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September 2022

Abstract

The Dutch government modified twice the taxation of intergenerational transfers aimed at mortgage down-payments and prepayments. We identify the causal effects of the tax exemption on prepayments and inter vivos transfers separately by exploiting changes in the policy design. Subsequent policy changes resulted in two expansions of the tax-free transfers that caused a significant increase in the probability of receiving such transfers – a relatively rare event – which translated then in a more modest increase in the probability to make prepayments, that are far more common. Initially the amounts prepaid increased by a similar magnitude, while the second expansion only increased the amounts being transferred but not the prepayments. The macroprudential policy goal of the reform was to reduce the number of underwater mortgages, at the time constituting more than one-third of all mortgages. We find that the prepayments triggered by the policy change increased mostly for borrowers with low original loan to value (LTV) ratios. This implies that most transfers were made from wealthy parents to housing-rich children. This because the policy was too generic, so it did not help to reduce the share of underwater mortgages.

Keywords: mortgage repayments, intergenerational transfers, household indebtedness
JEL classification: G5, H2

1. Introduction

The 2009-2013 asset-prices-crisis severely hit the Netherlands and left more than one-third of borrowers with an underwater mortgage.[1] Given the high share of interest-only (IO) mortgages, the Dutch government and De Nederlandsche Bank (the Dutch Central Bank, hereafter DNB) decided to employ a varied set of macroprudential tools. The tools include, for example, discouraging IO loans to new borrowers while allowing most of them to insure against residual debt,[2] adjusting risk weights for high LTV mortgages, and reducing LTI (loan to income) and LTV (loan to value) caps. Each of these measures is not unique for the Netherlands, and similar measures have been discussed in the international literature as well, but the combination of all of them is exclusively Dutch (OECD, 2020). The idea of the policy measures is to bolster financial stability and limit the negative externalities on consumption that are associated with significantly increased debt holding (Mian et al., 2008). Excessive indebtedness increases the homeowners' propensity to strategically default when the value of the mortgage exceeds the value of the house (Guiso et al., 2009). It also amplifies the procyclical expansion of leverage over the economic cycle, in combination with too optimistic expectations (Geanakoplos, 2010).

In this study, we consider a different and less common macroprudential tool.[3] We investigate the relaxation of inheritance tax law for intergenerational transfers[4] when these are used for either mortgage down-payments, prepayments,[5] or to finance a home-improvement.[6] With the new rules, the legislator aimed at preventing the underwriting of underwater mortgages (by stimulating down-payments) and at reducing the number of existing underwater mortgages (by

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[1] An underwater mortgage refers to a house purchased with a mortgage, which has a higher principal than the fair market value of the house.

[2] This is a government-back plan that transfers residual debt to a fund when a property with an underwater mortgage is being sold. Participation into the program is conditional on several conditions being met. The program was very popular with first-time buyers: participation in this group was about 80%.

[3] It is less common because normally macroprudential tools are established by NCBs (national central banks), and do not require a reform of either taxation or inheritance lows. This route must pass through a more complex political process, takes more time to be implemented and is therefore less common.

[4] Equal taxes apply to both inter vivos transfers and inheritances in order to avoid using the first to offset the second.

[5] Prepayment is the settlement of a mortgage installment before its official due date.

[6] Investing the money on home-improvements also increases the value of a house. Though debt is not reduced directly, the value of the asset typically increases or depreciates more slowly.

stimulating prepayments). We focus on the latter. We observe an increase in both mortgage prepayments and intergenerational transfers separately during the thirteen quarters in which the government relaxed these inheritance taxes in various ways.

Our research question is whether the tax exemption per se (hereafter, baseline policy), as well as its relaxation (hereafter, new policy), contributed to reducing existing indebtedness, and for whom. Thus, we discuss the distributional effects of these policies. Using a unique and custom-made panel data set, linking parents' wealth and inter vivos transfers, that contains almost the whole population of mortgage borrowers, we study whether the fiscal treatment of intergenerational transfers stimulated prepayments.

Related literature has revealed that intergenerational transfers can affect an individual's decision of taking up a mortgage in two ways. In the first, the intergenerational transfers from family members alleviate a down-payment constraint at home purchase. This literature interprets the positive correlation between an individual's homeownership and parental financial support as evidence of credit-market imperfections. Credit-market imperfections can also delay purchases in the form of down-payment requirements (Engelhardt and Mayer, 1998). Second, US data shows that intergenerational transfers lead to earlier purchases of more expensive homes with higher down-payments (Guiso et al., 2002; Luea, 2008), thus amplifying intergenerational inequality. Households that receive transfers may use them as substitutes for private savings. The institutional setting affects this process. European data show weak or no evidence that transfers from parents facilitate homeownership of children (Guiso and Jappelli (2002), Kolodziejczyk and Leth-Petersen (2013)). In the second pathway, the (lump-sum) intergenerational transfer may be annuitized, and then used for monthly scheduled mortgage repayments, or simply directly used for a lump-sum voluntary prepayment (thereby reducing the remaining mortgage repayments that borrowers face).

In our view three main concerns arise in related studies on intergenerational transfers. First, endogeneity arises when one thinks of intergenerational transfers as being correlated with several unobserved variables, for instance, the amount transferred could depend on debt aversion or credit worthiness, that are unobserved. Transfers can ease borrowing constraints (Cox, 1990), be determined by altruism (Mukherjee, 2020), or by a set of social rules (Cigno 1993), and this type of information is rare in both administrative and survey data. A second challenging factor in the literature on intergenerational transfers is data quality. Gale and Scholz (1994) notice for

instance lack of clarity in survey questions where the concepts of bequest and inter vivos are not properly defined nor is it clear whether one should adjust the value of transfers received in earlier years to reflect the present value of these transfers. Third, the effects being measured can be heterogenous across the population (Modigliani, 1988).

To address the first problem, the endogeneity issue, we identify the causal effect of the tax policies on intergenerational transfers, using a temporary change of their fiscal treatment. The change of policy was enacted twice for a period of thirteen quarters (five quarters starting from October 2013 and ending in December 2014, and eight quarters starting from January 2017 and ending in December 2018, when our data stopped being collected). The baseline policy was to allow parents to donate at most 52,000 euro tax free to their children up to age 35. During the first reform, the tax-exempted threshold increased to 100,000 euro, and again only usable once in a life-time. Anybody (not only parents) could become a donor, and the upper limit of the beneficiary's age restriction was dropped, thus enlarging the treatment group to the entire population. This means that someone who was older than 35 and did not qualify before the reform, suddenly qualified. This eliminates all control groups from the cross section in that period, as also orphans could in principle receive a tax-free transfer. With the second reform, starting in 2017q1, the only difference is that the upper limit of the beneficiary's age moved to 40. This means that, thanks to these discontinuities across groups, we can identify a treatment and control group, and thus a causal effect.

The second issue, data quality, is addressed using a new and unique combination of administrative data sources. Administrative data on intergenerational transfers, and how these are employed by the receiver, to the best of our knowledge, has not been widely investigated. These data mostly do not report why such transfers are being made or how the transfers are being used (Kotlikoff, 1988). One of the datasets we use is the DNB loan-level data (LLD[7]), which has very high coverage, frequency and granularity, and within the Netherlands, is the only dataset that makes our analysis possible. We observe about 80% of all existing mortgages in the Netherlands (about 95% of all mortgages offered by banks)[8]. The quarterly frequency of the

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[7] This dataset contains all loans originated by securitizing banks in the Netherlands, see Mastrogiacomo and van der Molen (2015)

[8] In the Netherlands, around 80% of mortgage is served by banks and is included in the dataset. The remaining 20% of mortgages is serviced by other financial institution such as pension funds, relative-small

dataset is higher than the annual data provided by the tax office, we can therefore precisely identify and follow the shift between the new and old policy period. The high granularity, where multiple loans per household are observed in all periods, allows us to capture features of indebtedness that cannot be studied otherwise, among which the type of loan (that signals the need for a prepayment, as many borrowers in the Netherlands have non-amortizing loans).[9] The panel nature of the data also allows the identification of the underwater state of the borrower and to observe how this evolves. Unique to this exercise is also that we link this data to tax records on parents' wealth, that are reported yearly. Additionally, we also perform the analysis using the tax records on transfers. This is micro data in which the tax office registers the purpose of the transfer (the tax office exempts one from inheritance taxes on transfers only for specific reasons, such as paying for a study), as it is mentioned whether the transfer was used to pay off mortgage debt.

Finally, we address the distributional concerns by looking at the home equity of the receiver and investigate whether policy-induced prepayments were mostly made by those with an underwater mortgage. In this case, the policy would favor intergenerational redistribution among home-owners.

The contribution of this study is on multiple domains. We add to the limited amount of research conducted on the effects of macroprudential tools at micro-level (see Caloia, 2020). In doing so, we use a neat research design that allows the identification of the causal effect of taxation on indebtedness, therefore isolating this effect from all other endogeneities. We show this effect on solvency risk, focusing on underwater mortgages, and show how intergenerational transfers could contribute to inequality. We also add to the literature that shows that prepayments are affected by observables (Green and Shoven, 2008; Krainer and Laderman, 2011), enlarging the set of covariates.

