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from rich parents to rich children?

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DP 11/2023-049

the review of income and wealth

Review of Income and Wealth
Series 0, Number 0, 2023
DOI: 10.1111/roiw.12644

MORTGAGE PREPAYMENTS AND TAX-EXEMPTED INTERGENERATIONAL TRANSFERS: FROM RICH PARENTS TO RICH CHILDREN?

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The Dutch government implemented two changes to the taxation of intergenerational transfers aimed at mortgage down payments and prepayments. We identify the effects of these tax exemptions on prepayments and inter vivos transfers separately by taking advantage of the changes in policy design. The policy changes resulted in two expansions of tax-exempt transfers, which increased the probability of receiving such transfers, translating into a modest increase in prepayments. Initially, the amounts prepaid increased by a similar magnitude, while the second policy change only resulted in an increase in the amounts being transferred but not the prepayments. The macroprudential policy goal was to reduce the number of underwater mortgages, but the policy was too generic and did not help to achieve this. The prepayments triggered by the policy change increased mostly for borrowers with low original loan-to-value ratios, implying that most transfers were made from wealthy parents to housing-rich children.

JEL Codes: G5, H2

Keywords: mortgage repayments, intergenerational transfers, household indebtedness

1. INTRODUCTION

The 2009–2013 asset-prices crisis severely hit the Netherlands and left more than one third of borrowers with an underwater mortgage, meaning that the value of the house was lower than the outstanding principal. Given the high share of interest-only (IO) mortgages, the Dutch government and De Nederlandsche Bank (the Dutch Central Bank, hereinafter: DNB) decided to employ a varied set of macroprudential tools. The tools included, for example, discouraging IO loans to new borrowers while allowing most of them to insure against residual debt, adjusting risk weights for high loan-to-value (LTV) mortgages, and reducing LTI (loan-to-income) and LTV caps. None of these measures is unique to the

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Netherlands, and similar measures have been discussed in the international literature as well, but the combination of all of them is exclusively a Dutch phenomenon (OECD 2020). The idea behind 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.* 2017). Excessive indebtedness increases homeowners' propensity to strategically default when the value of the mortgage exceeds the value of the house (Guiso *et al.* 2013). It also amplifies the procyclical expansion of leverage over the economic cycle, in combination with expectations that are too optimistic (Geanakoplos 2010).

In this study, we consider a different and less common macroprudential tool. We investigate the relaxation of the Dutch tax law governing intergenerational transfers (inter vivos and inheritances) when these are used either for mortgage down payments of first-time buyers, prepayments, or to finance a home improvement.[1] This is similar to the estate and gift taxes in the US (Poterba 2001). With the new rules, the Dutch government aimed at preventing the underwriting of underwater mortgages (by encouraging down payments) and at reducing the number of existing underwater mortgages (by encouraging prepayments). Here we focus exclusively on the latter because our data only allow for the identification of the prepayments of continuing costumers, but not the down payments of first-time buyers. This because we can only elicit down payments from the difference between originating debt and original property valuation. However, this difference is also positive for those who buy a new home by investing the equity of their previous home in the new contract, and for those who renegotiate their contract (possibly with a different bank), requiring the origination of a new mortgage. In these cases, we would erroneously assign down payments to a larger set of the population who were already homeowners and had not engaged in any behavior that would be relevant to the policy that we discuss here (saving, requesting transfers, etc.). Without an identifier for first-time buyers it is not possible to isolate the policy-relevant group for the study of down payments. We observe an increase in both mortgage prepayments and intergenerational transfers during the 13 quarters in which the government relaxed these taxes in various ways.

Our research question is whether the tax exemption per se (hereinafter: baseline policy), as well as its relaxation (hereinafter: 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 dataset that links parents' wealth and inter vivos transfers and that contains almost the entire population of mortgage borrowers, we study whether the fiscal treatment of intergenerational transfers encouraged prepayments.

Related literature has revealed that intergenerational transfers can affect an individual's decision on taking out a mortgage in two ways. First, intergenerational transfers from family members alleviate a down-payment constraint on home purchase. This literature interprets the positive correlation between an individual's home-ownership and parental financial support as evidence of credit-market

[1]Investing the money in 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.

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 and Jappelli 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 shows weak or no evidence that transfers from parents facilitate home ownership of children (Guiso and Jappelli 2002; Kolodziejczyk and Leth-Petersen 2013). In the second scenario (the lump-sum), the 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, which are both unobserved. Transfers can ease borrowing constraints (Cox 1990), be determined by altruism (Mukherjee 2020), or by a set of social rules (Cigno 1993); 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, a 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 the past to reflect the present value of those transfers. Third, the effects being measured can be heterogenous across the population (Modigliani 1988).

