Bidder Beware: Intergenerational Wealth Transfers in the Residential Housing Market

Jaap Bos, Nils Kok, and Jonas Wogh*

Maastricht University

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Abstract

Using an unanticipated tax-exemption policy in the Netherlands, we study the effect of wealth transfers on housing market outcomes. We find that buyers who receive a wealth transfer purchase homes that are 20-35% more expensive, and overpay for a given home by 0.5-2.0%, relative to other buyers. Overpayment is driven by the tightness of the local market in which a home is bought. At the local market level, the policy increases prices due to a spillover effect, whereby home buyers who did not receive a transfer similarly raise their bids in response to an influx of wealth transfer recipients.

JEL Codes: H31, R21, R28, R38

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*n.kok@maastrichtuniversity.nl (corresponding author)

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1 Introduction

After more than a decade of rapid house price growth in most developed economies, the issue of housing affordability has once again gained attention (Favilukis et al., 2023). In the owner-occupied sector, the situation of young households with limited savings is particularly worrisome. This has led policymakers across jurisdictions to design a variety of schemes supporting first-time homebuyers, ranging from shared-equity mortgages to outright cash grants.[1] Against this backdrop, the role of family wealth in housing markets has come under increased scrutiny. According to a recent report from Redfin, 36% of young homebuyers in the United States rely on family wealth transfers or inheritances to finance their down payment on a home (Redfin, 2024). This trend could exacerbate existing wealth inequalities by limiting access to homeownership for young households lacking familial wealth.

Following increased availability of data on family network links, economists have recently begun to study the role of family wealth in housing transactions. For instance, Benetton et al. (2024) find that parents extract home equity to help their children make a purchase. Brandsaas (2021) shows that parental transfers are important for relaxing borrowing constraints, and account for almost a third of the homeownership rate among young adults. What is missing, thus far, is evidence on how wealth transfers affect housing market outcomes. This study aims to fill the research gap, using granular administrative data from the Netherlands in conjunction with a

[1] For instance, US presidential candidate Kamala Harris has suggested a $25,000 cash grant for first-time homebuyers as part of her economic agenda (Bloomberg, 2024)

quasi-experimental policy that facilitated housing-related wealth transfers. We begin by studying how the receipt of a financial transfer affects bidding behavior and transaction outcomes. Using a variety of ways to match buyers who receive a transfer with those who do not, we find that find that transfer recipients pay higher prices for a given property, holding the list price constant. To address endogeneity concerns, we devise an instrumental variable strategy based on the amount of liquid assets that a buyer's parents hold in their portfolio. Depending on the specification, we estimate that transfer recipients pay 0.5% - 2.0% higher prices for the same home, compared to other buyers. This effect does not seem to be driven by liquidity constraints of the buyer or by the type of home that is purchased. Instead, we document that transfer-induced overbidding is most pronounced in local markets that are relatively tight to begin with.

Next, we investigate how an influx of wealth transfer recipients affects prices in local housing markets. We rely on variation in exposure to the tax-exemption policy across Dutch postcodes. In a difference-in-differences setting, we compare postcodes in which a larger fraction of buyers received transfers with those areas in which fewer buyers received a wealth transfer. We find that, while largely trending parallel prior to the policy, average bids made by non-recipients increase sharply shortly after the introduction of the policy. This suggests that untreated buyers adapt their bidding behavior in response to competition from wealth transfer recipients. Given that the treatment variable is assigned ex post and subject to endogeneity concerns, we run two additional tests, which support the interpretation of our findings. First, we

show that the effect size is increasing monotonically: prices increase most in those postcodes where the share of transfer recipients is the largest. Second, we argue that our findings do not simply reflect the fact that transfer recipients select into gentrifying markets: using tax assessed values as a placebo outcome, we do not find a significant effect of increased exposure to the policy.

Our work contributes to two strands of the literature. First, it adds directly to the literature on family wealth in housing markets. Charles and Hurst (2003), among the first to study inter-generational wealth patterns, document that the children of homeowning parents are much more likely to be homeowners themselves. Early research from Engelhardt and Mayer (1998) shows that wealth transfers allow households to make purchases earlier in their life-cycle, by reducing the required time to save for down payments. More recent empirical studies show that wealth transfers also have a sizable impact on the probability of becoming a homeowner. Using Swiss data, Blickle and Brown (2019) find that a wealth transfer (of any size) increases the transition rate into home ownership (among renters) by 35%. Similarly, Brandsaas (2021) calibrates an overlapping-generations model to data from the United States and finds that wealth transfers account for 31% of the home ownership rate among young adults. Benetton et al. (2024) study the underlying mechanism driving inter-generational transfers and find that they are often preceded by home equity extraction from parents. They show, therefore, that financial transfers are an important driver behind the observed inter-generational persistence in housing wealth.

To our knowledge, no study has yet examined the repercussions that

housing-related wealth transfers have on market outcomes. However, a number of closely related articles have identified a positive link between buyer wealth and house prices. For instance, Aiello et al. (2024) find that repeat buyers who make a gain on selling their previous home tend to overbid when buying the next home. They also find that an inflow of wealthy repeat buyers can drive up prices at the neighborhood level. Cvijanović and Spaenjers (2021), using transaction data from Paris, find that that wealthy "out-of-town" buyers make systematically higher bids than others, and that their inflow has a small, but positive effect on neighborhood prices.

Second, our finding of spillovers to untreated home buyers is in line with a growing literature on spillover effects and social interaction in real estate markets. Burnside et al. (2016) borrow from the epidemiology literature to model social interactions between buyers and the transmission of different beliefs about about housing fundamentals. Using social network data from Facebook, Bailey et al. (2018) find a strong effect of peers in residential housing markets: recent experiences of friends and relatives directly influence purchasing decisions, both at the extensive and intensive margin. More closely related to our work, a number of recent papers examine information spillovers across agents within local housing markets. Gupta (2019) shows that foreclosure probabilities increase due to other foreclosures in close proximity. In a similar spirit, McCartney and Shah (2022) establish that refinancing decisions are affected by the activities of close neighbors via the word-of-mouth channel, and Bayer et al. (2021) find that even real estate *investment* decisions are based on the activity of close neighbors. We add another dimension to this

literature, by showing that bidding behavior in residential transactions can also be affected by the activity of other buyers in a local market.

The findings in this paper have some important policy implications. At a time when housing prices are high and getting on the "housing ladder" is hard, policymakers are inclined to resort to subsidies, especially for the young. However, such subsidies are at least partially absorbed into higher house prices for *all* market participants. While this benefits existing homeowners, it hurts those who are supported in the first place - first time homebuyers. In addition, there are distributional effects in providing tax-incentives for intergenerational wealth transfers, potentially further increasing inequality in society.