Our main results show that the first and second introduction of the new policy resulted in, respectively, a 14% and 7% increase in the probability of making a prepayment, and respectively,

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banks and insurance companies that do not securitize, as well as foreign institutions, and are not included in our data.

[9] Interest-only loans were a main financial innovation that started in the 1990's. These loans became very popular due to some combined fiscal advantages linked to the mortgage interest deduction. About 60% of all Dutch loans were interest-only at the onset of the new policy described here.

a 75% and 30% increase in the probability of receiving a transfer, with the latter being a less frequent event. We also find a 14% increase in the amount prepaid in the first new policy period, and an increase of 12% and 44% in the amounts being transferred after the first and second introduction, respectively. However, we find heterogeneous effects of the new policy across the population of mortgage owners. We found a larger effect for borrowers with relatively low original LTV ratios. This is typically the case when home equity appreciates over time, and not because of previous transfers. The policy is cumulative over time (transfers in the past exclude transfers at present). This suggests that intergenerational transfers were made from financially wealthy households to housing-rich children. This, in turn, implies the main message of our study: while the new policy was effective in increasing prepayments, it was not targeted enough to reducing the share of underwater mortgages. The other section of the regulation, centered around down-payments, is not being investigated here due to a lack of data.[10]

The paper is organized as follows. In the next section, we describe Dutch household debt and describe changes to inheritance taxes on intergenerational transfers. In Section 3, we describe the data collected by DNB, how this is merged with other administrative records and present some descriptive statistics. In Section 4, we discuss the identification strategy and in Section 5 we present the main estimation-results. Section 6 offers conclusions.

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[10] This however has more likely contributed to reducing mortgages that originate underwater (at the time an option in the Netherlands). This is because starters on the housing market have by definition no pre-transfer housing wealth. This again signals that it is possible to target such rules to easily identifiable groups (new buyers in this case).

2. Background

2.1 Mortgage debt in the Netherlands

The lack of a down-payment constraint, a generous mortgage interest deduction (MID) and a high degree of financial innovation (which made non-amortizing loans possible) have made the Netherlands one of the leading countries in the world in terms of mortgage debt and high LTV ratios for first-time buyers.[11] In particular, in 2018, 55% of total mortgage debt was interest-only (IO), while the rest was either amortizing (20%) or with deferred amortization (25%). This debt was distributed across about 7 million loans belonging to about 3.5 million borrowers (on average two loans per borrower). Most IO loans were perpetuities, and borrowers often combined them with other types of loans. About 30% of all borrowers had exclusively IO-loans, they usually belong to the elderly (that often uses these loans as a mean to extract home equity). Approximately 50% of borrowers has a combination of IO loans with either annuity or saving loans, while the remaining 20% has no IO loans at all. As IO loans do not amortize, it is very relevant to investigate prepayments, since this is the sole source of debt reduction for these borrowers.

2.2 Relevant institutional changes

Between 2013 and 2018, the Dutch government introduced several measures to reduce the negative externalities of excessive indebtedness. One of these changes was to temporarily relax the tax exemption policy on intergenerational transfers, provided that it was used to make a mortgage prepayment, a down-payment, or finance a home-improvement. The evaluation made by the Dutch Court of Auditors (Algemene Rekenkamer, 2017) shows that the new policy has increased transfers for down-payments. This means that fewer borrowers have bought their houses with

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[11] Before the housing crisis (house prices started falling after the second quarter of 2008 until the fourth quarter of 2013), it was common to borrow up to 120% of the property value. The Dutch government has imposed progressively lower LTV caps starting from 2013 (when the LTV cap was fixed to 106%) down to 100% in 2018. Debt service-to-income caps were also tightened and the mortgage interest deduction was downsized for existing contracts and fully abolished for new interest-only loans (therefore stopping the production of such loans).

LTVs exceeding 100%. In this study, we focus on investigating whether there was also an effect on the prepayments of those who already owned a mortgage at the time of the policy change. Thus, our first focus is on prepayments, and eventually we also discuss transfers.

To make this policy possible, the government proposed a modification of the law administering inheritances. In order to avoid tax arbitrages, inheritances are taxed approximately in the same way as inter vivos gifts. So fiscal limits to intergenerational transfers apply. Before 2013q4, borrowers with age below 35, could receive 52,000 euro[12] as tax exempted gift, one off. At the onset of the credit crisis in 2009 in the Netherlands, which eventually peaked in 2013q1 after four years of house price reductions, this was the institutional situation.

Table 1 summarizes the relevant features of the policy changes described above in terms of the tax-free threshold, the sources of the transfer (parents for instance) and beneficiary's age in all relevant periods. In section 4, we further re-organize the information of Table 1 in order to facilitate the discussion of the identification strategy.

Mortgage-related transfers are the most common purpose-driven and tax-exempted intergenerational transfers. The rest refers to programs that apply for instance to those financing higher education, inheriting a family business or diverting funds from one tax-facilitated savings scheme to another. Communication was also part of the new policy, which is relevant for our research, as it is crucial that mortgage owners were informed about it and understood its' content. To make all participants acquainted with it, banks were requested to directly inform their customers with a variety of methods, like special news bulletins and a personal letter. Meanwhile, this topic was widely discussed in the media. There is also evidence that the number of users of the program and the amount being received increased during this period. Table A1 in Appendix A provides detailed information of these increases as summarized by the Dutch tax office for the Dutch Court of Auditors. It is thus reasonable to believe that the public was aware of the relaxation of the tax exemption policy when it was implemented.

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[12] Before the 1990s, should one receive more than the amount allowed (about 5,000 to 6,000 euro), inheritance taxes would apply to the excess amount. However, when in the 1990's household indebtedness increased (together with the rise of house prices), an additional 46,000 euro (one-off) lump-sum transfer was allowed.

Table 1: Overview of the relevant institutional changes in different periods

		Period 1 **Baseline policy1** before 2013q3	Period 2 **New policy 1** 2013q4-2014q4	Period 3 **Baseline policy 2** 2015q1-2016q4	Period 4 **New policy 2** 2017q1-2018q4
Maximum tax-free transfer	0-52k	✓	✓	✓	✓
	52k-100k		✓		✓
Maximum age of the receiver	18-35	✓	✓	✓	✓
	36-40		✓	✓	✓
	>40		✓		
Donor	parents	✓	✓	✓	✓
	anyone		✓		✓

Note: 2013q3 to 2018q4 are periods for which LLD is available. Transfers declared in Period 2 had to be transferred before 2016.

3. Data and descriptive statistics

3.1 The loan level data

We use the DNB loan-level data (LLD) from 2012q4 to 2018q4. The LLD is a quarterly administrative panel dataset, which is derived from the templates that the European Central Bank (ECB) requires for accepting securitized mortgages as collateral.[13] It collects information on six million loans and three million borrowers (a mortgage typically consists of multiple loans). The administrative nature implies that the dataset has low measurement error, and most banks source the preloading of household the tax-forms, a practice of the tax authority, using the same database used for the LLD. This means that the data source is yearly checked (and corrected) by households too, who approve or improve the pre-loaded information when posting their tax forms. Thanks to this comparable information of DNB and the tax-office, Statistics Netherlands could merge a large part of the LLD to tax records. The LLD contains about 75 variables related to the mortgage. For example, the mortgage provider and servicer, the loan types, interest rates, borrower's participation into the national mortgage guarantee (a residual debt insurance), origination and maturity and current property evaluation. Some information about the borrowers was also registered at origination, such as (household) income, type of employment, borrower's age and area code. Each record includes a unique loan and borrower identifier, which allows tracking debt over time if (and only if) the borrowers stay within the same bank. In our data, we have information on prepayments, as we can identify continuing costumers, but we are not able to identify first-time buyers and their down-payments. The LLD also lacks some relevant information. First, we can only observe the borrowers' original income at mortgage application, but not the current income at reporting date. Current household income, for instance, can only be extrapolated using statistical methods. Second, in the LLD one cannot observe the source of

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[13] To use a securitized mortgage as collateral, each lending institution must agree to the 100% transparency policy of the ECB and fill in a template meant to deliver data to the European Data Warehouse. While the ECB only requests information on the securitized mortgages, DNB also requests that mortgage lenders report the rest of their portfolio. Dutch pension funds, small banks and insurance companies that do not securitize and foreign institutions do not participate.

the money used for prepayments, think for instance of intergenerational transfers, personal household savings, or some exogenous sources (such as lottery winnings or unexpected inheritances). Third, in the LLD, we also cannot observe the financial situation of the parents of a borrower.

