnitude is then somewhat smaller than the baseline (as it is measured with cumulated prepayments). Finally, even after adding an amount of inheritances or a bank dummy to our baseline, the

results stay similar. With regards to per-period prepaid share, $PPS2$, β_3 shows again always a negative and highly significant value (with the same exceptions as in the $PPS1$ models), meaning that the insurance reduces per-period prepayments too in all specifications. Notice that we use the unconditional prepaid share for $PPS2$, which means that $PPS2$ contains zero values too.

5. Policy Discussion

The mean per-period prepaid share (PPS2) is similar in magnitude to the penalty fee set by banks in the Netherlands. Among the three major Dutch banks, two permit prepayments of up to 10% of the original principal without incurring fees, while the third bank allows prepayments ranging from 10% to 20% of the original principal. Similar ceilings are also set by other banks. Figures 8.1 to 8.3 group borrowers by loan suppliers, when we focus on the three major banks, and show the distributions of PPS2.

We only present PPS2 ranging from 0% to 30%, as larger prepayment shares typically imply full mortgage repayments. For reference, we additionally plot the median of PPS2 from the full distribution with a vertical line for every figure. The upper panel (Figures 8.1 and 8.2) displays the first two banks that permit 10% prepayments, while the third bank, which allows 10-20% prepayments, is depicted in Figure 8.3. In order to evade these penalties, borrowers need to strategically navigate around the no-fine thresholds. This behavior is evident in the prominent spikes seen in the first two Figures (around 10%) as well as in the final Figure (at both 10% but mostly around 20%).

Up to this point, our focus has been on examining how the design of insurance impacts prepayment patterns. However, it s important to emphasize that NHG is not the sole institutional aspect in play here. Banks' penalty fees also hold significance in influencing borrower prepayments. It s noteworthy that this factor doesn't pose a concern for our results. This is because these penalty fees are applicable both below and above the NHG threshold, and all banks have comparable shares of NHG participants.

Figure 8.1:

Distribution of prepaid share per-period (PPS2): prepayment fees are not applied below 10% (Bank A)

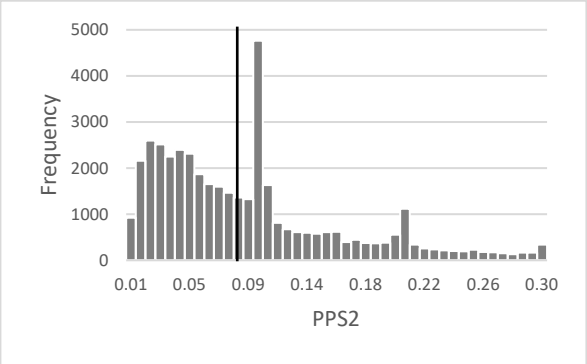


Figure 8.2:

Distribution of prepaid share per-period (PPS2): prepayments fees are not applied below 10% (Bank B)

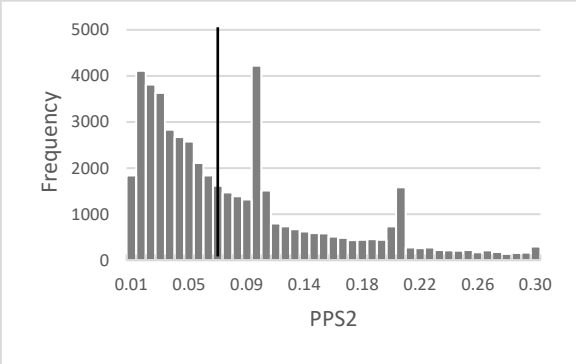
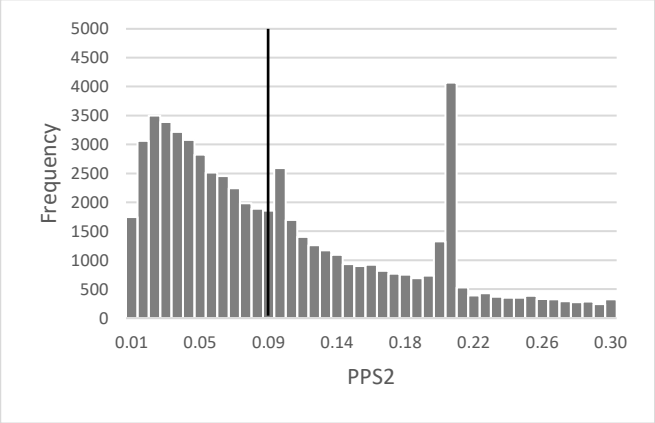


Figure 8.3:

Distribution of prepaid share per-period (PPS2): prepayments fees are not applied below 10-20% (Bank C)



Explanatory note: Source, LLD (DNB), and income and wealth data of individuals & households (CBS), own computations. Frequency distributions of prepaid share per-period (PPS2) by different suppliers. Vertical lines show the median of the full distribution, and they represent the median prepayment relative to the original principal.