2 Institutional Setting and Policy Design

Comparable to the United States, the Dutch residential housing market is characterized by high ownership rates, which at 70% in 2022 are substantially higher than most other Western European countries. Another commonality with the U.S. is the co-existence of supply-constrained "superstar cities," such as Amsterdam and Utrecht, and rural areas where recent price growth has been more muted. The most distinctive feature of the Dutch market is a very strong dependence on mortgage finance, which is often explained by generous tax deduction rules for mortgage loans and the general absence of down-payment requirements (see Bernstein and Koudijs (2024), for a discussion of the Dutch mortgage market).

2.1 Stabilizing Policies after the Financial Crisis

As in most other European countries, a period of strong real house price growth in the Dutch market since the 1990s ended abruptly in the wake of the global financial crisis in 2008. However, while house values across the European Union began to recover after 2011, the Netherlands experienced a second downtrend that lasted until 2013. Rouwendal and Petrat (2022) argue that this prolonged slump was due to a tightening of mortgage underwriting rules at the beginning of 2011. Real house prices fell by more than 30% in the period from 2008 to 2013, whereas the annual transaction volume dropped from more than 200,000 transactions in 2008 to around 100,000 transactions in 2013 (Boelhouwer, 2017).

In the aftermath of the financial crisis, the Dutch government initiated a number of policy changes in the housing market. These were predominantly macro-prudential measures to improve stability. For example, eligibility criteria for the national mortgage insurance scheme (NHG) were tightened. The maximum purchase price to qualify for the insurance was gradually lowered from €350,000 in 2011 to €290,000 by 2013, causing the number of new loans covered by the scheme to drop by more than 20%. Another important policy change involved an update to the tax-deduction criteria for mortgage payments. From 2013 onward, only interest payments on fully-amortizing mortgages remained tax deductible, which largely eliminated the market for interest-only and savings mortgages (Bernstein and Koudijs, 2024). Finally, in 2012, a statutory limit on loan-to-value (LTV) ratios was introduced. This limit, which was initially set at 106% and subsequently lowered to 100% in

2018, reduced (but did not eliminate) the potential problem of negative home equity in downward markets. At the same time, however, it was another tightening measure that curbed demand and contributed to the prolonged fall of house prices, given that buyers commonly used their mortgages to finance not only the property, but also transaction costs, such as notary fees, transfer taxes, and brokerage fees (Biesenbeek et al., 2022).

2.2 The *Jubelton* Policy for Tax-exempt Transfers

Following the restrictive macro-prudential measures, the government eventually also took policy steps to stimulate housing demand. In 2011, the transfer tax was lowered from 6% to 2%, resulting in a significant reduction of effective purchase prices, given that this tax is levied on buyers. While initially planned as a temporary measure to be phased out in 2012, the reduced rates continued to stay in place for almost ten years.[2] Nevertheless, transaction volumes remained low and prices continued to fall after 2011, with the reduction of the transfer tax missing its intended purpose (Solinge and Fiedler, 2020).

Whereas the reduction in transfer tax directly lowered purchase prices, a second policy measure, introduced in 2013, was aimed at stimulating home ownership and reducing indebtedness among young and first-time home buyers by facilitating *inter-vivos* transfers. In particular, this policy offered a generous tax-exemption for financial gifts related to housing expenditures. Figure

[2]In 2021, two changes to the flat 2% tax rate were introduced. First, for young, first-time home buyers, transfer taxes were fully eliminated. Secondly, for recreational homes, commercial real estate or investment properties, they were increased to 10.4%.

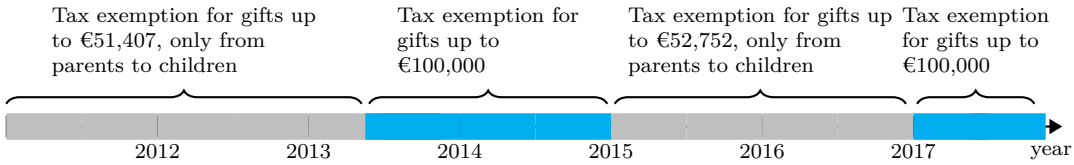


Figure 1: Timeline of the *Jubelton* Policy

1 sketches the timeline of events. Before 2013, the annual tax-exemption threshold for financial gifts was €2,057, with an extended threshold of €5,141 for gifts from parents to children. However, an exemption threshold of €51,407 was in place for one-time transfers from parents to children, conditional upon usage for housing-related expenditures. On September 17, 2013, the government announced a change to the extended threshold along two dimensions. First, it was raised to €100,000, and second, gifts could be made by everyone, regardless of the relationship between donor and recipient.[3] Moreover, it was communicated that the policy would be in place for a limited time period – between October 31, 2013 and December 31, 2014.

The policy had a high take-up rate and received broad media coverage. Overall, more than 150,000 housing-related transfers were made during the time that the policy was in place. This translated into forgone tax revenue of about €1 billion, around 0.5% of the Dutch government's annual tax revenue at the time (Statistics Netherlands, 2019).[4] For our analysis, we identify all

[3]This fiscal policy has become known as the *"Jubelton"*, Dutch for a "celebration of one hundred thousand". Given that gifts are taxed at 10-30%, depending on the relationship between donor and recipient, the policy offered substantial tax relief for not just inter-generational transfers, but also transfers between unrelated individuals.

[4]The total sum of transfers related to the *Jubelton* policy was €9.16 billion in 2013 and 2014 combined.

people who received a financial transfer under the extended tax-exemption threshold for housing expenditures in 2013 and 2014. This group includes a total of 87,000 gift recipients. Based on our available data, we estimate that around 65% of the recipients used the funds to pay off existing mortgages. In this paper, we focus instead on the smaller group of recipients using the gift to finance the purchase of a home. A third group of beneficiaries invested the money in upkeep or improvement of their existing home, or to acquire ground lease rights. The vast support for the *Jubelton* prompted the government to re-introduce the policy in 2017 in a slightly adapted form, whereby gift recipients had to be between 18 and 40 years old. However, take-up rates for the new scheme, which was ultimately phased out in 2023, never reached the original levels. For instance, in 2017 and 2018, just 24,000 and 10,000 transfers were made, respectively (Statistics Netherlands, 2021).

3 Identification Strategy

Since the *Jubelton* policy triggered a large number of financial transfers earmarked for housing expenditures, it offers the ideal laboratory to study the effect of wealth transfers on transaction outcomes. Of course, the group of transfer recipients represents a highly selected sample of the total population of buyers, an issue that we will address through a combination of matching and instrumental variables. In order to analyze market-level effects, we will rely on variation in exposure to the policy across postcodes.