It is to overcome these three limitations, that we merge the LLD with transfers and inheritances files, parent's wealth information and income files from Statistics Netherlands (CBS). However also this merge has some limitations. We identify about three-quarters of LLD borrowers, yet an impressive number. Only fiscally relevant transfers, above 6000 euro, are tax relevant and thus available in the data, but many prepayments are lower. Finally, only parents that are residents in the Netherlands are in the family links register and the records of parents of older borrowers are often missing. So, in order to better ensure that the borrowers in our dataset have the possibility to receive observable intergenerational transfers, we restrict our sample to the borrower's age from 31 to 45. We show in Table A2 in Appendix B an illustration of the steps of the selection process that we apply to one of the waves of the LLD data for the different estimating samples. The table shows that in the sample used for the baseline results, we lose most observations when we apply the age selection.

3.2 CBS data

In order to perform a separate analysis on transfers and inheritances, we merged the yearly CBS income tax records with personal demographics information, transfers and inheritances files, household wealth, parental and parent's wealth information from CBS. Table A3 in Appendix B shows the steps of the selection process that we apply to the CBS data for the different estimating samples for transfers and inheritances analysis.

3.3 Define mortgage prepayment

We derive the prepayments based on a dynamic analysis of the LLD data. We compute the first-difference of the principal in each quarter. Not all reductions in principals should be considered as prepayments of course. Annuity loans for instance are contractually repaid each period for an

amount that increases over time. These contractual repayments must be excluded from the prepayments. A similar treatment is needed for saving and life-insurance loans, whose deposits are registered similarly in the data as the contractual amortization of annuities.

So, we elicit prepayments as an irregularly large drop in the principal of borrowers that are observed for at least 3 consecutive quarters. As it was bank practice not to allow prepayments smaller than 2,000 euro per event, we also impose this limit, so small prepayments, that could arise from minimal time differences in our data, are dropped. Appendix B provides more detailed information on how we derive the prepayments.

3.4 Descriptive statistics

In order to describe our data, we start with two indicators, the share of those who made a prepayment relative to the population of borrowers (the prepayment rate, our extensive margin) and the mean prepayment (conditional on making a prepayment, our intensive margin).

Figure 1: The prepayment rate and conditional amount of prepayments in the standard policy period and new policy period
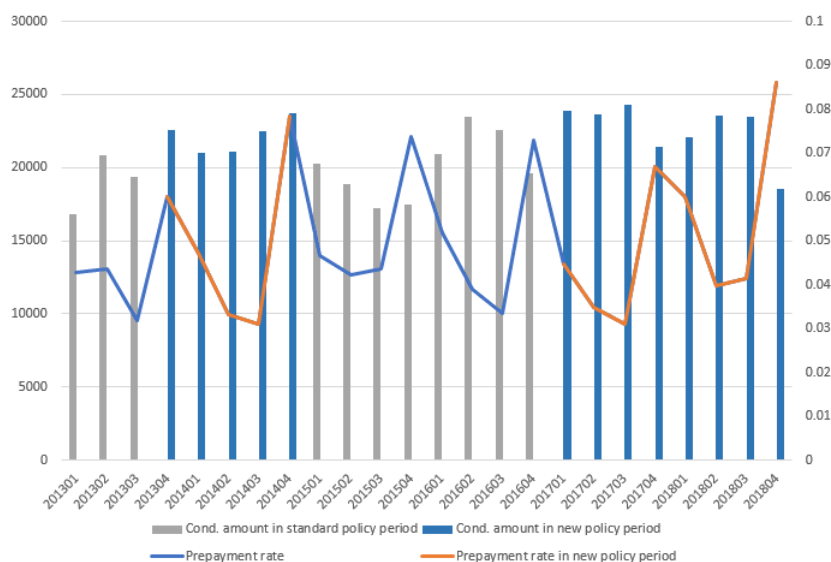
We notice (see Figure 1) that from 2013q4, with the first reform, prepayments are higher (left axis) and their rate peaks at about 8% (right axis) in 2014q4. After repealing the new policy

(2015q1), prepayments dropped to the pre-policy levels. This is remarkable, as the interest rates were lower in this period, which was found to positively affect prepayments (Li et al. 2016). When the policy was reintroduced in 2017, we observe again an increase in prepayments and a new peak in their rate in 2018q4. The 4th quarter is typically the most common for prepayments, possibly due to the end of each fiscal year, and the additional spikes in the new policy period suggest a positive effect of the policy. In Section 4 and 5, we aim to identify how the changes in policy, affected these trends.

Figure 2: Mortgage debt and prepayments by LTV category at borrower level in 2013q1
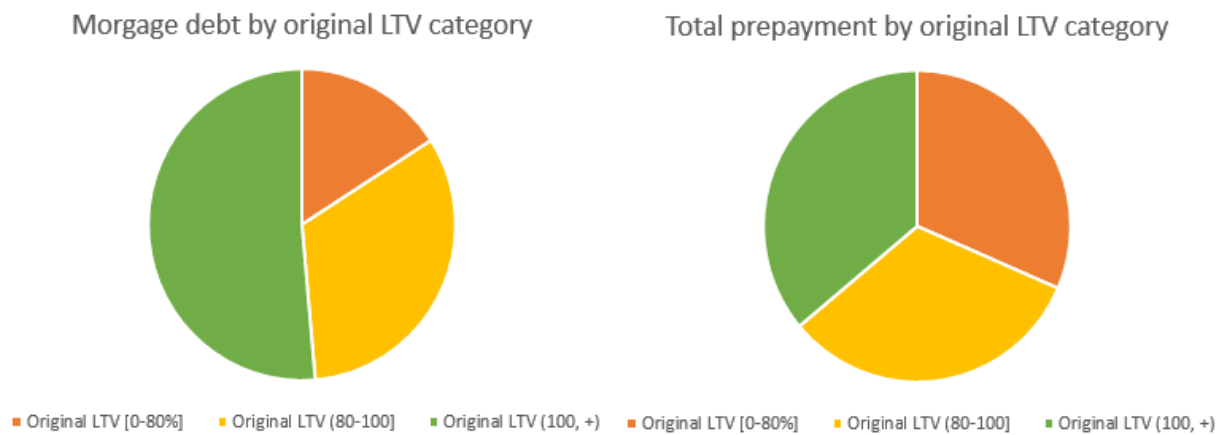
Explanatory note: the figure shows borrower level analysis. Left the distribution of original debt shows that most debt has LTV>100%, right prepayments are mostly observed on mortgages with LTV <100%.

The left panel of Figure 2 plots how mortgage debt (left) and total prepayments (right) were distributed by original LTV category in 2013q1. We see that while most debt was originated underwater, most prepayments were observed for borrowers with relatively low LTV, and relatively less so for those underwater (original LTV mortgage>100). Mortgages with low LTV (original LTV<= 80%) made up 16% of total debt but contributed 32% to total prepayments.

Underwater mortgages made up 51% of total debt but only contributed 36% to total prepayments. Section 5.3 investigates in detail the association between prepayments and original LTV levels[14].

In order to appreciate how relevant the reform was, we show in Table 2 descriptive statistics on transfers during the first reform period (2014) and in 2016, when the policy was approximately back to the baseline. The table shows a donor level data analysis. Evidently in 2016, the 5th exemption type (other donors) is not present, and the 3rd only moderately (one-off transfers *parents to children* could be done on paper in 2014 and completed within 2 years) while it was the most used in 2014 (67% of all donations). The median amounts do not differ much in any case.

Table 2: Fiscally relevant transfers, descriptive statistics, nominal amounts.

		2014			2016		
		Number of donations	Gross transfer		Number of donations	Gross transfer	
	Type of exemption	Share	Mean	Median	Share	Mean	Median
1	Regular exemption, parent to child	16%	63,659	45,000	59%	69,790	50,000
2	One-off higher exemption, parent to child	6%	25,855	25,000	13%	24,767	25,000
3	Additional one-off exemption, parent to child	67%	60,957	50,000	17%	44,575	50,000
4	Regular exemption from other donor	2%	34,640	13,149	9%	44,226	15,494
5	Additional one-off exemption, other donor	8%	49,245	40,000			
6	Exemption for business continuity plan	1%	816,958	467,899	2%	855,291	521,045
	N	156,617	(4.4% of mortgage holders)		54,727		

Note: Source: CBS, own computations.

In the first category (the regular baseline transfer), we observe transfers with a median of 50k and a somewhat higher mean. Despite this category potentially containing also transfers to pay for education, these are typically transfers received by 40plus children, on which the regular exemption of 52,000 euro applies. This means that the rest of the transfer is not tax-free. The same applies to the 4th category, but the exempted threshold is then lower. The last category

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[14] At present the share of original LTV ratios above 100% has dropped. As the new policy was enacted at the same time with a gradual reduction of the LTV cap (from 106% in 2013 to 105% in 2014) to be reduced to 100% in 2019, down payments became more popular, but not necessarily prepayments, that we study here. So here we do not need to worry on the possible causality of LTV policy on prepayments.