To address the endogeneity issue, we identify the causal effect of the tax policies on intergenerational transfers, using a temporary change of their fiscal treatment. The policy was changed twice spanning a period of 13 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 give a one-off, tax-exempt gift of a maximum of 52,000 euro to their children up to age 35. During the first reform, the tax-exempt threshold increased to 100,000 euro, and was again only available once in a lifetime. Anybody (not only parents) could become a donor, and there was no more upper age limit for the beneficiary, thus enlarging the treatment group to the entire population. So 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 orphans could in principle also receive a tax-free transfer. With the second reform, starting in 2017Q1, the only difference is that the upper age limit for the beneficiary was set at 40. This means that, thanks to these discontinuities across groups, we can identify a treatment and control group, and thus a causal effect. Assessing this is relevant as observed intergenerational transfers tend to be lower than the level that would be implied by intertemporal models given their tax treatment (Poterba 2001). This could partly be due to tax design, which has been found to be relevant in affecting transfers (Advani and Tarrant 2021).

The second issue, data quality, is addressed using a new and unique combination of administrative data sources. To the best of our knowledge, administrative data on intergenerational transfers and how beneficiaries use such transfers have not been widely investigated. This data generally does not report why such transfers are made nor how the transfers are used (Kotlikoff 1988). One of the datasets we use is DNB loan-level data (LLD), which has very high coverage, frequency, and granularity, and which is the only dataset in the Netherlands that makes our analysis possible; see Mastrogiacomo and van der Molen (2015). This enables us to analyze about 80 percent of all existing mortgages in the Netherlands (about 95 percent of all mortgages offered by banks). The quarterly frequency of the dataset is higher than the annual data provided by the tax authorities, meaning we can 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, including the type of loan (that signals the need for a prepayment, as many borrowers in the Netherlands have non-amortizing loans).[2] The panel nature of the data also allows for the identification of the underwater state of the borrower and enables us to observe how this evolves. A unique feature of this study is that we link this data to tax records on parents' wealth as reported yearly. Additionally, we also perform the analysis using the tax records on transfers. This is micro data in which the tax authorities register the purpose of the transfer, i.e. whether the transfer was used to pay off mortgage debt (exemptions from inheritance taxes on transfers are available only for specific reasons, such as paying for higher education).

We address the distributional concerns by looking at the home equity of the beneficiaries 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 homeowners. Note that intergenerational transfers are not necessarily associated with easing home ownership (Guiso and Jappelli 2002), so it is relevant to look how such transfers affect the debt position of the borrower.

This study contributes to multiple domains. We add to the limited amount of research on the effects of macroprudential tools at the micro-level (see Caloia 2020). In doing so, we use a well-established research design that allows for the identification of the causal effect of taxation on indebtedness, thus isolating this effect from all other endogeneities. We show this effect on solvency risk, focusing on underwater mortgages, and we reveal how intergenerational transfers could contribute to inequality. We also add to the literature that shows that prepayments are affected by observables (Green and Shoven 1986; 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 a 14 percent and 7 percent increase, respectively, in the probability of

[2]Interest-only loans were a noteworthy financial innovation that started in the 1990s. These loans became very popular due to various combined fiscal advantages linked to the mortgage interest deduction. About 60 percent of all Dutch loans were interest-only at the inception of the new policy described here.

making a prepayment, and a 75 percent and 30 percent increase, respectively, in the probability of receiving a transfer, with the latter being a less frequent event. We also find a 14 percent increase in the amount prepaid in the first new policy period, and an increase of 12 percent and 44 percent 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 borrowers. 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, leads to the main conclusion of our study: while the new policy was effective in increasing prepayments, it was not targeted enough to reduce the share of underwater mortgages. The other component of the policy, focusing on down payments, is not investigated here due to a lack of data.[3] It is worthwhile noting related literature that shows that tax incentives are an important consideration in transfer behavior among wealthy people, though such incentives do not fully and consistently explain their behavior.

The study is organized as follows. In the next section, we describe household debt in the Netherlands and discuss 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 contains our conclusions.

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. In 2018 in particular, 55 percent of total mortgage debt was interest-only (IO), while the rest was either amortizing (20 percent) or with deferred amortization (25 percent). This debt was distributed across about seven 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 percent of all borrowers had exclusively IO loans; these types of loans are generally held by the oldest segment of the population (a segment that often uses these loans as a means to extract home equity). Approximately 50 percent of borrowers had a combination of IO loans with either annuity or saving loans, while the remaining 20

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

percent had no IO loans at all. As IO loans do not amortize, it is highly 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 such a transfer was used to make a mortgage prepayment, a down payment, or to finance a home improvement. The evaluation made by the Dutch Court of Audit (Algemene Rekenkamer 2017) shows that the new policy increased transfers for down payments too. So only estimating the effect on prepayments could have a downward bias on the overall effect of the policy on reducing mortgage debt in general, which was also an aim of the policy. The Court of Audit estimates that prepayments accounted for 74 percent of all transfers under the new policy and only 26 percent for the remaining purposes (down payments and home improvements). It is thus possible that fewer borrowers purchased their homes with LTVs exceeding 100 percent, but there is also a concern that the new policy might have induced home price inflation if the beneficiaries of transfers were the type of optimistic marginal buyers that overbid on scarce properties (Geanakoplos 2010). If this was the case, then the bias could actually have been upwards due to a higher level of debt among home buyers facing higher prices induced by overbidding. In view of this, along with the limited use made of these transfers for down payments, we speculate that the downward bias was limited. Nevertheless, the scope of our study is narrower and we do not focus on debt reduction in general. Rather, we focus on investigating whether there was an effect on the prepayments of those who already owned a mortgage at the time of the policy change. Thus, our primary focus is on prepayments, and we later also discuss transfers.