3.1 Data Sources

The analysis in this paper draws on data from two different sources. We use a comprehensive data set on owner-occupied, residential real estate transactions from the largest Dutch real estate broker association (NVM). This data set covers more than 70% of all sales in the Netherlands and has been used extensively for research purposes (e.g., Aydin et al. (2020); Dröes and Koster (2016); Leib et al. (2021)). For each property that is transacted, it contains a large set of structural characteristics, reported by the listing agent. More importantly for this study, the data set reports both the price at which a house was initially listed, as well as the final sale price. The ratio between these two, which we refer to as "spread", is the main outcome of interest in our empirical analysis.

We link the transaction data with a number of administrative data sets from Statistics Netherlands (CBS). We begin by identifying, for each transaction from January 2012 until December 2014, the main buyer, based on ownership records that are reported annually for property taxation purposes. Then, we identify each buyer's parents, based on a network data set that includes all links between family members. Finally, we add buyer demographics, household-level income, and wealth information for buyers and their parents, measured in 2012. The wealth data is particularly detailed, as it reports not only total net wealth, but also a breakdown into its different components, including financial assets, bank balances, home equity and mortgage debt. This allows us to compare, through a matching exercise, transfer recipients to other buyers based on their financial situation prior

to purchase. Finally, we add information on all financial transfers that a household received in a given year. Importantly, this includes both gross and net gifted amounts, as well as information on the type of transfer. This detailed information allows us to cleanly identify transfers that were made under the *Jubelton* policy. Table 1 reports key summary statistics for all buyers, depending on whether they received a tax-exempt transfer or not. While recipients and non-recipients are relatively equal in terms of disposable household income and age at the time of purchase, they differ greatly in terms of wealth. The median net worth among treated buyers (prior to receiving the transfer) is more than three times larger than that of control buyers. This is predominantly driven by differences in liquid assets (bank balances, stocks, and bonds) and lower levels of debt.

Table 1: Summary Statistics: Treated vs. Untreated Buyers

	Regular Buyers			Transfer Recipients		
	Mean	Med.	SD	Mean	Med.	SD
Buyer						
Disp. Income (€1,000)	52.14	46.67	34.29	56.42	45.74	57.54
Net Wealth (€1,000)	147.26	26.85	365.94	393.50	98.52	704.39
Total Assets (€1,000)	289.40	199.94	450.14	545.92	260.96	803.02
Liquid Assets (€1,000)	52.29	16.53	13.50	157.26	44.24	335.01
Real Estate (€1,000)	191.16	171.04	240.98	256.91	190.79	256.91
Mortgage Debt (€1,000)	199.94	176.36	127.73	149.87	119.35	150.21
Age (years)	35.63	33.00	10.39	35.39	34.00	10.28
Property						
List Price (€1,000)	237.66	199.50	144.20	291.90	245.00	175.51
Sale Price (€1,000)	224.54	190.00	132.65	275.62	232.50	160.31
Floor Space (m^2)	115.04	110.00	43.54	119.23	115.00	51.86
Number of Buyers		99,183			5,026	

Notes: All income and wealth variables are reported at the household level and measured before *Jubelton* transfers were received. Total assets are the sum of all assets a household owns. Liquid assets comprise bank balances, stocks and bonds. Real estate assets are assessed at their taxation value and include both owner-occupied and other (investment) properties.

3.2 Matching Process

Our empirical analysis is based on a comparison of home buyers who received a tax-exempt financial transfer (*treated buyers*) with those who did not (*untreated buyers*). The challenge to identification in this context arises from the fact that treated buyers are not selected at random from the homebuyer population. To address this concern, we implement two different matching strategies, based on buyer characteristics. In addition, we deal with endogeneity concerns by implementing an instrumental variable strategy based on the share of liquid assets in the buyer's parents wealth portfolio. Details of the IV approach are described in the next subsection. We begin here by laying out the matching procedure.

The purpose of matching is twofold. First, it ensures comparability between treated and untreated buyers in observable characteristics, without assuming a functional form for the conditional expectation function. Second, it restricts the analysis to an area of common support. That is, it retains only those treated buyers for which comparable buyers exist in the untreated group, and vice versa. This entails a bias-variance trade-off. On the one hand, more granular matching reduces model dependence, because it does not require extrapolation for observations outside the area of common support. On the other hand, it reduces the sample size and hence the precision of the estimate. What works in our favor is that our data set includes a large number of households that can potentially serve as control observations, since they did not receive a transfer under the *Jubelton* policy. That means we can be very selective when matching treated households with untreated households, in

order to alleviate concerns about selection bias as much as possible.

The starting point for the matching procedure is the full data set of all 104,209 transactions taking place between November 2013 and December 2014, including 5,026 treated and 99,183 untreated buyers. We start by removing all buyers with recorded net wealth above €5 million or net debt above €1 million, and those with disposable household income above €1 million or below 0. In addition, observations are removed if the buyer's parents are missing in the data set, either because they are deceased or because they do not live in the Netherlands. This is a first step in improving comparability among buyers, given that the vast majority of transfers were made from parents to their children.

We apply two separate matching strategies to the remaining sample. First, we perform nearest neighbor matching based on propensity scores. To that end, we model the probability of receiving treatment as a linear function of buyer age, net wealth, total assets, disposable income and prior homeownership. Then, a greedy matching algorithm pairs each treated buyer with exactly one untreated buyer that is most comparable. The resulting sample contains 10,052 observations and is perfectly balanced in treatment and control observations.

In addition, we perform coarsened exact matching, as proposed by Iacus et al. (2012). This method is more flexible than exact matching, since it allows the researcher to define strata for each matching variable. In our case, we consider two observations matched if they are in the same decile of the distribution of net wealth, total assets, disposable income and age. Additionally, they are

matched on their previous homeownership status. Conversely, observations are removed if no match exists within the other group. In the estimation, treated and untreated buyers within the same stratum are compared by including matching-cell fixed effects. The resulting matched sample comprises 63,777 observations. Most treated buyers are retained, whereas almost two-thirds of untreated buyers are excluded due to lack of an adequate match.

3.3 Empirical Strategy

Based on our matched samples, we estimate the effect of wealth transfers on transaction outcomes via the following linear regression:[5]

$$Y_i = \beta \cdot Transfer_i + \phi \cdot X_i + \lambda_j + \epsilon_i \tag{1}$$

where $Transfer_i$ equals 1 if the buyer in transaction i has received a tax-exempt transfer and 0 otherwise. We study two separate outcomes, denoted by placeholder variable Y_i. First, the natural logarithm of the property i's list price. In this model, the β reveals to what extent buyers use a financial transfer to select more valuable homes. Second, the natural logarithm of the ratio between sale price and list price, which we denote as spread. Here, β captures the degree to which transfer recipients increase their bid for a given home. Given that price spreads might differ across price segments, we control for a home's list price in this model. Finally, λ_j captures matching-cell fixed effects in the CEM sample.