6. Summary and Conclusions

Borrowers insured against residual mortgage debt are less likely to prepay it compared to those without the insurance. This is also true when there are (unexpected) inter vivos transfers to the borrowers, as additional exogenous shock. The establishment of a causal link between insurance and reduced prepayments relies on the utilization of a precise regression discontinuity design for identification. This is facilitated by the design of the Dutch residual debt insurance, exclusively accessible for properties valued below a specified legal threshold. This strategy is reinforced by considering the impact of unexpected inheritances. Our results are in line with moral hazard theory, as the design of the insurance gives little room to explanations based on adverse selection. Such selection could take place when a specific group could more easily buy houses above the threshold and end up in the uninsured group, for instance because they have affluent families. We show that this is not the case as we are able to link parental wealth to our analysis.

We find that the NHG insurance has induced a 3 percentage points lower share of prepaid debt, relative to original outstanding. As borrowers tend to repay about 11.5% of their original principal, this effect is sizable. Uninsured borrowers prepay 12.8% of debt while NHG participants only 9.9%, when they make a prepayment around the threshold. This boils down to a relative decrease of the prepaid share of 22.8% induced by NHG that increases to 25.4% when we look at inheritors only. This effect is subjected to the several robustness checks in which we use different definitions of unexpected inheritances. All of our robustness checks show a very high significant effect, very similar in magnitude to our baseline specification. The tests also show similar results when we use per-period prepaid shares (rather than accumulated prepaid shares as in our baseline). For our placebo test, on the contrary, we find no effect when we replace the NHG indicator with a dummy for wealthy parents, which confirms our main results.

Residual debt insurance induces moral hazard in the form of suppressed prepayments among the low income/wealth households who, according to literature, deleverage less (or consume more) in response to an income shock. This makes borrowers and lenders as a whole more vulnerable to systemic risks especially in the institutions where there is a large share of interest-only loans, as well as highly leveraged loans, such as those in the Netherlands.

Appendix

Sample Selection Table

Note for deletion	# of borrowers (at household level)	
	Deleted obs	Remaining obs
- Loan Level data (DNB) + income & wealth data of individuals / households (CBS) for year 2016		2,098,338
- no information on main variables in LLD (e.g. non-performing loans, NHG participation, etc.)	83,095	2,015,243
- no information on main variables in CBS data (e.g. household income, no homeownership, etc.)	43,549	1,971,694
- add transfer (gift or inheritance) data between 2007 and 2016	0	1,971,694
- only one household member within household (keep one household head)	932	1,970,762

Note for deletion	PPS1		PPS2	
	# of prepaid households		# of prepaid households	
	Deleted	Remaining	Deleted	Remaining
- no information on prepayments in terms of PP1 or PP2	165,600	1,805,162	858,218	1,112,544
- zero prepayment from origination to 2016 in terms of PP1	310,583	1,494,579		
- no mortgage debts in 2016 according to CBS data	28,499	1,466,080	22,444	1,090,100
- irrelevant data points for this study (e.g. origination year before 1991, etc.)	46,487	1,419,593	135,722	954,378
- tale value in LLD data (e.g. too small or large house values, etc.)	43,620	1,375,973	36,559	917,819
- tale (calculated) values in LLD data (e.g. prepayments, mortgage debts at household level, etc.)	452,751	923,222	31,268	886,551
- tale value in CBS data (e.g. disposable income, household financial assets, etc.)	5,676	917,546	5,386	881,165
- inconsistency within LLD data (e.g. qualification vs participation of NHG, etc.)	261,387	656,159	230,902	650,263
Final Sample		656,159		650,263

References

Abrahams, S. W., 1997. The new view in mortgage prepayments: Insight from analysis at the loan-by-loan level. *The Journal of Fixed Income*, 7(1), p.8.

Adelman, S., Cross, M. and Shrider, D., 2010. Why do homeowners make mortgage curtailment payments?. *Journal of Housing Research*, 19(2), pp.195-212.

Amromin, G., Huang, J. and Sialm, C., 2007. The tradeoff between mortgage prepayments and tax-deferred retirement savings. *Journal of Public Economics*, 91(10), pp.2014-2040.

Amromin, G., Huang, J., Sialm, C. and Zhong, E., 2018. Complex mortgages. *Review of Finance*, 22(6), pp.1975-2007.