To make this policy possible, the government proposed an amendment to inheritance legislation. In order to avoid tax arbitrage, inheritances are taxed approximately in the same way as inter vivos gifts. This means that fiscal limits apply to intergenerational transfers. Before 2013Q4, borrowers under the age of 35 could receive 52,000 euro as a one-off, tax-exempt gift.[4] This was the institutional situation at the onset of the credit crisis in 2009 in the Netherlands, which eventually peaked in 2013Q1 after 4 years of declining house price.

Table 1 summarizes the relevant features of the policy changes described above in terms of the tax-exempt threshold, the sources of the transfer (e.g. parents), and the beneficiary's age in all relevant periods. In Section 4, we refine 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-exempt intergenerational transfers. The rest relate to programs that apply, for instance, to those financing higher education, inheriting a family business or

[4]Before the 1990s, inheritance taxes applied to any excess amount above the tax-exempt threshold (about 5,000 to 6,000 euro). However, when household indebtedness increased (along with higher house prices) in the 1990s, an additional 46,000 euro (one-off) lump-sum tax-exempt transfer was allowed.

TABLE 1
Overview of the Relevant Institutional Changes in Different Periods

Features of the policy		Period 1 Before 2013q3	Period 2 2013q4–2014q4	Period 3 2015q1–2016q4	Period 4 2017q1–2018q4
Maximum tax-free transfer	0–52 k	✓	✓	✓	✓
	52 k–100 k		✓		✓
Maximum age of the beneficiary	18–35	✓	✓	✓	✓
	36–40		✓	✓	✓
	>40		✓		
Donor	Parents	✓	✓	✓	✓
	Anyone		✓		✓

Notes: 2013Q3 to 2018Q4 are periods for which LLD is available. Transfers declared in Period 2 had to be transferred before 2016.

diverting funds from one tax-facilitated savings program 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 the policy changes and understood what they meant. Banks were asked to inform their customers directly about the policy changes through a variety of methods, including special news bulletins and a personal letter. This topic was also widely discussed in the media. There is also evidence that the number of people taking advantage of the tax exemption and the amount transferred increased during this period. Table A1 in Appendix S1 provides detailed information on these increases as summarized by the Dutch tax authorities for the Dutch Court of Audit. It is thus reasonable to assume that the public was aware of the relaxation of the tax-exempt policy when it was implemented.

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. 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 preloaded information into household the tax-forms, a practice of the tax authorities, using the same database used for the LLD. This means that the data source is checked (and corrected) yearly by households too, who approve or improve the pre-loaded information when submitting their tax returns. Thanks to the comparability of this information from DNB and the tax authorities, Statistics Netherlands (CBS) could merge a large part of the LLD with tax records. The LLD contains about 75 variables related to mortgages, e.g. the mortgage provider and servicer, the loan types, interest rates, borrower's participation in the national mortgage guarantee (a residual debt insurance), origination and maturity, and current property valuation. 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 with the same bank. We have information on prepayments in our data, 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, the LLD does not reveal the source of the money used for prepayments, e.g. intergenerational transfers, personal household savings, or some exogenous sources (such as lottery winnings or unexpected inheritances). Third, the LLD does not reveal the financial situation of a borrower's parents either.

We merge the LLD with transfers and inheritance files, parent's wealth information, and CBS income files from to overcome these three limitations. This merge also involves some limitations, however. We identify about three quarters of LLD borrowers, which is an impressive number. Only fiscally relevant transfers (i.e. above 6000 euro) are tax-relevant and thus available in the data, but many prepayments are lower. Finally, only parents that are residents of the Netherlands are present in the family links register, and the records of parents of older borrowers are often missing. We therefore restrict our sample to borrowers aged 31 to 45 to better ensure that the borrowers in our dataset are eligible for receiving observable intergenerational transfers. Also, in order to avoid unjustified selections, we merge the income and register data with the LLD and transfer data separately, as a complete merge would otherwise exclude about half of our sample, i.e. mostly younger and first-generation immigrants. Table A2 in Appendix S1 presents an illustration of the selection process steps that we apply to one of the waves of the LLD data for the different estimating samples. The table reveals that we lose most observations when we apply the age selection in the sample used for the baseline results.

3.2. *CBS data*

In order to perform a separate analysis on transfers and inheritances, we merge the yearly CBS income tax records with personal demographics information, transfers and inheritances files, household wealth, and CBS information on parental wealth. Table A2 in Appendix S1 also shows the selection process steps that we apply to the CBS data for the different estimating samples for transfers and inheritance analysis.