[5]Since we estimate equation (1) as a pooled cross-section we omit time subscripts.

The spread between sale price and list price serves as an intuitive proxy for relative over- versus underpayment. It is based on the assumption that, conditional upon a property's unobserved true value, list prices are independent of the buyer's treatment status. Given that list prices are determined before potential buyers arrive, this assumption seems plausible.

3.4 Addressing Endogeneity in Financial Transfers

By virtue of matching, we only compare buyers who are similar in a number of observable characteristics. However, matching cannot help overcome endogeneity concerns due to unobserved confounders. The estimated treatment effect from (1) could therefore be subject to bias. If, for instance, transfer recipients have more housing market experience or hire better real estate agents, which in turn helps them to better negotiate the price of a property, the estimated least-squared coefficient would underestimate the true causal effect of the transfer. Assuming that donors (typically parents) who make housing-related wealth transfers are familiar with the housing market, it is plausible that transfer recipients are more sophisticated than the average buyer in our sample. Addressing this concern requires exogenous variation in financial transfers that is unrelated to any confounding factor. To isolate such variation, we rely on the high quality of Dutch administrative data and propose an instrument based on the financial situation of buyers' parents.

The policy we study was announced surprisingly and only in place for a relatively short time. Therefore, we argue that it is not parental wealth per se, but their ownership of liquid assets, that drives the supply of wealth

transfers. Two potential donor families with equal net wealth could have had a very different propensity to make a transfer, depending on the type of assets that they owned as the policy was announced. Parents who owned a sizable amount of liquid assets were well-positioned to make a transfer, whereas those who derived their wealth from illiquid assets, such as real estate, may have been unable to do so. We therefore use the amount of liquid assets (bank deposits plus stocks and bonds) that a buyer's parents own as an instrument. Given that we also control for the parents' net wealth, this instrument captures the share of liquid assets in the parental wealth portfolio.

In order for our first instrument to qualify, it needs to meet two conditions. The first is instrument relevance. It requires the existence of a strong first stage, with nonzero correlation between the instrument and the endogenous treatment variable. We provide a test of this condition to the next section. The second condition, the exclusion restriction, cannot be formally tested. It requires that the instrument is uncorrelated with the error term in the reduced form regression. Intuitively, conditional on treatment assignment and control variables, a buyer's bid for a house must not be related to the share of liquid assets that her parents hold.

There are a number of reasons for why this assumption could be violated. On the one hand, parental liquid wealth may be related to a buyer's socioeconomic status, which in turn affects the bidding behavior. However, we argue that differences in unobserved characteristics would rather be related to a family's total net wealth, and not to the composition of their asset portfolio. A second threat to the exclusion restriction would arise if parents used their

liquid assets to support a home purchase in ways other than by making a wealth transfer. For instance, mortgage underwriters could offer more favorable terms to applicants whose parents can give financial support or even act as a guarantor. However, while we cannot rule this out theoretically, conversations with mortgage practitioners have suggested that it is very uncommon in practice. Finally, parents with a low share of liquid assets typically have much of their wealth tied up in real estate. While this restricts them in their capacity to make financial transfers, it could imply a higher level of sophistication in dealing with housing transactions, due to more experience. To address this, we include parental home ownership as a control variable in the IV estimation.

We implement the IV strategy by estimating the following system of equations:[6]

$$Y_i = \beta \cdot \widehat{Transfer}_i + \phi \cdot X_i + \lambda_j + \epsilon_i \tag{2}$$

$$Transfer_i = \pi \cdot \mathbf{Z}_i + \phi \cdot X_i + \lambda_j + \eta_i \tag{3}$$

Equation (2) is the second stage in the 2SLS system and equation (3) is the first stage. In the first stage, we estimate a linear probability model to predict the receipt of a wealth transfer based on our instrument (Z_i): the natural logarithm of liquid assets that a buyer's parents owned in 2013. To account for potential non-linearity in this relationship, we also include the square of this instrument. Additional control variables include: parental net wealth, financial assets and home ownership status. The predicted transfers

[6] As was the case for equation (1), we omit time subscripts.

from (3) are then used to study transaction outcomes in (2). We apply this model to the CEM sample and control for matching-cell fixed effects, in order to maintain comparability in observable buyer characteristics.

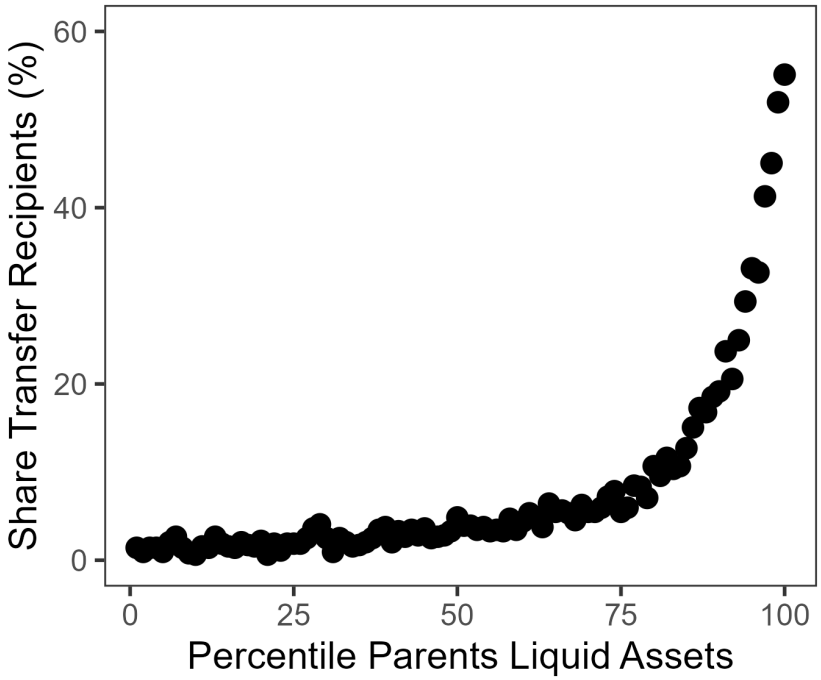
4 Empirical Results

4.1 First Stage Results

We begin by providing graphical evidence for the instrument's relevance. Figure 2 displays a binned scatterplot, in which the share of transfer recipients is depicted for each percentile in the distribution of parental liquid assets. Two observations stand out: first, transfers increase largely monotonously as a function of parental liquid assets. Second, the relationship is strongly nonlinear: while 4.9% of children of parents at the median of the parental liquid assets distribution (€37,000) receive a tax-free transfer, 19.2% of those with parents at the 90th percentile (€282,000) do.