Andersen, S. and Nielsen, K. M., 2011. Participation constraints in the stock market: Evidence from unexpected inheritance due to sudden death. *The Review of Financial Studies*, 24(5), pp.1667-1697.

Blood, R., 2009. Mortgage Insurance in Housing Finance Policy in Emerging Markets. In: L. Chiquier and M. Lea, eds., Housing Finance Policy in Emerging Markets. Washington: World Bank.

Brown, J. R., Coile, C. C. and Weisbenner, S. J., 2010. The effect of inheritance receipt on retirement. *The Review of Economics and Statistics*, 92(2), pp.425-434.

Brueckner, J. K., Calem, P. S. and Nakamura, L. I., 2016. House-price expectations, alternative mortgage products, and default. *Journal of Money, Credit and Banking*, 48(1), pp.81-112.

Carroll, C. D. and Kimball, M. S., 1996. On the Concavity of the Consumption Function. *Econometrica*, 64(4), pp.981-992.

Carroll, C. D., 1997. Buffer-Stock Saving and the Life Cycle/Permanent Income Hypothesis. *Quarterly Journal of Economics*, 112(1), pp.1-55.

Charlier, E. and Van Bussel, A., 2003. Prepayment behavior of Dutch mortgagors: an empirical analysis. *Real Estate Economics*, 31(2), pp.165-204.

Christelis, D., Georgarakos, D., Jappelli, T., Pistaferri, L. and Van Rooij, M., 2019. Asymmetric consumption effects of transitory income shocks. *The Economic Journal*, 129(622), pp.2322-2341.

Clauretie, T. M. and Herzog, T., 1990. The effect of state foreclosure laws on loan losses: Evidence from the mortgage insurance industry. *Journal of Money, Credit and Banking*, 22(2), pp.221-233.

Cunha, M. R., Lambrecht, B. M. and Pawlina, G., 2013. Determinants of outstanding mortgage loan to value ratios: evidence from the Netherlands. In: EFA 2009 Bergen Meetings Paper.

de Haan, L. and Mastrogiacomo, M., 2020. Loan to value caps and government-backed mortgage insurance: Loan-level evidence from Dutch residential mortgages. *De Economist*, 168(4), pp.453-473.

Deng, Y., Quigley, J. M. and Van Order, R., 2000. Mortgage terminations, heterogeneity and the exercise of mortgage options. *Econometrica*, 68(2), pp.275-307.

Di Maggio, M., Kermani, A., Keys, B. J., Piskorski, T., Ramcharan, R., Seru, A. and Yao, V., 2017. Interest rate pass-through: Mortgage rates, household consumption, and voluntary deleveraging. *American Economic Review*, 107(11), pp.3550-3588.

Dynan, K., Mian, A. and Pence, K. M., 2012. Is a household debt overhang holding back consumption?. Brookings Papers on Economic Activity, pp.299-362.

Edelberg, W., 2004. Testing for adverse selection and moral hazard in consumer loan markets. Available at SSRN 515903.

Elinder, M., Erixson, O. and Waldenström, D., 2018. Inheritance and wealth inequality: Evidence from population registers. *Journal of Public Economics*, 165, pp.17-30.

ESRB (European Systemic Risk Board), 2016. Vulnerabilities in the EU residential real estate sector.

European Central Bank, 2009. Housing Finance in the Euro Area. In: Occasional Paper Series.

Fu, Q., 1997. Retiring Early: An Empirical Analysis of the Mortgage Curtailment Decision. University of Wisconsin Ph.D. Dissertation.

Garmaise, M. J., 2013. The attractions and perils of flexible mortgage lending. *The Review of Financial Studies*, 26(10), pp.2548-2582.

Grace, T., Hallissey, N. and Woods, M., 2015. The Instruments of macro-prudential policy. *Quarterly Bulletin*, 1, pp.91-95.

Green, J. and Shoven, J. B., 1986. The Effects of Interest Rates on Mortgage Prepayments. *Journal of Money, Credit and Banking*, 18(1).

Green, R. K. and LaCour-Little, M., 1999. Some truths about ostriches: Who doesn't prepay their mortgages and why they don't. *Journal of Housing Economics*, 8(3), pp.233-248.

Groot, S. P. and Lejour, A. M., 2018. Financial incentives for mortgage prepayment behavior: Evidence from Dutch micro data. *Journal of Housing Economics*, 41, pp.237-250.

Hassink, W. and Van Leuvensteijn, M., 2011. The importance of income and housing wealth constraints for future residential mobility. *Housing Studies*, 26(04), pp.575-591.

Joulfaian, D., 2006. Inheritance and saving. Working Paper 12569, National Bureau of Economic Research.

Karpestam, P. and Johansson, S., 2019. Interest-only-mortgages and housing market fluctuations in Denmark. *Journal of Housing Economics*, 46, p.101627.