3.3. *Definition of 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 principal can be ascribed to prepayments, of course. Annuity loans, for instance, are contractually repaid each period at 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.

We therefore define prepayments as an irregularly large drop in the principal of borrowers that are observed for at least three 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 excluded. Table A2 in Appendix S1 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).

We note (see Figure 1) that from 2013Q4, at the time of the first reform, prepayments seem higher (left axis) and that their rate peaks at about 8 percent (right axis) in 2014Q4. After repealing the new policy (2015Q1), prepayments dropped to pre-policy levels. This was despite the fact that interest rates were lower in this period, which was found to positively affect prepayments (Li and Mastrogiacomo 2016). When the policy was reintroduced in 2017, we again observe a small increase in prepayments and a new peak in their rate in 2018Q4. The fourth 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 that is worth investigating whether the policy had a positive effect on prepayments. In Sections 4 and 5, we endeavor to identify how the changes in policy affected these trends.

In order to appreciate how relevant the reform was, we present descriptive statistics in Table 2 on transfers during the first reform period (2014) and in 2016, when the policy had returned approximately to the baseline. The table shows a donor-level data analysis. Evidently in 2016, the fifth exemption type (other donors) is not present, and the third only moderately (one-off transfers *parents to children*

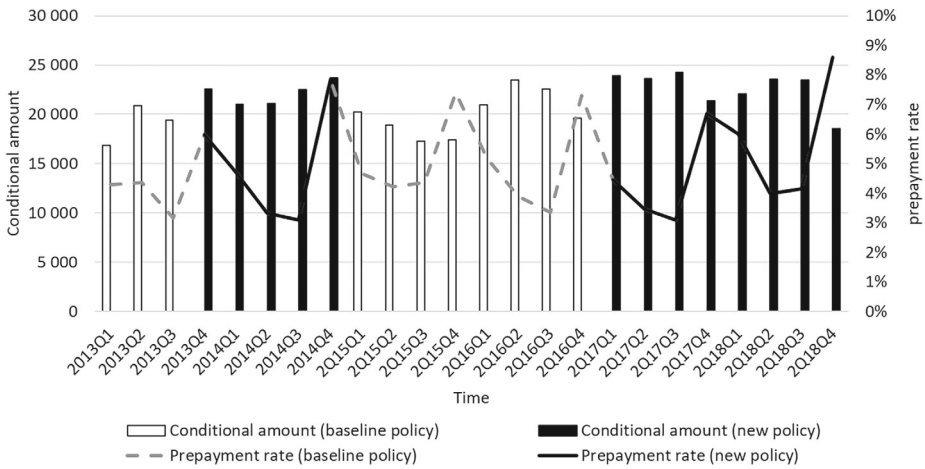

Figure 1. Prepayment Rate and Amount (Conditional on Making a Prepayment) with Baseline Policy and New Policy.
Note: No Data is Available Before 2013 or After 2018. DNB loan level data, own computations.

TABLE 2
Fiscally Relevant Transfers, Descriptive Statistics, Nominal Amounts

		2014			2016		
		Number of donations	Gross transfer		Number of donations	Gross transfer	
Type of exemption	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: The first reform period was in 2014, the second in 2017. CBS microdata, own computations.

could be done on paper in 2014 and completed within 2 years) while it was the most common transfer in 2014 (67 percent of all transfers). The median amounts do not differ much in any case.

In the first category (the regular baseline transfer), we observe transfers with a median of 50 k and a somewhat higher mean. Despite this category potentially also containing transfers to pay for education, these are typically transfers received by children aged over 40, to whom the regular exemption of 52,000 euro applies. This means that any excess is not tax-exempt. The same applies to the fourth category, but the tax-exempt threshold is then lower. The sixth category (business continuity) is only relevant for business owners, who can transfer a company with a net worth of up to 1mln euro to their children. All other types can instead be used for mortgage prepayments (though the second category could also be used to pay for education, but this is less common for individuals who already have a mortgage).

In Figure 2, we present the distribution of these transfers aimed at prepayments taken from CBS data and LLD separately. We have divided both distributions into bins, highlighting some relevant institutional thresholds. The first bandwidth stops at 6000 euro, for instance. 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 with no tax liability, so many households do not even report these to the tax office (though officially they should). The information campaign for the reform may also have encouraged smaller transfers. We also have bandwidths up to 52,000 euro and 100,000 euro, as these are the policy thresholds discussed above.