First stage results, reported in Table A.1, confirm the graphical impression. The instruments, log(Liquid Assets Parents) and log(Liquid Assets Parents)^2, are highly significant after controlling for other financial characteristics of buyers and their parents. The Kleibergen-Paap F-Statistic in the full model, including a home's list price, is equal to 738.3, far exceeding any conventional rule-of-thumb threshold.

Figure 2: Parental Liquid Assets and Wealth Transfers



Notes: This figure plots the share of transfer recipients for homebuyers whose parents are in different percentiles of the liquid asset distribution.

4.2 Financial Transfers and Transaction Outcomes

The outcomes of the baseline analysis are provided in Table 2. Column (1) reports unmatched OLS results, while nearest neighbor and CEM results are reported in columns (2) and (3), respectively. Finally, column (4) contains the second-stage results of our IV estimation, applied to the CEM sample. As discussed above, OLS and matching results could be downward-biased if treated buyers are more sophisticated in real estate negotiations. However, our first-stage visualization in Figure 2 suggests that most transfers are made by parents far in the right tail of the distribution of liquid assets. If children of these wealthy parents are more prone to using their financial windfall for

overbidding and for buying pricier homes, the local average treatment effect in our IV estimation would be larger than the average treatment effect. We therefore believe it is reasonable to view matching and IV estimates as lower and upper bounds for the true causal effect of a financial transfer.

The estimated effect of wealth transfers on house selection is reported in panel A. Matching results suggest that transfer recipients buy homes that are approximately 20% more expensive than others. To be precise, the nearest-neighbor coefficient of 0.205 translates into an increase of 22.75%, or €47,500 above the average-priced home. The IV estimates suggests an even larger effect size, of roughly 35%. Taken together, these results indicate that financial transfers have substantial intensive-margin effects. Of the €100,000 that the median treated buyer was given in our sample, at least half was spent on purchasing more expensive homes (reflecting, for example, higher quality, larger living area etc.).

The analysis of bidding outcomes is reported in panel B. Here, the estimated effect of a transfer ranges from 0.005, for the OLS and CEM analysis, to 0.020 for the IV model. The results imply that for a home of given quality, transfer recipients pay a premium of approximately 0.5% to 2.0%, relative to non-treated buyers. Compared to the average transfer size and the list price effect reported above, this is relatively modest. However, it is economically non-trivial and especially noteworthy in view of the overall market condition at the time. Transfer recipients were willing to raise their bids in an economic environment where most homes sold in bilateral negotiations and at a significant discount relative to the list price. Below, we will discuss

their motive of wealth-transfer recipients for doing so and the ripple effects that this behavior might have on other buyers.

Table 2: Effect of the Wealth Transfer on Bidding Outcomes

	(1) OLS	(2) PSM	(3) CEM	(4) IV
Panel A: Housing Consumption	*Dep. Variable: log(List Price)*			
Transfer	0.200***	0.205***	0.163***	0.300***
	(0.007)	(0.010)	(0.006)	(0.032)
Buyer Controls	Yes	No	No	No
Parents Controls	No	No	No	Yes
Mean Outcome Control	12.26	12.25	12.28	12.28
Observations	104,209	10,052	63,777	63,777
Adjusted R^2	0.152	0.041	0.278	0.282
Panel B: Bidding Outcomes	*Dep. Variable: log(Spread)*			
Transfer	0.005***	0.006***	0.005***	0.020***
	(0.001)	(0.001)	(0.001)	(0.004)
log(List Price)	−0.016***	−0.018***	−0.016***	−0.017***
	(0.001)	(0.001)	(0.001)	(0.001)
Buyer Controls	Yes	No	No	No
Parents Controls	No	No	No	Yes
Mean Outcome Control	−0.053	−0.055	−0.054	−0.054
Observations	104,209	10,052	63,777	63,777
Adjusted R^2	0.027	0.025	0.035	0.030

Notes: This table reports estimates of the effect of wealth transfers on bidding outcomes. The treatment variable is a dummy, indicating whether a buyer has received a tax-exempt wealth transfer around the time of the purchase. Models in columns (1) - (3) either control for observable buyer characteristics or match buyers based on these characteristics. Column (4) employs an instrumental variables model based on the amount of liquid assets that a buyer's parents own. Heteroskedasticity-robust standard errors are reported in parantheses, and stars indicate 10% (*), 5% (**) and 1% (***) significance levels.

4.3 Robustness Tests

For the remainder of the empirical analysis, we focus on our second outcome, overpayment relative to untreated buyers. We first subject the baseline findings to a series of additional robustness tests. These are constructed to address different empirical threats to our design, related to the measure of overpayment, the use of a binary treatment dummy, and the specifics of the policy itself. The results of these tests, each of which we conduct both on the unmatched and on the CEM sample, are reported in Table 3.

First, we relax the assumption that the value of all structural and location characteristics is subsumed in the list price, such that buyers do not overbid systematically for homes of a certain type. This considers the possibility that transfer recipients could have preferences for certain amenities or locations, and that these carry a premium over the list price.[7] Columns (1) and (2) show that, while point estimates decrease slightly to 0.4% in the OLS and CEM model, the baseline effect appears to be robust to the inclusion of these additional hedonic characteristics.

Next, we address the concern that overbidding by transfer recipients could be driven by the particular design of the policy we study. One feature of the *Jubelton* policy was that after receiving a transfer, recipients were obliged to use the money during the policy period, in order not to forgo the tax exemption. It is possible, therefore, that transfer recipients only started overpaying towards the end of the policy period, due to time pressure. This

[7]Specifically, we control for the following variables: apartment dummy, construction period, living area, building type, number of rooms, interior and exterior condition.

would threaten the external validity of our findings, since wealth transfers do typically not involve a time constraint. However, these concerns appear to be unjustified, as an estimation of the baseline model excluding the last three months of the policy window shows. The results, reported in columns (3) and (4), are in line with the baseline model, suggesting that the tendency of transfer recipients to overbid is not tied to the particular policy under study, but inherently linked to the financial windfall they receive.