(business continuity) is only relevant for business owners, who can transfer a company of up to 1-million-euro net worth to their children. All other types can instead be used for mortgage prepayments (though the second category could also be used to pay a study, but this type of use is less common for individuals who already own a mortgage).

In Figure 3, we present the distribution of these transfers aimed at prepayments in CBS data and the prepayments in the LLD separately. We have divided both distributions in bins, highlighting some relevant institutional thresholds. The first bin for instance stops at 6000 euro. If prepayments below these thresholds were financed with a transfer, this would not show up in the transfer data (the threshold varies by year but is always between 5,000-6,000 euro). This is because amounts below 6,000 euro can be transferred to children every year, without being taxed; so many households do not even report these to the tax office (though officially one should). The information campaign for the reform could have stimulated small transfers too. We also have bins up to 52,000 euro and 100,000 euro, as these are the policy thresholds discussed above.

Figure 3: Distribution of transfers and prepayments around policy relevant thresholds.
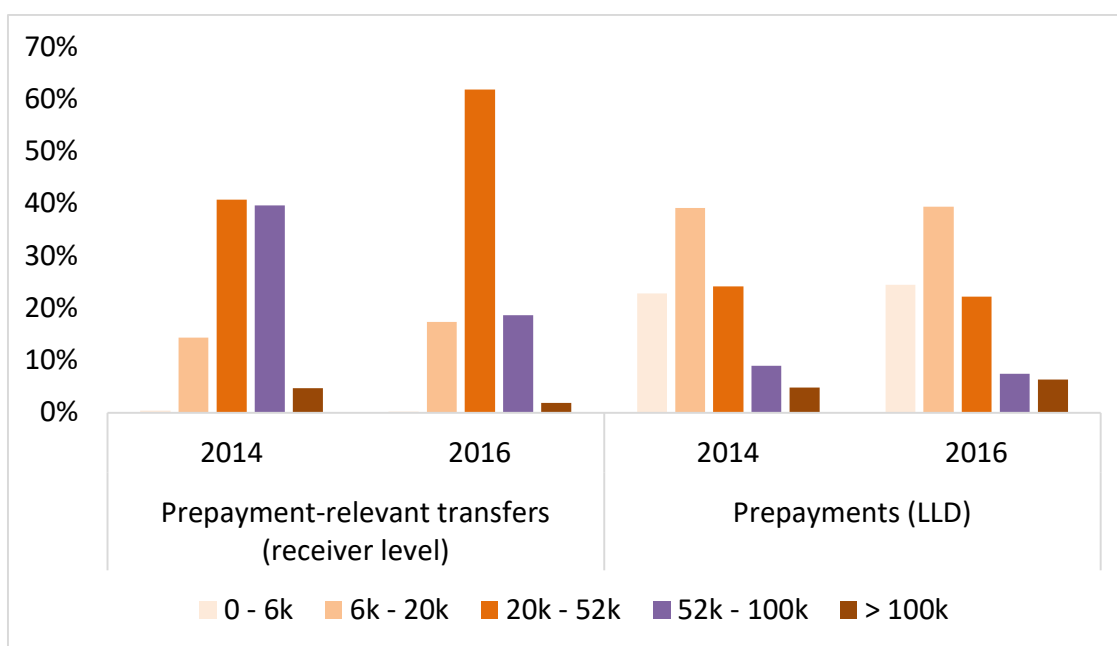
Explanatory note: Source, CBS microdata and DNB loan level data (LLD), own computations.

The figure shows that there are almost no transfers being reported below 6k, while on the contrary 25% of all prepayments are below this limit. These prepayments could still profit of transfers, but

they are unlikely to be reported in the tax data. Most reported transfers are between 20k and 52k, while most prepayments are below 20k. This means that transfers aimed at prepayments and real prepayments are very different phenomena. This is most evident when one considers that, once the new policy was repealed in 2016, transfers in the category 52k-100k dropped by 50%, while prepayments in the same category staid the same. It means that eliminating the higher thresholds of 52k or 100k will likely not affect most prepayments, that are already below the 6k threshold.

Table 3. Summary statistics of LLD quarterly mortgage data from 2013q1 to 2018q4

	Standard policy periods			New policy periods		
	Age group			Age group		
	31-35	**36-40**	**41-45**	**31-35**	**36-40**	**41-45**
Outcomes						
Indicator for prepayment	0.038	0.050	0.059	0.042	0.052	0.060
Mean of prepayment (conditional on prepayment>0)	18215	20945	22608	21608	23148	23943
Demographics						
Age	33.10	38.01	43.06	33.13	38.03	43.06
Economic related						
Interest rate Euribor (at time t).	-0.02	-0.03	-0.03	-0.06	-0.09	-0.09
Mortgage interest rate at reporting quarter (r_{it}) - Euribor$_t$	-4.58	-4.51	-4.47	-4.60	-4.58	-4.52
Household income at mortgage origination	55824	65680	71003	54551	64341	70558
Wage-employed at mortgage origination	0.77	0.69	0.63	0.76	0.68	0.62
Self-employed at mortgage origination	0.05	0.06	0.07	0.05	0.06	0.07
Other employment status at mortgage origination	0.01	0.01	0.02	0.01	0.01	0.01
Employment status missing at mortgage origination	0.15	0.22	0.28	0.13	0.20	0.26
Log of parents' wealth	5.25	5.38	5.34	5.46	5.58	5.52
Indicator for receiving transfers	0.003	0.003	0.001	0.004	0.005	0.004
Mortgage characteristics						
Reporting year - loan origination year (weighted by loan	4.63	5.59	6.55	4.44	5.49	6.45
Year of mortgage maturity - reporting year (the max. of	27.39	26.71	25.87	27.59	26.98	26.27
NHG indicator	0.68	0.51	0.40	0.72	0.55	0.43
Share of annuity or linear loan in a mortgage	0.15	0.11	0.09	0.17	0.11	0.09
Share of IO loan in a mortgage	0.23	0.28	0.30	0.22	0.27	0.29
Share of saving insurance loan in a mortgage	0.60	0.58	0.55	0.60	0.60	0.56
Share of investment loan in a mortgage	0.01	0.01	0.03	0.01	0.01	0.03
Share of other type loan in a mortgage	0.01	0.02	0.03	0.01	0.01	0.02
Current property value	205518	242765	271144	213199	251929	283874
Number of observations	1154580	1283311	1421609	1078154	1287953	1434906

Explanatory Note: a. NHG is short for National Mortgage Guarantee (in Dutch Nationale Hypotheek Garantie). If the borrower s situation changes for certain reasons beyond control (such as unemployment or getting divorced), the NHG provides a guarantee for repayment to the mortgage lender. Mean values reported. Source: DNB and CBS, own computations.

Table 3 reports summary statistics of outcome and control variables when analyzing prepayments based on LLD under the baseline policy periods (2013q1-2013q3 and 2015q1- 2016q4)

and the new policy periods (2013q4-2014q4 and 2017q1-2018q4). The table shows that the prepayment rate and conditional prepayment amount are higher under the new policy. The table also shows that the macro interest rate (measured by Euribor) is lower under the new policy, which could lead to more prepayments. For other variables, there is little difference across all periods.

Table 4. Summary statistics of CBS yearly transfer data from 2012 to 2018

	Standard policy periods			New policy periods		
	Age groups			Age groups		
	31-35	**36-40**	**41-45**	**31-35**	**36-40**	**41-45**
Outcomes						
Indicator for transfers	0.007	0.006	0.003	0.007	0.008	0.008
Mean of transfers (conditional on >0)	39954	43068	60444	52940	53539	57102
Demographics						
Age	33.07	38.05	43.06	33.06	38.02	43.08
Indicator for male	0.51	0.51	0.50	0.51	0.51	0.50
Number of parent(s)	1.70	1.61	1.48	1.69	1.61	1.49
Economic related						
Interest rate Euribor (at time t)	-0.01	-0.01	-0.01	-0.14	-0.14	-0.12
Personal gross income	43738	47632	49128	45538	49816	51641
Indictor for company director	0.01	0.02	0.03	0.01	0.02	0.03
Indictor for self-employed	0.08	0.10	0.12	0.09	0.10	0.12
Indictor for unemployed	0.02	0.02	0.02	0.01	0.02	0.02
Indictor for retirement	0.00	0.00	0.01	0.00	0.00	0.01
Indictor for other employment status	0.06	0.06	0.07	0.06	0.06	0.07
Gross value of own house	207229	231088	248900	225309	245147	259068
Log of parents' wealth	9.04	8.91	8.64	9.02	8.87	8.66
Number of siblings	1.70	1.61	1.48	1.69	1.61	1.49
Mortgage characteristics						
Indicator for having underwater mortgage	0.71	0.63	0.46	0.50	0.51	0.42
Number of observations	2384138	2711909	3261632	1749616	1960435	2208584

Explanatory Note: Mean values reported. Source: CBS, own computations.