Kau, J. B., Keenan, D. C., Muller, W. J. and Epperson, J. F., 1992. A generalized valuation model for fixed-rate residential mortgages. *Journal of Money, Credit and Banking*, 24(3), pp.279-299.

Kau, J. B., Keenan, D. C. and Muller III, W. J., 1993. An option-based pricing model of private mortgage insurance. *Journal of Risk and Insurance*, pp.288-299.

Kau, J. B., Keenan, D. C. and Kim, T., 1994. Default probabilities for mortgages. *Journal of Urban Economics*, 35(3), pp.278-296.

Kuang, W., Liu, C., Wu, Q. and Zeng, H., 2021. How do interest rate changes affect mortgage curtailments? Evidence from China. *Real Estate Economics*, 49(S2), pp.395-427.

Li, Y. and Mastrogiacomo, M., 2023. Mortgage Prepayments and Tax-exempted intergenerational Transfers: From rich Parents to rich Children? *Review of Income and Wealth,* DOI: 10.1111/roiw.12644

Lin, C. C. and Yang, T. T., 2005. Curtailment as a mortgage performance indicator. *Journal of Housing Economics*, 14(3), pp.294-314.

Martinello, A., 2014. Inheritances and saving patterns; the long-run effects of large transitory shocks on

wealth accumulation. Mimeo.

Mastrogiacomo, M., 2016. Will we repay our debts before retirement? Or did we already, but nobody noticed? *Netspar Design Paper*, 64.

Mastrogiacomo, M. and Van der Molen, R., 2015. Dutch mortgages in the DNB loan level data (No. 1304). *Netherlands Central Bank*, Research Department.

McCollum, M. N., Lee, H. and Pace, R. K., 2015. Deleveraging and mortgage curtailment. *Journal of Banking & Finance*, 60, pp.60-75.

Mian, A. and Sufi, A., 2010. Household Leverage and the Recession of 2007–09. *IMF Economic Review*, 58(1), pp.74-117.

Mian, A., Rao, K. and Sufi, A., 2013. Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics*, 128(4), pp.1687-1726.

Mirowsky, J., 1999. Subjective life expectancy in the US: correspondence to actuarial estimates by age, sex and race. Social Science & Medicine, 49(7), pp.967-979.

NVB, 2014, August. The Dutch Mortgage Market. Dutch Banking Association. https://www.nvb.nl/publicaties/rapporten-verslagen-brochures/the-dutch-mortgage-market/

Park, K. A., 2016. FHA loan performance and adverse selection in mortgage insurance. *Journal of Housing Economics*, 34, pp.82-97.

Scanlon, K., Lunde, J. and Whitehead, C., 2008. Mortgage product innovation in advanced economies: more choice, more risk. *European Journal of Housing Policy*, 8(2), pp.109-131.

Stanton, R., 1995. Rational prepayment and the valuation of mortgage-backed securities. *The Review of Financial Studies*, 8(3), pp.677-708.

Suari Andreu, E., 2015. The effect of house price changes on household saving behaviour: a theoretical and empirical study of the Dutch case. SOM Research Reports; Vol. 15018-EEF. Groningen: University of Groningen, SOM research school.

Suari Andreu, E., 2018. Behavioural responses of older Europeans to inheritance receipt. Leiden University Department of Economics Research Memorandums.

Tam, M. W. Y., Hui, E. and Zheng, X., 2010. Residential mortgage default behaviour in Hong Kong. *Housing Studies*, 25(5), pp.647-669.

Titman, S. and Torous, W., 1989. Valuing commercial mortgages: An empirical investigation of the contingent-claims approach to pricing risky debt. *The Journal of Finance*, 44(2), pp.345-373.

van Beers, N., Bijlsma, M. and Mocking, R., 2015. House price shocks and household savings: Evidence from Dutch administrative data (No. 299. rdf). CPB Netherlands Bureau for Economic Policy Analysis.

Weil, D. N., 1994. The saving of the elderly in micro and macro data. *The Quarterly Journal of Economics*, 109(1), pp.55-81.

Zagorsky, J. L., 2013. Do people save or spend their inheritances? Understanding what happens to inherited wealth. *Journal of Family and Economic Issues*, 34(1), pp.64-76.

Zeldes, S. P., 1989. Optimal Consumption with Stochastic Income: Deviations from Certainty Equivalence. *Quarterly Journal of Economics*, 104(2), pp.275-298.

DeNederlandscheBank

EUROSYSTEEM

De Nederlandsche Bank N.V.
Postbus 98, 1000 AB Amsterdam
020 524 91 11
dnb.nl