Finally, we replace the binary treatment dummy with a series of bins, indicating the size of the (net) transfer a buyer has received. If recipients are actually leveraging their extended budget to overbid, the estimated premium should increase with the size of the transfer. The results, reported in columns (5) and (6), confirm that this is indeed the case. Interestingly, for buyers with transfers below €50,000, we observe no significant effect. This suggests that bidding behavior is not the initial margin of adjustment for home buyers as their budget grows. Beyond €50,000, the estimated effect is positive and increasing in the size of the transfer. Based on the OLS and CEM results, we find that buyers with transfers larger than €100,000 - those exceeding the exemption limit - pay a premium of about 1%, relative to comparable buyers without any transfer.

4.4 Underlying Mechanisms

The baseline analysis shows that buyers increase their bid for a home after receiving a wealth transfer. This result can be driven by different underlying mechanisms, including characteristics of the buyer, the purchased property or

Table 3: Robustness Analysis

	(1) OLS	(2) CEM	(3) OLS	(4) CEM	(5) OLS	(6) CEM
	Dep. Variable: log(Spread)					
Transfer	0.004***	0.004***	0.005***	0.005***		
	(0.001)	(0.004)	(0.001)	(0.001)		
log(List Price)	−0.007***	−0.007***	−0.017***	−0.017***	−0.016***	−0.016***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Transfer ≤ 50k					0.000	-0.002
					(0.002)	(0.002)
50k < Transfer < 100k					0.004***	0.005***
					(0.001)	(0.001)
Transfer = 100k					0.006***	0.006***
					(0.001)	(0.001)
Transfer > 100k					0.011***	0.009***
					(0.003)	(0.003)
Matching Cell FE	No	Yes	No	Yes	No	Yes
Hedonic Controls	Yes	Yes	No	No	No	No
Sample Period	Full	Full	Short	Short	Full	Full
Observations	104,209	63,777	76,728	47,122	104,209	63,777
Adjusted R^2	0.081	0.084	0.030	0.037	0.028	0.035

Notes: This table reports additional robustness tests of the baseline analysis of bidding outcomes. In columns (1) and (2), hedonic characteristics are added to the model. In columns (3) and (4), the final months of the policy are excluded from the sample. In columns (5) and (6), the treatment variable is grouped into a series of bins. Heteroskedasticity-robust standard errors are reported in parantheses, and stars indicate 10% (*), 5% (**) and 1% (***) significance levels.

the local market in which this property is located. In this section, we include a series of interaction terms in the baseline model, to assess the merit of these different explanations.

The Dutch mortgage market is characterized by quite generous limits on loan-to-value ratios. In 2013, the maximum mortgage size was equal to 105% of the assessed house value. This allowed buyers to finance not just the full purchase, but additional cost as well, including transaction taxes or broker fees. Nonetheless, home purchases entail a range of additional expenses, including those on home-related durables and improvements (Benmelech et al., 2023). Therefore, liquidity-constrained buyers may have an incentive to negotiate aggressively in order to secure a low price. We hypothesize that, among previously liquidity-constrained buyers, the receipt of a transfer will have a more pronounced effect on price spreads. We test this in columns (1) and (2) of Table 4, where we interact the treatment with an indicator that takes the value 1 for buyers with below median liquid assets. However, the results show that the relevance of this channel seems to be limited, at best. While the main coefficient remains largely unchanged, at 0.4%, we do not find a significant difference between transfer recipients with large and small holdings of liquid assets.

Alternatively, financial transfers may help buyers find homes with a higher match value. Given that a larger budget allows buyers to broaden their search into higher price segments, it increases their chances of finding a "dream home", for which they are willing to pay a premium. We proxy for this effect by distinguishing between non-detached and detached units. Non-detached

units - including apartments, row houses and corner houses - are relatively standardized in most Dutch markets. Therefore, we argue that they are less dispersed with respect to the idiosyncratic match value they offer to buyers. In contrast, a buyer's chance of finding a unique property that is a perfect match is larger in the market for detached homes, which offers much greater variety. In order to test this mechanism, we interact the treatment with a dummy for detached units. Additionally, we include postcode-level fixed effects in this model, to control for the fact that detached and non-detached units tend to be located in different areas. The results, reported in columns (3) and (4), do not support the hypothesis that overpayment is driven by higher match values. In contrast, the estimated interaction effect - Transfer $\times$ Detached Home - is actually negative, albeit only significant at the 10% level. We therefore conclude that transfer recipients do not overpay to optimize idiosyncratic match value, as measured by the bidding behavior on detached homes.

Finally, bidding behavior could be driven by the local market environment. In competitive markets, buyers with the necessary means might raise their bid in order to reduce the risk of being outbid and to avoid a lengthy search process. To test this, we group postcodes into "tight" and "relaxed" markets, based on whether the average time on market was below or above the median (weighted by the average transaction volume) before the introduction of the policy. The results of this model, reported in columns (5) and (6), suggest that the local market environment does indeed determine the bidding behavior of transfer recipients. In fact, we find that transfer recipients only overpay, relative to other buyers, in relatively tight housing markets. While the uninteracted

treatment does not have a significant effect in these models, the estimated coefficient on the interaction terms is 0.4% and 0.7% in the OLS and CEM models, respectively. In the context of our analysis, overpayment therefore seems to be driven by the extent of competition in a local market. Recalling that our analysis is set against the backdrop of a sluggish housing market, we expect this effect to be even stronger in the context of a heated housing market.

5 Spillovers to Local Housing Markets

In the previous section, we established that recipients of wealth transfers make higher bids for a given home and that this effect is driven by the degree of competition in a market. Motivated by the growing evidence on spillovers in residential housing markets (Bayer et al., 2021), we now examine whether home purchases by wealth transfer recipients affect the bidding behavior of other buyers in a local market. Within a local market, potential buyers are often competing for the same units, and information about neighboring transactions is communicated indirectly via real estate agent networks. Therefore, it is plausible that during their search, buyers are informed about transaction outcomes in a local market. To the extent that buyers take this into account during their bidding decision, the influx of wealth transfer recipients into a local market could lead other buyers to increase their bids.