Table 4 reports summary statistics of outcome and control variables when analyzing transfers based on CBS data under the baseline policy periods (2012, 2015 and 2016) and the new policy periods (2013, 2017 and 2018). The table shows that probability of receiving a transfer and amount being transferred are higher under the new policy. The value of house is also higher in the new policy period. For other variables, there is little difference across all periods.

4. Empirical strategy

Our study focuses on the effects of the baseline (52,000 euro tax exemption sourcing from parents) and new policy (additional 48,000 euro, all sources) on mortgage prepayments and transfers. The new policy together with the advertisement campaign (as we say in Figure 3), was aimed at stimulating transfers for prepayments, but could also stimulated a non-reported transfer (below the legal threshold for taxation). Before presenting the empirical strategy, we summarize the changes of the baseline and new policy on different age groups in different periods in Table 5, where we re-organize the information in Table 1 in order to facilitate the discussion of the identification strategy. The table shows that for the identification of the effect of the baseline policy, we use a set of regressions that does not use the policy change, but the comparison of different groups differentially affected by the given institutional design. For the identification of the effect of the new policy instead, we use the change in policy over adjacent periods.

Table 5: Overview of the identification strategy for effect of policy changes

		Identification effect baseline policy						Identification effect new policy			
		(1) Period 1 Baseline policy 1 before 2013q3	(2) Period 2 New policy 1 2013q4-2014q4	(3) Period 3 Baseline policy 2 2015q1-2016q4	(4) Period 4 New policy 2 2017q1-2018q4			(5) Period 1 Baseline policy 1 before 2013q3	(6) Period 2 New policy 1 2013q4-2014q4	(7) Period 3 Baseline policy 2 2015q1-2016q4	(8) Period 4 New policy 2 2017q1-2018q4
Age 31-35	Tax exemption: Source:	52k parents	52k+48k all	52k parents	52k+48k all	Age 31-35	Tax exemption: Source:	52k parents	52k+48k all	52k parents	52k+48k all
Age 36-40	Tax exemption: Source:	none	52k+48k all	52k parents	52k+48k all	Age 36-40	Tax exemption: Source:	none	52k+48k all	52k parents	52k+48k all
Age 41-45	Tax exemption: Source:	none	52k+48k	none	none	Age 41-45	Tax exemption: Source:	none	52k+48k	none	none

Explanatory note: The dark-shaded areas identify the group subject to the baseline and new policy, the light-shaded areas identify those are not subject to it. 52K and 48K are notation for the 52,000 and 48,000 euro tax free thresholds, respectively.

Baseline policy effect (52,000 euro tax exemption, sourcing from parents)

To identify the effects of the baseline policy, we use within period variation across age groups. Our primary strategy is to use a regression discontinuity design for the period 2012q4-2013q3 and 2015q1-2016q4 for the prepayments respectively (based on LLD) and 2012-2013 and

2015-2016 for the transfers respectively (based on CBS data). So, age group dummies capture the baseline policy effect, and we do not need to bother about checking common trends. The effect of aging itself instead is picked up by other variables, such a indicators for being a student, that are age-related. We compare the difference in four outcome measures (prepayment and transfers events, and the amounts being prepaid and transferred) across two adjacent age groups. As exemplified in the left panel of Table 5, there are two discontinuity points to identify the effect of the baseline exemption (the first one is between aged 31-35 and 36-40, the second one is between aged 36-40 and 41-45). We have highlighted these discontinuities in blue, where dark blue cells contain those falling within the baseline policy (treated) and the light blue indicates those outside the baseline policy (control).

We first look at column (1) of Table 5 and compare the outcomes of those aged 31-35 to those aged 36-40 in period 1 (refers to the term $Age_{i,t}^{3135}$ in Equation (1)). Next, we look at column (2) of Table 5 and compare the outcomes of those aged 36-40 and those aged 41-45 in period 3 (refers to the term $Age_{i,t}^{3640}$ in Equation (2)). We use the following two regression equations, in which $Age_{i,t}^{3135}$ and $Age_{i,t}^{3640}$ indicate the corresponding age groups, and β_1^1 and β_1^2 capture the baseline policy effect.

$$y_{it} = \beta_0 + \beta_1^1 * Age_{i,t}^{3135} + \gamma * \mathbf{Z}_t + \rho * \mathbf{X}_{it} + \varepsilon_{it}, \tag{1}$$

where the sample contains individuals aged 31-40 and the sample period spans from 2013q1 to 2013q3 (period 1) for the study of prepayments, and from 2012 to 2013 for transfers, respectively. Next,

$$y_{it} = \beta_0 + \beta_1^2 * Age_{i,t}^{3640} + \gamma * \mathbf{Z}_t + \rho * \mathbf{X}_{it} + \varepsilon_{it}, \tag{2}$$

where the sample contains individuals aged 36-45 and the sample period spans from 2015q1 to 2016q4 (period 3) for the study of prepayments, and from 2015 to 2016 for transfers, respectively. This makes the estimation of β_1^1 and β_1^2 depend on the differential introduction of the policy over time.

In both Equation (1) and (2), initially, y_{it} is an indicator of individual i making a prepayment or receiving transfers at time t (extensive margin). Next, y_{it} measures the prepayment amount or transfer amount (intensive margin).

During the observation period, the interest rate paid on saving accounts sharply dropped and reached unprecedented low levels (even negative), we use quarterly (for analysing

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prepayments in LLD) or yearly (for analysing transfers using CBS data) macro interest rates (Euribor rate) to capture the time effects (and assume no other form of time effect). These time related effects, as well as seasonal dummies for seasonal effects (only for analyzing prepayments as CBS data are collected yearly) are included in Z_t. The declining interest rate also makes alternative financial investments less attractive and prepayments more attractive, as the interest rate on mortgages is mostly higher. Such arbitrage possibility is controlled for in the empirical specification in the matrix X_{it} (only for analysing prepayments using LLD). The X_{it} matrix contains also all other variables listed in Tables 3 and 4, such as demographics and household finance information. We describe the results of this approach in the next section.

New policy (additional 48,000 euro tax free, all sources)

To identify the effect of the new policy, we use across-period-variation caused by the repeal and re-activation of the new policy, while the baseline policy was always active. As we discussed in Section 3, the new policy applied to all age groups at once in period 2. Therefore, when identifying the effects of the first activation of the new policy, no control group is available (in a cross-sectional sense) within the same period. A difference-in-differences design could not be applied here either, since the effects of the additional tax exemption is always on top of the effects of the standard exemption, and there is no age group who only qualified for the standard exemption but did not qualify for the additional tax exemption. Our primary strategy is to assume that macro related variables (such as the interest rate and seasonal dummies) control for time effects on outcomes, and that time (period) dummies capture the new policy effects. Then we can compare the difference in outcomes between the baseline policy period and the new policy period.

Possibly the new policy may have weaker effects when re-activated in period 4 (those who had already donated in the past could donate now only partially) or depending on age of the recipient (as people over time might have saved for a prepayment), so we separately estimate the effects of the new policy in period 2 and 4 and for the different age groups. This is again exemplified in Table 5 with 3 sets of dark (treatment) and light (control) colors, where these sets will be identified by the letter j in equation (3) below. Thus, for those with 31-35, we look at outcomes in period 2 (corresponds to left dark green in Table 5) and period 4 (corresponds to right dark green in Table 5) compare them to those in period 1 (corresponds to left light green in Table 5) and in period 3

(corresponds to right light green in Table 5), respectively. These are the cases where j is either equal to 1 or 2. For those with age 36-40, we look at outcomes in period 4 (corresponds to right dark green in Table 5) compare them to those in period 3 (corresponds to right light green in Table 5). This is the case with j = 3. We use the following three regressions, in which $Treatment_t$ indicates the corresponding period. Our prime interest is to estimate β_2^j, which captures the causal effects of the new policy:

$$y_{it} = \beta_0 + \beta_2^j * Treatment_t + \gamma * Z_t + \rho * X_{it} + \varepsilon_{it}, \tag{3}$$

As the identification is based on the interaction of the different periods with age, in each of the j cases described above we must estimate Equation (3) on specific age-related subsamples. For *j=1*, 2 and 3, the sample contains those with age 31 to 35, 31 to 35 and 36 to 40; *t* covers period 2013q3-2014q4 (period 1 and period 2), 2015q1-2018q4 (period 3 and period 4) and 2015q1-2018q4 (period 3 and period 4) for the study of prepayments, and 2012 to 2014,2015 to 2018 and 2015 to 2018 for transfers, respectively.

5. Results

Table 6 reports the main results of 20 separate regressions of our model, estimated by OLS[15], while the full results are contained in Appendix C. The left panel looks at quarterly prepayments, the right panel at yearly transfers. Panel A, upper section, reports the effects of the baseline policy and Panel B, in the lower, reports the effects of the new policy. In both panels we report on the left the study of prepayments, and on the right the study of transfers. Within each, we look at the prepayment or transfer rate (white section) and amounts (grey section). So, on the left, we summarize results for models explaining the probability to make a prepayment (models C1, C2, C9, C10, C11), while in models C3, C4, C12, C13 and C14 we look at the prepaid amount. Models C5, C6, C15, C16 and C17 explain the probability to receive a transfer, and models C7, C8, C18, C19 and C20 the transferred amount.