The figure shows that almost no transfers below 6 k are reported, while 25 percent of all prepayments are below this limit. These prepayments could still be reliant on transfers, but they are unlikely to be reported in the tax data. Most reported transfers are between 20 k and 52 k, while most prepayments are below

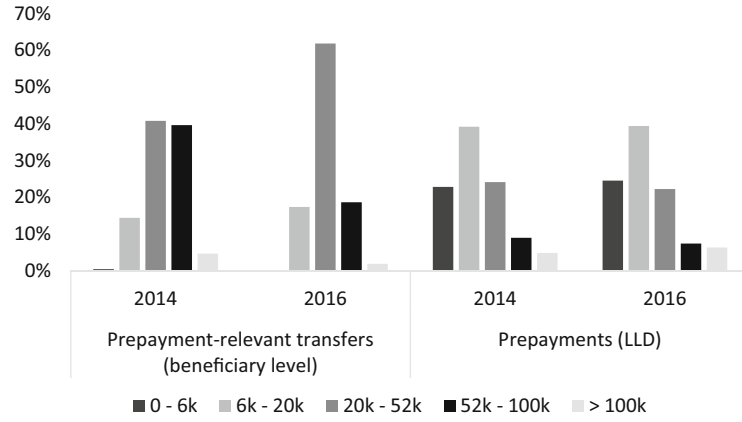

Figure 2. Distribution of Transfers and Prepayments by Policy-Relevant Thresholds.
Notes: Transfers are Prepayment-Relevant when they Belong to One of the Cases Described in Table 2. CBS Microdata and DNB Loan Level Data, own computations.

20 k. Transfers aimed at prepayments and actual prepayments are very different phenomena. This is most evident when considering that, once the new policy was repealed in 2016, transfers in the category 52 k–100 k dropped by 50 percent, while prepayments in the same bandwidth stayed the same. Thus eliminating the higher thresholds of 52 k or 100 k is unlikely to affect most prepayments, which are already below the 6 k threshold. Because transfers and prepayments are so different, we analyze them separately elsewhere in this paper.

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 (Euribor) is lower under the new policy, which could result in more prepayments. For other variables, there is little difference across all periods.

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 the probability of receiving a transfer and the amount transferred are higher under the new policy. The value of a 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 information campaign (see discussion about Figure 2) was aimed at encouraging transfers for prepayments, but could also encourage a non-reported transfer (below the legal

TABLE 3
SUMMARY STATISTICS OF LLD QUARTERLY MORTGAGE DATA FROM 2013Q1 TO 2018Q4

	Baseline policy periods			New policy periods		
Variables	Mean	s.d	*N*	Mean	s.d	*N*
Outcomes						
Indicator for prepayment	0.050	0.22	3859500	0.052	0.22	3711069
Mean of prepayment (if prepayment >0)	21043	33243	182182	23142	36739	187229
Demographics						
Age	38.40	4.29	3859500	38.53	4.25	3801013
Economic related						
Macro interest rate Euribor_t	−0.03	0.17	3859500	−0.08	0.27	3711069
r_{it}—Euribor_t	−4.51	0.80	3844689	−4.56	0.80	3711069
Household income at mortgage origination	64684	30040	3844689	63916	29441	3711069
Wage-employed at mortgage origination	0.69	0.46	3844689	0.69	0.46	3711069
Self-employed at mortgage origination	0.06	0.24	3844689	0.06	0.24	3711069
Other employment status at mortgage origination	0.01	0.11	3844689	0.01	0.10	3711069
Employment status missing at mortgage origination	0.22	0.42	3844689	0.20	0.40	3711069
Log of parents' wealth	5.33	5.14	3844689	5.52	5.14	3711069
Indicator for receiving transfers	0.00	0.05	3844689	0.00	0.07	3711069
Mortgage characteristics						
Reporting year—loan origination year	5.66	3.42	3844689	5.56	3.30	3711069
Year of maturity—reporting year	33.55	11.48	3844689	34.29	11.46	3711069
NHG indicator	0.52	0.50	3844689	0.55	0.50	3711069
Share of annuity or linear loan in a mortgage	0.13	0.29	3844689	0.18	0.34	3711069
Share of IO loan in a mortgage	0.38	0.32	3844689	0.36	0.32	3711069
Share of saving insurance loan in a mortgage	0.42	0.38	3844689	0.40	0.38	3711069
Share of investment loan in a mortgage	0.05	0.20	3844689	0.04	0.18	3711069
Share of other type loan in a mortgage	0.02	0.12	3844689	0.02	0.11	3711069
Current property value	242057	130391	3853416	252984	134320	3794996

Notes: NHG is short for National Mortgage Guarantee (in Dutch: Nationale Hypotheek Garantie). If the borrowers' situation changes for certain reasons (such as unemployment or getting divorced), the NHG provides a guarantee for repayment to the mortgage lender. Mean values reported. r_{it} = Mortgage interest rate at reporting quarter. Reporting year—loan origination year is weighted by loan type. Year of maturity—reporting year is the max of each loan type. DNB loan level data and CBS microdata, own computations.