To investigate this, we rely on the fact that there is substantial regional variation in where transfer recipients buy homes. We define exposure as the share of transfer recipients among homebuyers in a postcode during the

Table 4: Underlying Mechanisms

	Buyer		**Building**		**Market**	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	CEM	OLS	CEM	OLS	CEM
	Dep. Variable: log(Spread)					
Transfer	0.004***	0.004***	0.003***	0.003***	0.000	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Liquidity Const.	−0.001***	−0.002**				
	(0.000)	(0.001)				
× Transfer	0.002	0.001				
	(0.002)	(0.002)				
Detached Home			−0.019***	−0.018*		
			(0.001)	(0.001)		
× Transfer			−0.004*	−0.005*		
			(0.003)	(0.003)		
Tight Market					0.012***	0.013***
					(0.000)	(0.000)
× Transfer					0.007***	0.004***
					(0.002)	(0.002)
log(List Price)	−0.016***	−0.016***	−0.015***	−0.014***	−0.015***	−0.015***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Matching Cell FE	No	Yes	No	Yes	No	Yes
Postcode FE	No	No	Yes	Yes	No	No
Observations	104,209	63,777	104,209	63,777	103,969	63,618
Adjusted R^2	0.028	0.035	0.131	0.145	0.040	0.048

Notes: This table reports results from three heterogeneity analysis aimed at studying the mechanism driving the bidding response. In columns (1) and (2), the treatment is interacted with a dummy indicating liduidity constrained buyers. In columns (3) and (4), it is interacted with a dummy for detached homes. In columns (5) and (6), it is interacted with a dummy for tight postcodes. Heteroskedasticity-robust standard errors are reported in parantheses, and stars indicate 10% (*), 5% (**) and 1% (***) significance levels.

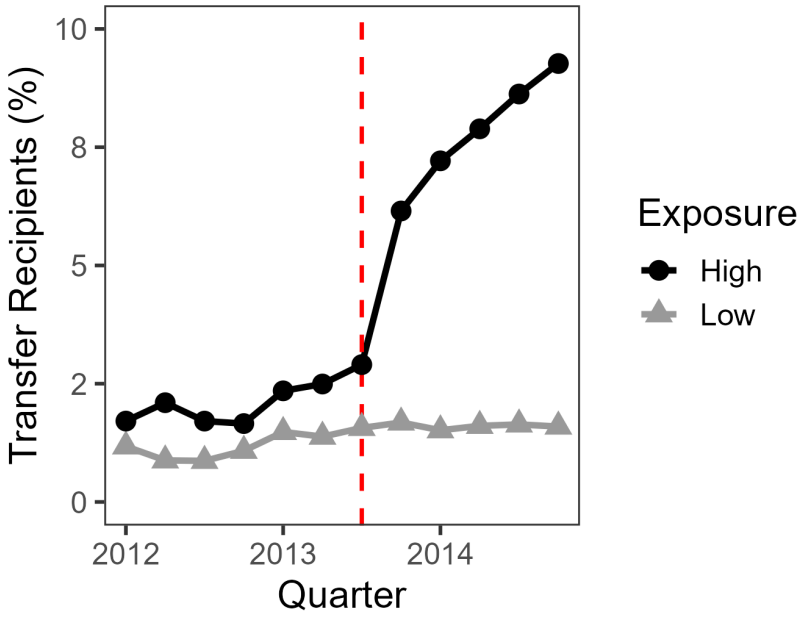
policy window, ranging from 1.54% at the 25th percentile to 6.93% at the 75th percentile, with a median of 3.98%. We base the empirical analysis on postcodes, not entire municipalities, in order to control more precisely for unobserved confounders that may correlate with treatment exposure. Dutch postcodes are somewhat smaller than their US equivalent, with a mean population size of 4,000.

In Figure 3, we split postcodes into those with below and above median exposure, and plot the average share of transfer recipients among all homebuyers in a given quarter. Even before the introduction of the policy, some purchases are made by buyers who received a tax-exempt transfer. As the introduction of the *Jubelton* policy raises the exemption threshold to €100,000, the share of transfer recipients rises quickly in high-exposure postcodes, from 2.9% in Q3/2013 to 7.2% in Q1/2014. Interestingly, transfers become increasingly popular over time, and they peak just before the policy is phased-out in Q4/2014. In contrast, in below-median exposed postcodes, the share of transfer recipients stays virtually flat after the introduction of the policy.

5.1 Price Spreads in Exposed Markets

We examine whether competition from wealthy transfer recipients drives up average bids in a local market. To this end, we compare postcodes with varying exposure to the tax-exemption policy. The main challenge to identification stems from the fact that regional exposure to the treatment, much like the individual-level treatment, is not randomly assigned. To address this

Figure 3: Share of Transfer Recipients over Time



Notes: This figure plots, on a quarterly level, the share of transfer recipients among all buyers in postcodes classified as high exposure versus low exposure. The dashed red line indicates the announcement of the policy.

issue, we include a pre-period into the analysis and control for postcode- and quarter-fixed effects. Any remaining bias would therefore have to come from time-varying factors that correlate both with postcode-level bidding patterns and the location choices of transfer recipients. If, for instance, transfer recipients would systematically select into postcodes that "gentrify" around the time of the policy introduction, our estimates include the effect of other, omitted, factors. Given that we lack (quasi-) random variation in the allocation of treated buyers across space, we cannot rule out these concerns entirely. However, we will provide additional evidence below to support the interpretation that changes in local prices are indeed driven by the influx of

transfer recipients.

Empirically, our approach resembles a dynamic difference-in-differences model with a continuous treatment variable. The following model is estimated, based on the extended sample, covering all transactions from January 2012 until December 2014:

$$Y_{ipt} = \sum_{k} \beta_k \cdot 1\{t = k\} \cdot Exposure_p + \mu_p + \delta_t + \varepsilon_{ipt} \tag{4}$$

As in the individual level analysis, Y_{ipt} captures the log sale-to-list ratio for transaction i. The additional subscripts t and p indicate the quarter in which a transaction took place and the postcode in which the home is located. $Exposure_p$ denotes the share of transfer recipients among all homebuyers in postcode p during the policy window. While the baseline differences across postcodes are captured by the unit fixed effects, μ_p, the variables of interest are the interaction terms between postcode exposure and the quarter-dummies, $\sum_k 1\{t = k\}$, ranging from Q1/2012 until Q4/2014. We omit Q2/2013, the quarter just before the announcement of the *Jubelton* policy. Therefore, the coefficient vector β_k captures differences in price spreads across postcodes with varying exposure, relative to the baseline differences in Q2/2013. Finally, quarter fixed effects δ_t absorb aggregate time trends.