Before describing the main results, notice that we only report the coefficient of interest, β_1 and β_2. In all regressions, we include all the controls as discussed in Section 3. To briefly comment on these variables, we notice that the prepayment rate is positively related to income, the presence of an interest-only loan, house prices and personal financial wealth, while it is negatively related to the interest rate, age, residual debt insurance and loan maturity. The amount prepaid is positively related to income, to being employed, and to receiving a transfer, while it is negatively related to the interest rate, debt insurance, and the presence of interest-only loans. These variables are not strictly exogenous, as insured borrowers are typically younger and have less expensive homes, while older people have more often bullet loans. The probability to receive a transfer and the transfer amount instead are positively related to age, being a male, being either self-employed or retired, or being relatively more financially solid, while it is negatively related to being currently underwater.

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[15] Despite our panel data, we cannot estimate in most cases a fixed effect model, because our identification exploits discontinuity across group within a given period. This period is often too short to allow multiple observations (for instance in case of the baseline policy the first period only elicits one full year), and in a few cases we have two years. But even in these cases transfers can only be received once. Prepayments instead could be repeated. Results with individual fixed effects confirm our results for the amount prepaid, while the effect on the prepayment rate becomes either not-significant or negative.

Table 6. Main estimation results for prepayment (using quarterly LLD) and transfers (using yearly CBS data)

Panel A: Baseline Policy

Full results in appendix table:	C1	C2		C3	C4		C5	C6		C7	C8	
	Indicator for prepayment			Amount prepaid (if > 2000)			Indicator of transfers			Amount transferred (>0)		
Sample	Period 1 age 31-40	Period 3 age 36-45		Period 1 age 31-40	Period 3 age 36-45		2012 age 31-40	2015-2016 age 36-45		2012 age 31-40	2015-2016 age 36-45	
Coeff.	-0.000525	-0.000905	\	-475.8	-609.7	\	0.00457***	0.00358***	\	-3,194***	-2,109*	\
Standard error	0.0010	0.0006	\	828.9	429.5	\	0.0002	0.0001	\	864.4	1,109	\
Mean dep. Var.	0.0362	0.0537	\	18095	21269	\	0.008	0.002	\	45986	33077	\
Relative effect	not sig.	not sig.	\	not sig.	not sig.	\	56%	155%	\	-6.9%	-6.4%	\
N	589875	2033077	\	21371	109138	\	2627025	2842382	\	21337	6569	\

Panel B: New Policy

Full results in appendix table:	C9	C10	C11	C12	C13	C14	C15	C16	C17	C18	C19	C20
	Indicator for prepayment			Amount prepaid (if > 2000)			Indicator of transfers			Amount transferred (>0)		
Sample	Period 1-2 age 31-35	Period 3-4 age 31-35	Period 3-4 age 36-40	Period 1-2 age 31-35	Period 3-4 age 31-35	Period 3-4 age 36-40	2012-2014 age 31-35	2015-2018 age 31-35	2015-2018 age 36-40	2012-2014 age 31-35	2015-2018 age 31-35	2015-2018 age 36-40
Coeff.	0.00511***	0.00285***	-0.0001	2,666***	-111.0	-1,746***	0.00976***	0.00154***	0.0108***	5,955***	14,133***	12,832***
Standard error	0.0006	0.0005	0.0005	475.6	535.2	441.0	0.0002	0.0001	0.0002	448.2	815.4	695.2
Mean dep. Var.	0.0378	0.0376	0.0462	18658	19306	21073	0.0129	0.0051	0.0129	48378	41669	40667
Relative effect	13.5%	7.6%	not sig.	14.3%	not sig.	-8.3%	76%	30%	84%	12.3%	33.9%	31.6%
N	798276	1405212	1683337	30143	52844	77700	1814687	2318849	2071344	23436	11930	14523

Explanatory note: the table reports the coefficient and standard error of the treatment indicators, the mean of the dependent variable in the estimating sample, and the mean effect of the treatment indicator relative to this sample mean. Full results of each of these models are reported in Tables C1 through C20 in the appendix.

Panel A shows the estimated effect of the baseline policy ($\beta_1^j, j = 1,2,3$). Models C1 to C4 show that those affected by the baseline policy are not more likely to prepay, nor to prepay more when they make a prepayment. One possible explanation is that younger household (e.g., aged 31-35) might have already benefited of transfers for a down-payment and now either their parents have less available funds or have transferred already the maximum amount (as the rule is one-off one then does no longer qualify). Instead, the baseline policy contributed to increasing the probability of a transfer (see models C5 and C6). In the sample period 2012-2013, comparing those aged 31-35 to aged 36-40, the results suggest an increase by 56%. The causal effect of the policy is instead around three times as large if we use sample period 2015-2016 and compare ages 36-40 to ages 41-45.[16] These results suggest that the baseline policy contributed significantly to increasing transfers. However, while the probability of a transfer increases , we also find evidence that for the households that receiving transfers, the marginal transfer amounts are lower. The results in Models C7 and C8 show that those affected by the baseline policy received about 3200 and 2000 euro less transfers, notwithstanding whether we identify the effect using the different age groups in different periods. This indicates that relatively 'marginal' households started transferring too, but lower amounts.

Panel B shows the effect of the new policy, on top of the baseline policy ($\beta_2^j, j = 1,2,3$).[17] Lifting the tax-exempted threshold from 52,000 euro to 100,000 euro with extra sourcing in period 2 and 4 increased the probability of making prepayment by about 0.5 and 0.3 percentage points respectively (Models C9 and C10). Comparing these estimates to a base prepayment rate of about 3.8 percentage points (the average prepayment rate of the estimating sample), this means that the activation and re-activation of the new policy induced a 13.5% and 7.6 % increase in the prepayment rate, respectively. For older borrowers (Model C11) we found no significant effect.

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[16] One should notice that these effects look relatively large, while the absolute transfer's rate is very low (in the order of magnitude of 0.23%).

[17] Other control variables in the analysis of prepayments using the specification of column (1)-(2) in Panel B are reported in Table A1 and Table A2 in Appendix C. We observe that larger arbitrage opportunities (when the mortgage interest rate is higher), increase both the probability and the conditional amount of making a prepayment. Higher income households (at loan origination) are more likely to make prepayments and for a larger amount. Self-employed households are less likely to make prepayments and their prepayments are lower. The probability of prepayment and their size are positively related to the combine effects parents' wealth and parents being observed in the datasets and to the number of parents being alive.

Prepaid amounts instead increased by 2666 euro only in the first new policy period (Model C12) and not after the policy was reintroduced (Models C13 and C14). Relative to the average prepayment of 18,662 euro, the first introduction of the new policy in period 2 induced a 14,3% increase of prepaid amount.[18]

The new policy also resulted in an increase in transfers rate and transferred amounts for all age groups; the first and second activation of the new policy increased transfer rate by 0.97 0.15 and 1.08 percentage points. In relative terms these increases amount to 76%, 30% and 84% relative to the mean of the dependent variable, that is though a small number, as the probability of a transfer is low in general. Transferred amounts increased by 5955, 14133 and 12832 euro, respectively.[19]
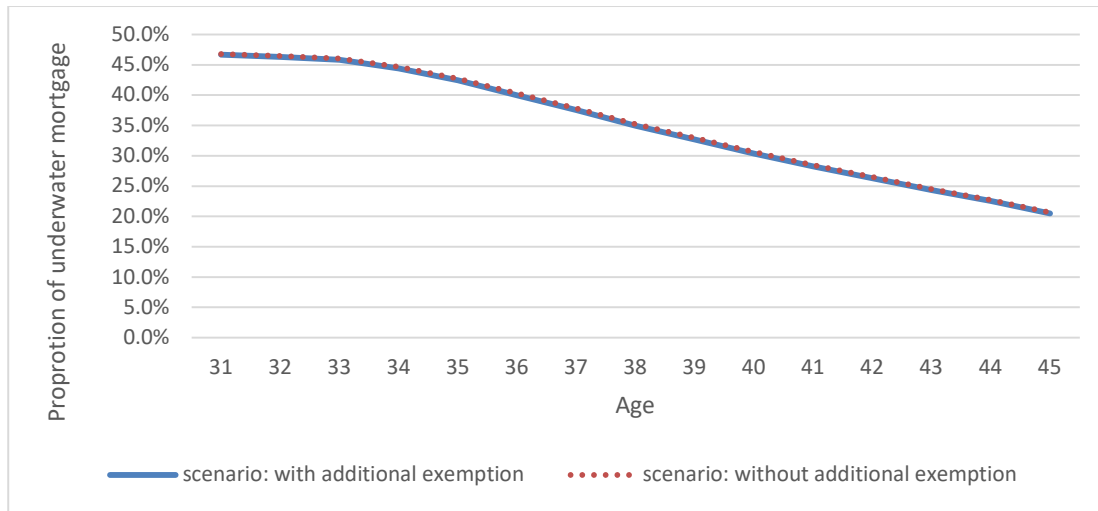
5.1 Underwater mortgages