TABLE 4
SUMMARY STATISTICS OF CBS YEARLY TRANSFER DATA FROM 2012 TO 2018

	Baseline policy periods			New policy periods		
	Age groups			Age groups		
Variables	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	0.5	0.51	0.51	0.5
Number of parent(s)	1.7	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	0.1	0.12	0.09	0.1	0.12
Indictor for unemployed	0.02	0.02	0.02	0.01	0.02	0.02
Indictor for retirement	0	0	0.01	0	0	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	1.7	1.61	1.48	1.69	1.61	1.49
Mortgage characteristics						
Indicator for having underwater mortgage	0.71	0.63	0.46	0.5	0.51	0.42
Number of observations	2384138	2711909	3261632	1749616	1960435	2208584

Note: Mean values reported. CBS microdata, own computations.

threshold for taxation). Before presenting the empirical strategy, we summarize the effects of the baseline and the new policy on different age groups in different periods in Table 5, where we refine the information from 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 do not involve the policy change, but the comparison of different groups differentially affected by the given institutional design. We use the change in policy over adjacent periods for the identification of the effect of the new policy instead.

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

We use within-period variation across age groups to identify the effects of the baseline policy. Our primary strategy is to use a regression discontinuity design for the period 2012Q4–2013Q3 and 2015Q1–2016Q4 for the prepayments (based on LLD) and 2012–2013 and 2015–2016 for the transfers (based on CBS data). Age group dummies thus capture the baseline policy effect, and we do not need to check for common trends. The age effect itself is picked up by other variables (e.g. indicator for being a student) that are age-related. We compare the difference in four outcome measures (prepayment and transfers events, and the amounts prepaid and transferred) across two adjacent age groups. As indicated 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 with colors, where dark grey cells with underlined prints contain those falling within the baseline policy (treated) and the light grey cells with underlined prints indicate 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.

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

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,

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

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 Equations (1) and (2), y_{it} is initially 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). Note that we do

TABLE 5
OVERVIEW OF THE IDENTIFICATION STRATEGY FOR EFFECT OF POLICY CHANGES

		Identification effect, baseline policy						Identification effect, new policy					
		(1) Period 1	(2) Period 2	(3) Period 3	(4) Period 4			(5) Period 1	(6) Period 2	(7) Period 3	(8) Period 4		
		Baseline Policy 1 Before 2013Q3	*New Policy 1 2013Q4-* 2014Q4	*Baseline Policy 2 2015Q1-* 2016Q4	*New Policy 2 2017Q1-* 2018Q4			*Baseline Policy 1 Before* 2013Q3	*New Policy 1 2013Q4-* 2014Q4	*Baseline Policy 2 2015Q1-* 2016Q4	*New Policy 2 2017Q1-* 2018Q4		
Age 31–35	Tax exemption	**52 k**	52 k + 48 k	52 k	52 k + 48 k	Age 31–35	Tax exemption	52 k	52 k + 48 k	52 k	52 k + 48 k		
	Source	**Parents**	all	parents	all		Source	Parents	All	Parents	All		
Age 36–40	Tax exemption	**None**	52 k + 48 k	**52 k**	52 k + 48 k	Age 36–40	Tax exemption	None	52 k + 48 k	52 k	52 k + 48 k		
	Source		all	**Parents**	all		Source		All	Parents	All		
Age 41–45	Tax exemption	None	52 k + 48 k	**None**	none	Age 41–45	Tax exemption	None	52 k + 48 k	None	None		
	Source						Source						

Notes: The dark-shaded areas identify the group subject to the baseline and new policy, the light-shaded areas identify those that are not subject to it. 52 K and 48 K indicate the 52,000 and 48,000 euro tax-exempt thresholds, respectively.

not estimate a model in which transfers explain prepayments directly due to the limitations of the dataset merge explained above.[5]

During the observation period, the interest rate paid on saving accounts dropped sharply and reached unprecedented low levels (even negative). We use quarterly (for analyzing prepayments in LLD) or yearly (for analyzing 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, along with seasonal dummies for seasonal effects (only for analyzing prepayments as CBS data are collected yearly), are included in $\boldsymbol{Z}_t$. The declining interest rate also makes alternative financial investments less attractive and prepayments more attractive, as the interest rate on mortgages is generally higher. Such an interest rate arbitrage opportunity is controlled for in the empirical specification in the matrix $\boldsymbol{X}_{it}$ (only for analyzing prepayments using LLD). The $\boldsymbol{X}_{it}$ matrix also contains 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.

4.2. *New policy (additional 48,000 euro tax-exempt, 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 are always in addition to the effects of the standard exemption, and there is no age group that 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. We can then compare the difference in outcomes between the baseline policy period and the new policy period.

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

[5]We have tested the possibility of analyzing the effect of receiving transfers on mortgage prepayments (using tax exemption eligibility as an instrument). Results (not presented here) were biased by a significant drop in sample size, mostly among younger cohorts, and in most cases by a weak instrument problem.

aged 36–40, we look at outcomes in period 4 (corresponds to right dark grey cell in Table 5), and we compare them to those in period 3 (corresponds to right light grey cell 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 primary interest is to estimate β_2^j, which captures the causal effects of the new policy:

$$y_{it} = \beta_0 + \beta_2^j * Treatment_t + \gamma * \boldsymbol{Z}_t + \rho * \boldsymbol{X}_{it} + \varepsilon_{it}. \tag{3}$$

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) based on specific age-related subsamples. For $j = 1$, 2, and 3, the sample contains those aged 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,[6] while the full results are presented in Table A3 in Appendix S1. The left panel looks at quarterly prepayments, the right panel at yearly transfers. Panel A reports the effects of the baseline policy, and Panel B reports the effects of the new policy. In both panels we report the analysis of prepayments on the left and transfers on the right. For each, we look at the prepayment or transfer rate (white section) and amounts (grey section). On the left, we summarize results for models explaining the probability of making a prepayment (models C1, C2, C9, C10, C11), while in models C3, C4, C12, C13, and C14 we look at the prepayment amount. Models C5, C6, C15, C16, and C17 explain the probability of receiving a transfer, and models C7, C8, C18, C19, and C20 the transferred amount.