Increases in house prices and mortgage amortization (contractual or voluntary) help to reduce the share of underwater mortgages. In this section, we will turn to examine to what extent the new policy achieved the aim of reducing the share of underwater mortgages. The estimates in the left of panel B of Table 6 report the causal effects of the new policy on prepayments. We interpret these effects as being causal because we correct in the analysis for a large number of age-related and period related effects. Besides the policy, mostly in the second period, was pre-scheduled and therefore independent from the business cycle at the time of implementation. This means that we can predict within sample prepayments using our preferred estimates. We use a two-step micro-simulation model, based on the specifications in column (1) (for the decision to prepay) and column (4) (for the prepaid amounts) in Panel B. These predictions can be compared to a second

[18] Other control variables in the analysis of prepayments using the specification Models C9 and C10 show that larger arbitrage opportunities (when the mortgage interest rate is higher), increase both the probability and the conditional amount of making a prepayment. Higher income households (at loan origination) are more likely to make prepayments and for a larger amount. Self-employed households are less likely to make prepayments and their prepayments are lower. The probability of prepayment and their size are positively related to the combine effects parents' wealth and parents being observed in the datasets and to the number of parents being alive.

[19] Other control variables in the analysis of transfers using the specification of Models C15 to C20 show that the rate of borrowers receiving transfers and the amount being transferred are positive related to the number of parents and the log of parents' wealth. Self-employed children are more likely to receive transfers then wage-employed children.

set of predictions in which we neutralize the effect of the new policy (by setting the estimates of β_2 to zero). The simulated prepayments can then be used to replace the real prepayments in order to recompute a counterfactual underwater status.

Figure 4: Micro simulations for scenario with additional exemption and without additional exemption over ages

The simulated distribution of the share of underwater mortgages by age and the counterfactual distribution are presented in Figure 4. The share of the underwater mortgages decreases with age (due to the combined effect of amortization and price increases). There is however no discernible difference between the simulated and counterfactual distributions. This indicates that the new policy has no effect on reducing the share of underwater mortgages.[20]

Another way to appreciate the effect of the new policy across households with different levels of indebtedness is to look at the debt reductions induced by the new policy by originating LTV. We capture this identifying two originating LTV categories (0,100%] and above 100%, that we also multiply by $Treatment_t$ of Equation (3). This means replicating the specifications in column (1) and (3) of Panel B in Table 6 for two different originating LTV category.

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[20]The mean difference between the two lines is 0.21%.

Based on these model estimates, we perform again a prediction with counterfactual as we did above. For the original LTV categories (0,100%], we find a predicted prepayment rate of 0.0494 and a counterfactual prepayment rate of 0.0427 (without new policy). The predicted prepaid amount instead was 18,621 euro while the counterfactual amount was 17,346 euro. When combining the effects on both the prepayment rate and the prepaid amount, the average prediction appears to be 179 euro larger than the counterfactual one. Given the observed residual debt in *t-1* is 212,709 euro, this implies that the new policy resulted in a reduction of 0.08% of remaining debt for non-underwater mortgages. Similarly, the simulation shows that the reduction was 0.03% of remaining debt for underwater mortgage.

Overall, this implies that debt reductions were larger for debtors that originated above water, and that the share on underwater mortgages did not decrease as a result of the policy. Put differently, most transfers motivated by the new policy were made from wealthy parents to relatively more housing-rich children.

Table 7: Regression results by original LTV category

		LTV<=100	LTV>100
1st stage: extensive margin	Avg. of predicted prep. rate with new policy (*A*)	0.0494	0.0285
	Avg. of predicted prep. rate without new policy (*B*)	0.0427	0.028
2nd stage: intensive margin	Avg. of predicted conditional amount with new policy (*C*)	18,621	18,453
	Avg. of predicted conditional amount without new policy (*D*)	17,346	16,525
Combined effects	*AxC*	920	526
	BxD	741	463
	Difference	179	63
	Avg. remaining debt before prep.	212,709	240,500
	Effects of new policy in the reduction of debt (in %)	0.0008	0.0003

6. Conclusions

From 2013 to 2018, the Dutch government modified twice the taxation of intergenerational transfers aimed at mortgage down-payments and prepayments. The original regulation allowed parents to transfer 52,000 euro tax free to children below age 35. During the periods in which the policy was relaxed (threshold moved to 100,000 euro, age limit was dropped, and anyone could donate), we observed an increase in mortgage prepayments and intergenerational transfers. We identify the effects of the tax exemption on prepayments and transfers by exploiting these changes. The two policy changes resulted in a 14% and 7% increase in the probability of making a prepayment respectively, and a 75% and 30% increase in the probability of receiving a transfer. The first policy change also increased the amounts prepaid by 14% and the amounts being transferred by 12%, while the second policy change only increased the amounts being transferred by 44%. These new fiscal policies were meant to reduce the existing number of underwater mortgages, at that time more than one-third of all mortgages. We find that the prepayments motivated by the policy change, increased mostly for borrowers with low original loan to value (LTV) ratios. This implies that most transfers were made from wealthy parents to housing-rich children, and that the policy did not help to reduce the share of underwater mortgages. This suggests that policy makers might want to target these types of policies more precisely, directing it more to the desired group of highly indebted households. Generic wealth-tax measures do not automatically sort in favor of the desired groups.

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Appendix A: Transfers

There is a strong increase in the number of users of the tax-exemption policy and the amount being received during the relaxation period. Table A1 provides information of these transfers and was elicited from all tax forms of receivers and has been produced ad hoc by the tax office. The left panel of Table 1 shows a sharp increase in the number of users during the policy relaxation period in 2013 and 2014, with a total transferred amount increasing from 2.7 to 9.4 billion euro. Also, the right panel of Table A1 shows that this increase in the number and amount is almost exclusively due to transfers falling within the relaxation tax exemption (both in terms of number of receivers (73%) and transferred amount (74%)).

The figures produced by the tax office are however too aggregated to study the characteristics of the receivers, and do not allow for instance to tell about the distributional effects of the measure. Also, microdata on these transfers were never released, so we need to resort to other data sources in order to understand these effects.

Table A1: Tax-facilitated intergenerational transfers from tax-records

	Total tax-exempted transfers		Transfers within tax-exemption threshold	
	num. of receivers	amount received (in bln EUR)	num. of receivers	amount received (in bln EUR)
2012	49,803	2.72	NA	NA
2013	101,871	5.71	NA	NA
2014	158,930	9.40	116,325	6.99
2015	50,521	3.33	NA	NA

Note: 1. Source from Dutch tax office (In Dutch: Belastingdienst). 2. Business equity transfers are excluded.

Appendix B: data selection

Table A1: Steps of the sample selection, 2016q1 LLD and CBS wealth data.

Step	Description	Observations left	% left of previous step
0	**Number of mortgages in the LLD**	2 191 070	
1	Drop mortgages that appears less than 6 times. (we need this to be able to	2 008 617	92%
2	Data quality issues		
	Household income missing or trimmed		
	Interest rate too high (>10%)		
	Original LTV >150%		
	Inactive loans	1 595 656	79%
	Maturity in the past		
	Vintage above 30 years		
	*Original valuation amount > 5*10^6*		
3	Age selection 31-45	467 251	29%
4	Prepayments missing or trimmed	461 736	99%
5	Prepayments in each period (misclassification of standard repayments)	458 533	99%
6	**Merged wealth data**		
	No household identifier available	263 786	58%
	No parental household identifier	252 786	96%
	No parental wealth available	229 709	91%

Explanatory note: This table shows the sample selection process for a given quarter in the data. In the different table we pool multiple periods, therefore the sample sizes differ. Also, not all regression run on the base of the LLD, some are based on transfer data of CBS.

Appendix C - Full regression results

Table A2: Regression results for transfers

Model	C5	C6	C7	C8	C15	C16	C17	C18	C19	C20
Qualifies for baseline policy	0.00457***	0.00358***	-3,194***	-2,109*						
Qualifies for new policy					0.00976***	0.00154***	0.0108***	5,955***	14,133***	12,832***
Age	0.000281***	-9.97e-05***	-998.8***	126.9	0.000508***	-9.09e-07	-3.28e-05	-817.7***	-693.6***	-343.7**
Male	0.000591***	-0.000179***	-474.1	-233.4	0.00145***	0.000727***	0.000625***	-47.26	875.4	27.02
Company director (0/1)	0.00557***	0.000189	11,411***	2,714**	0.0139***	0.00198***	0.0106***	12,191***	9,246***	6,642***
Self-employed (0/1)	0.00170***	0.000294***	3,255***	1,035	0.00381***	0.000397**	0.00269***	4,095***	4,983***	3,450***
Unemployed (0/1)	-0.000746*	-0.000283	2,235	3,445	-0.00131**	-0.000569	-0.000969*	3,440*	6,660**	-664.6
Retiree (0/1)	0.00396***	-0.000460	12,747***	5,218	0.00713***	0.000683	0.00482***	12,096***	11,376*	1,236
Other employment status (0/1)	0.000994***	4.82e-06	6,048***	312.1	0.00158***	1.89e-06	0.00137***	6,809***	8,347***	590.4
Gross income/10^6	47.40***	8.152***	-8.754e+06	1.032e+06	103.0***	34.96***	56.23***	654,728	3.464e+07***	-3.184e+06
Value of main residence/10^6	0.0230***	0.00106***	48,576***	10,262***	0.0340***	0.00332***	0.0370***	44,769***	24,738***	16,470***
Underwater mortgage (0/1)	-0.00278***	0.000139**	-2,177***	-915.3*	-0.00615***	0.000180*	-0.00439***	-1,327***	301.3	-443.4
Number of parents	-0.00656***	-0.00135***	-3,469***	-1,624***	-0.0111***	-0.00414***	-0.0103***	-3,200***	-2,969***	-2,850***
Log parental wealth	0.00270***	0.000569***	3,016***	847.8***	0.00434***	0.00155***	0.00418***	2,315***	1,467***	1,046***
Number of siblings	-0.00154***	-0.000365***	-931.5***	-906.5***	-0.00206***	-0.000796***	-0.00211***	-975.5***	-1,083***	-739.9**
Interest rate (Euribor)	0.0525***	-0.00193***	64,333***	1,133	0.0336***	-0.000517	0.0757***	66,250***	-335.2	1,712
Constant	-0.0269***	0.00140	32,115***	22,839***	-0.0370***	-0.00438***	-0.0268***	29,933***	37,423***	36,342***
Observations	2,627,025	2,842,382	21,337	6,569	1,814,687	2,318,849	2,071,344	23,436	11,930	14,523
R-squared	0.012	0.003	0.109	0.016	0.017	0.003	0.022	0.095	0.093	0.068
Log Lik.	2.621e+06	4.601e+06	-250648	-74211	1.399e+06	2.829e+06	1.607e+06	-275593	-139122	-168959

Explanatory note: This table presents the full set of results summarized in Table 6, right panel.

Table **A3**: Regression results for prepayments

Model	C1	C2	C3	C4	C9	C10	C11	C12	C13	C14
Qualifies for baseline policy	-0.000525	-0.000905	-475.8	-609.7						
Qualifies for new policy					0.00511***	0.00285***	-6.92e-05	2,666***	-111.0	-1,746***
Interest rate in t - mortgage interest rate	0.00498***	0.00432***	3,057***	1,473***	0.00368***	0.00447***	0.00292***	2,285***	1,829***	1,500***
Log age	0.0186***	-0.00839*	4,343	-1,420	0.00489	0.00449	-0.00838*	8,730**	-1,009	-4,739
Household income	0.192***	0.105***	76,622***	48,214***	0.321***	0.186***	0.186***	70,364***	53,230***	35,062***
Self-employed	-0.00812***	-0.00347***	4,511***	6,402***	-0.00764***	0.000839	-0.00150**	2,688***	6,366***	6,357***
Employment status: other	0.0208***	0.108***	4,235***	1,162**	0.0103***	0.0279***	0.0599***	1,896	1,473	2,825***
Employment status unknown	0.00477***	-0.00633***	1,551***	1,794***	0.00748***	-0.00315***	-0.00418***	764.2*	1,270**	2,935***
Current vintage	-0.00330***	0.000324***	-129.2	-163.5***	-0.00103***	0.000938***	0.000440***	266.9***	357.5***	314.0***
Loan term (<=30 years)	-0.00262***	-0.000597***	387.2***	510.5***	-0.000524***	-0.000401***	-0.000455***	362.9***	839.1***	634.2***
Residual debt insurance	-0.00946***	-0.0175***	-1,153**	-762.8***	-0.0132***	-0.0162***	-0.0141***	-1,700***	-1,839***	-1,079***
Share Interest-Only loans	-0.0135***	0.0481***	-7,591***	-4,093***	0.00899***	0.0116***	0.0187***	-3,055***	-292.5	-3,561***
Share saving loans	-0.00748***	-0.00167**	-7,660***	-5,265***	-0.0112***	-0.00384***	-0.00174***	-2,303***	24.67	-2,699***
Share investment loans	0.00842***	0.0422***	-10,589***	-249.0	0.0159***	0.0344***	0.0312***	-3,316**	2,987	-399.6
Share other loans	-0.0137***	0.0102***	-5,675***	-1,443	-0.0198***	-0.00608**	0.00163	-1,150	2,764	-2,435
Current valuation amount	0.0928***	0.103***	23,554***	29,256***	0.0954***	0.0873***	0.0950***	25,840***	17,345***	26,163***
Parental wealth	0.466***	0.167***	19,667	-130,040***	0.0737	0.211***	0.373***	-66,331**	-56,891*	-94,995***

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Region 2 (0/1)	-0.000983	-0.0119***	-5,860***	329.1	-0.0117***	-0.00493**	-0.00574***	-502.4	-187.3	65.93
Region 3 (0/1)	-0.000647	-0.0120***	-7,844***	279.7	-0.0135***	-0.00982***	-0.00851***	-1,558	2,186	-1,109
Region 4 (0/1)	0.00353	-0.00387**	-7,646***	538.6	-0.00427	-0.00340	-0.00361*	-2,391	852.0	-925.3
Region 5 (0/1)	0.00172	-0.00829***	-7,521***	689.5	-0.0103***	-0.00634***	-0.00411**	-2,404	0.606	-872.0
Region 6 (0/1)	-0.00156	-0.0118***	-7,128***	-352.5	-0.00983***	-0.00731***	-0.00688***	-360.8	875.7	-692.5
Region 7 (0/1)	-0.00155	-0.0125***	-5,326**	1,780*	-0.0141***	-0.00754***	-0.00812***	455.5	1,833	529.4
Region 8 (0/1)	0.00431*	-0.00863***	-5,883***	1,997**	-0.00892***	-0.00401*	-0.00500***	1,097	1,192	669.7
Region 9 (0/1)	0.000514	-0.0121***	-5,914***	994.2	-0.0133***	-0.00708***	-0.00750***	-1,257	897.1	-499.1
Region 10 (0/1)	0.000151	-0.0125***	-7,087***	971.6	-0.0138***	-0.00798***	-0.00836***	-1,349	65.80	-33.82
Region 11 (0/1)	-0.000988	-0.00910***	-7,247***	1,140	-0.0123***	-0.00817***	-0.00440**	-570.9	3,279*	-863.8
Region 12 (0/1)	0.000465	-0.00727***	-6,355***	1,292	-0.00956***	-0.00464**	-0.00325*	-824.8	1,317	-924.6
Region 13 (0/1)	0.00856***	0.00341*	-8,295***	717.9	0.00145	0.00549**	0.00801***	-1,318	-72.19	-161.1
Region 14 (0/1)	0.00290	-0.00868***	-6,217***	403.1	-0.0104***	-0.00431*	-0.00584***	-1,974	-458.0	-517.9
Region 15 (0/1)	0.00442	-0.00780***	-4,935**	1,591	-0.0101***	-0.00706***	-0.00680***	-610.2	5,727***	1,379
Region 16 (0/1)	0.00252	-0.00710***	-6,227***	853.0	-0.00906***	-0.00486**	-0.00281	-1,679	1,185	-402.7
Quarter 2 (0/1)	0.000557	-0.0113***	4,935***	1,241***	-0.00424***	-0.00709***	-0.0114***	2,174***	1,591***	1,346***
Quarter 3 (0/1)	-0.0112***	-0.0129***	2,832***	-2,593***	-0.0152***	-0.00750***	-0.0141***	1,247**	592.3	-1,006**
Interest rate in t		0.0347***		-15,711***	-0.140***	0.0178***	0.0261***	-15,121***	-20,762***	-15,225***
Quarter 4 (0/1)		0.0243***		-5,640***	0.00929***	0.0178***	0.0202***	2,180***	-3,127***	-4,138***
Interest rate type fixed			-10,327	-36,435**						-60,318**
Constant	0.0603***	0.0905***	5,010	11,728	0.0705***	0.0380***	0.0775***	-19,999	-3,840	17,964
Observations	589,875	2,033,077	21,371	109,138	798,276	1,405,212	1,683,337	30,143	52,844	77,700
R-squared	0.013	0.025	0.053	0.065	0.015	0.011	0.017	0.045	0.025	0.036
Log Lik.	156361	169617	-250408	-1.295e+06	196639	346049	254323	-350124	-632569	-930472

Explanatory note: This table presents the full set of results summarized in Table 6, left panel.