Before we discuss the main results, note that we only report the coefficient of interest, β_1 and β_2. In all regressions, we include all controls as discussed in Section 3. To briefly comment on these variables, we note (see Table A3 in the Appendix S1) that the prepayment rate is positively related to income in general, 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

[6]Despite our panel data, we cannot estimate a fixed effect model in most cases because our identification exploits discontinuity across a group within a given period. This period is often too short to allow multiple observations (the first period of the baseline policy only covers one full year, for instance, and in a few cases we have only two years of data). However, even in these cases transfers can only be received once. Conversely, prepayments can be repeated. Results with individual fixed effects confirm our results for the amount prepaid, while the effect on the prepayment rate becomes either insignificant 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.**	0%	0%	0.0%	0.0%
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	−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	0.0002	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	0.0%	0.0%	**Not sig.**	0.0%	**Not sig.**	0.0%	0%	0%	0%	0.0%	0.0%	0.0%
N	798276	1405212	1683337	30143	52844	77700	1814687	2318849	2071344	23436	11930	14523

Notes: 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. DNB loan level data and CBS microdata, own computations.

are more likely to have bullet loans. Conversely, the probability of receiving a transfer and the transfer amount are positively related to age, being male, being either self-employed or retired, or being relatively more financially independent, while it is negatively related to being currently underwater.

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 households (e.g. aged 31–35) might have already benefited from transfers for a down payment, and their parents now either have less available funds or have already transferred the maximum amount (the one-off rule means that an individual may no longer qualify). Instead, the baseline policy contributed to increasing the probability of a transfer (see models C5 and C6). In the 2012–2013 sample period, comparing ages 31–35 to ages 36–40, the results suggest an increase of 56 percent. In comparison, the causal effect of the policy is around three times larger if we use the 2015–2016 sample period and compare ages 36–40 to ages 41–45. These effects appear relatively large, while the absolute transfer rate is very low (in the order of magnitude of 0.23 percent). 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 the marginal transfer amounts are lower for households that receive transfers. The results from models C7 and C8 show that those affected by the baseline policy received about 3200 and 2000 euro less in transfers, respectively, notwithstanding whether we identify the effect using the different age groups in different periods. This can be explained in various ways. It is possible, for instance, that relatively "marginal" households started transferring too, but that they transferred lower amounts; alternatively this could also depend on the cumulative nature of the new policy for households that had already received transfers under the baseline policy.

Panel B shows the effect of the new policy on top of the baseline policy ($\beta_2^j, j = 1, 2, 3$).[7] Raising the tax-exempt threshold from 52,000 euro to 100,000 euro with additional sourcing in periods 2 and 4 increased the probability of making a 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 percent and 7.6 percent increase in the prepayment rate, respectively. For older borrowers (Model C11) we found no significant effect. 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

[7]Other control variables are reported in Table A3 and A4 in Appendix S1. For prepayments, for instance, 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 combined effects parents' wealth as observed in the datasets and to the number of parents who are alive.

18,662 euro, the first introduction of the new policy in period 2 induced a 14.3 percent increase of the prepaid amount.[8]

The new policy also resulted in an increase in the transfer rates and transferred amounts for all age groups; the first and second activation of the new policy increased transfer rates by 0.97, 0.15, and 1.08 percentage points for the three age groups. In relative terms these increases amount to 76 percent, 30 percent, and 84 percent relative to the mean of the dependent variable; the number is small, however, as the probability of a transfer is low in general. Transferred amounts increased by 5955, 14133, and 12832 euro for the three age groups.[9]

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 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. Moreover, the policy, particularly in the second period, was pre-scheduled and therefore independent from the business cycle at the time of implementation. This means that we can compute 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 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.

The simulated distribution of the share of underwater mortgages by age and the counterfactual distribution are presented in Figure 3. The share of the underwater mortgages decreases with age (due to the combined effect of amortization and price increases). There is no discernible difference between the simulated and counterfactual distributions, however. This indicates that the new policy has no effect on reducing the share of underwater mortgages (the mean difference between the two lines is 0.21 percent points).

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

[8]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 of making a prepayment and the conditional amount. 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 its size are positively related to the combined effects parents' wealth as observed in the datasets and to the number of parents who are alive.

[9]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 transferred are positively related to the number of parents and the log of parents' wealth. Self-employed children are more likely to receive transfers than wage-employed children.

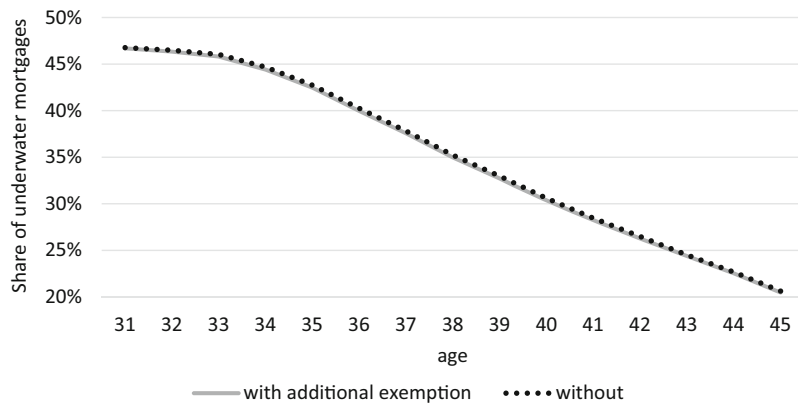

Figure 3. Micro Simulations for Scenario with Additional Exemption and Without Additional Exemption by Age.
Notes: The Additional Exemption is for Transfers up to 100,000 Euro. DNB Loan Level Data, own computations.

TABLE 7
Regression Results by Original LTV Category

Relevant statistic	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

Note: Avg, Average, prep, prepayment. DNB loan level data and CBS microdata, own computations.

policy by original LTV. We capture this by identifying two original LTV categories, less than or equal to 100 percent and greater than 100 percent, that we also multiply by $Treatment_t$ in Equation (3). This means replicating the specifications in column (1) and (3) of panel B in Table 6 for two different original LTV categories.

Based on these model estimates, we perform again a prediction with counterfactual as we did above, and report the results in Table 7.

For the original LTV categories (≤ 100 percent), 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 percent of remaining debt for non-underwater mortgages. Similarly, the simulation shows that the reduction was 0.03 percent of remaining debt for underwater mortgages. This inverse relation between LTV and prepayments is also suggested by the covariates in Table A3 in Appendix S1, where we cannot correct for LTV directly as loans and property values are already present in the model (either in the dependent variable or as regressor). For instance, we observe a positive effect from income, but this is negatively related to the LTV (high income borrowers are typically older and/or have more savings and thus lower LTV ratios). Overall, this implies that debt reductions were larger for debtors that originated above water, and that the share of 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.

6. Conclusions

Intergenerational transfers in the Netherlands are a relatively rare event compared to mortgage prepayments. Nevertheless, the Dutch government amended policy to fiscally facilitate the former in order to incentivize the latter. From 2013 to 2018, the government twice modified 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 dropped, and anyone could donate), we observe 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 percent and 7 percent increase in the probability of making a prepayment, and a 75 percent and 30 percent increase in the probability of receiving a transfer. The first policy change also increased the amounts prepaid by 14 percent and the amounts being transferred by 12 percent, while the second policy change only increased the amounts being transferred by 44 percent. There are different possible reasons for the second policy change having no effect on prepayments. The policy was cumulative: once used in 2014 it could not be re-used in 2017. However, prepayments are far more common than transfers. As the policy was heavily publicized as a way to recover from the housing crisis that had left 36 percent of homeowners with an underwater mortgage, it could have had the (possibly unintended) effect of promoting prepayments as something advisable and inherently responsible in a period of financial instability. This might in turn have had a multiplier effect on prepayments even in the absence of transfers. The effects of the first policy change did not dissipate, and the second policy change, that was enacted in 2017 and 2018 (when underwater mortgages ranged between 15 percent and 5 percent), did little to further boost the program's popularity. These new fiscal policies were meant to reduce the existing number of underwater mortgages, a risk that had mostly vanished by 2018. We find that the prepayments motivated by the policy change increased most 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 is not at all surprising, as a policy that raises the tax-exempt threshold will primarily benefit relatively affluent parents, who presumably are more likely to have relatively affluent children. Moreover, the initial threshold is unlikely to be a factor of any significance for relatively poor parents, who subsequently remain indifferent when the threshold is raised. This suggests that policy-makers might want to target these types of policies more precisely, aiming them more at the envisaged group of highly indebted households. Generic wealth-tax measures do not automatically work in favor of the envisaged target groups.

Our findings are in line with related literature that shows that tax incentives are an important consideration in the transfer behavior of the rich, though such incentives do not fully and consistently explain their behavior (Joulfaian and McGarry 2004). We find a weak link between the fiscal incentive and prepayments that is consistent with US evidence where tax-facilitated intergenerational transfers also tend to be lower than the level that intertemporal models suggest (Poterba 2001). This is also consistent with the view that the lack of a strong policy effect could partly be due to tax design (Advani and Tarrant 2021).

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Supporting Information

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Appendix S1. Supplementary Information.