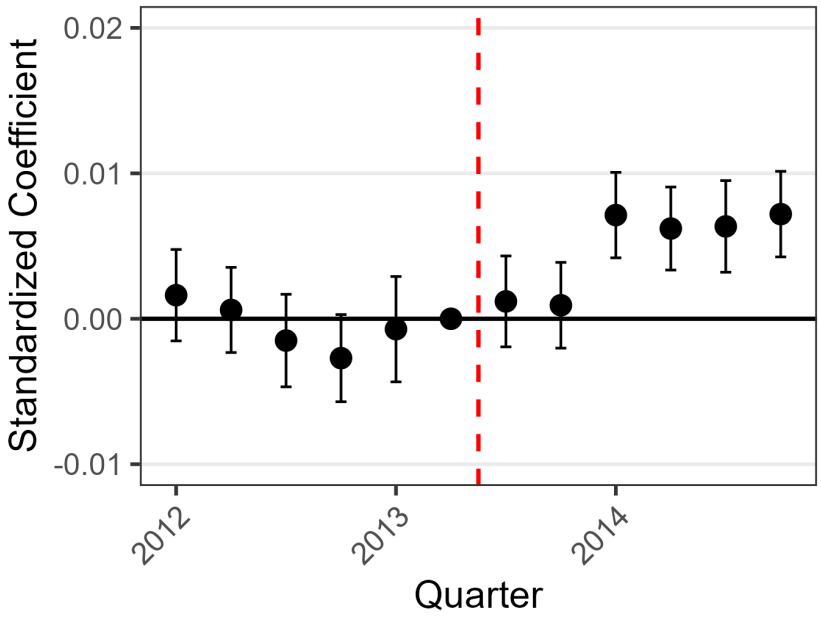
We plot the standardized model coefficients in Figure 4. Prior to the introduction of the policy, when hardly any housing related wealth transfers were made, average price spreads evolved similarly across postcodes. Importantly, we do not find any evidence for significant pre-trends. After the

introduction of the policy (at the end of Q3/2013), price trends stay the same for an additional quarter, before increasing significantly in those postcodes where many transfers are made. This is consistent with the actual share of transfer recipients in high-exposure postcodes documented in Figure 3, rising after the policy introduction, but only peaking over the course of the following year. Throughout 2014, we estimate that in postcodes with a one standard deviation increase in exposure, average sale prices rose by approximately 0.7%, relative to the list price. Importantly, this effect is not driven by transfer recipients alone. In Figure A.1, we repeat the same analysis, but omit all transfer recipients from the estimation. The results are largely unchanged, suggesting that regular buyers updated their bidding behavior in response to an influx of transfer recipients.

5.2 Robustness Tests

Even though we can control for all time-invariant differences across postcodes, our findings need not reflect a true causal effect of competition from transfer recipients on the bidding behavior of other buyers. Particularly, the fact that postcode-level exposure is defined ex-post opens the door for alternative explanations. For instance, wealth transfer recipients could self-select into gentrifying neighborhoods where prices would have risen anyway. Alternatively, our findings could be driven by reverse causality if, due to rising prices, some postcodes become relatively less attractive to prospective buyers who did not receive financial support. Fully overcoming these concerns would require (quasi-) random variation in the allocation of transfer recipients

Figure 4: Price Spreads in Exposed Markets



Notes: This figure visualizes the coefficients from a dynamic difference-in-differences model, studying the effect of a higher share of transfer recipients among buyers on price spreads. Error bars represent the 95% confidence interval. The dashed red line indicates the announcement of the policy.

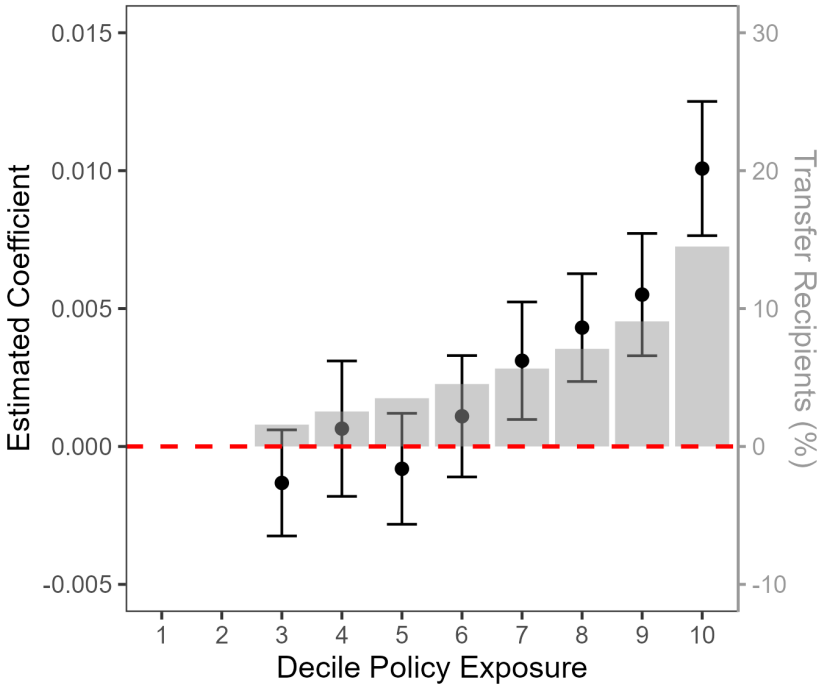
across local housing markets, which is not available in our setting. Instead, we implement two additional tests, in order to rule out alternative explanations.

We begin by estimating an alternative model where, instead of using the continuous exposure variable from (4), we group postcodes into exposure deciles, weighted by the average transaction volume in the pre-policy period. Then, we re-estimate the baseline model in its static form, comparing outcomes before and after the introduction of the policy. Postcodes in the bottom two exposure deciles, which did not attract any transfer recipients, are the omitted group. The intuition behind this test is straightforward: if price increases truly are driven by competition from transfer recipients, we expect the effect size

to grow monotonically as a function of postcode exposure. Moreover, the relationship could be non-linear, if transfer recipients are only "noticed" in a local market once they are sufficiently numerous. The results, depicted in Figure 5, are in line with this interpretation. The figure plots coefficients from the interaction terms between the post-policy dummy and each exposure decile. In addition, it shows, on the secondary y-axis, the average share of transfer recipients in each decile. Up until the sixth exposure decile, there is no measurable price effect, relative to postcodes which attracted no transfer recipient at all. Beyond that, however, the effect size is growing quickly. Going from the 9th to the 10th exposure decile, the estimated coefficient almost doubles, from 0.55% to 1%.

Next, we address the concern that transfer recipients are drawn into gentrifying neighborhoods, where prices would have also risen in the absence of the policy. To investigate this, we implement a placebo test, relying on another piece of information about properties that is available in the data from Statistics Netherlands. In the Dutch market, properties are assessed for taxation purposes on an annual basis, and all tax-assessed values are public information. Municipalities are responsible for the assessment, which is supposed to provide an objective measure of a property's current market value. The intuition underlying our placebo test is the following: in case the share of transfer recipients in a postcode is indeed unrelated to confounding factors, it should not predict changes in tax-assessed values over time. If, instead, assessed values would be on a differential trend in exposed and unexposed postcodes, this would rule out spillovers from transfer recipients as the (sole)

Figure 5: Price Spreads in Exposure Deciles

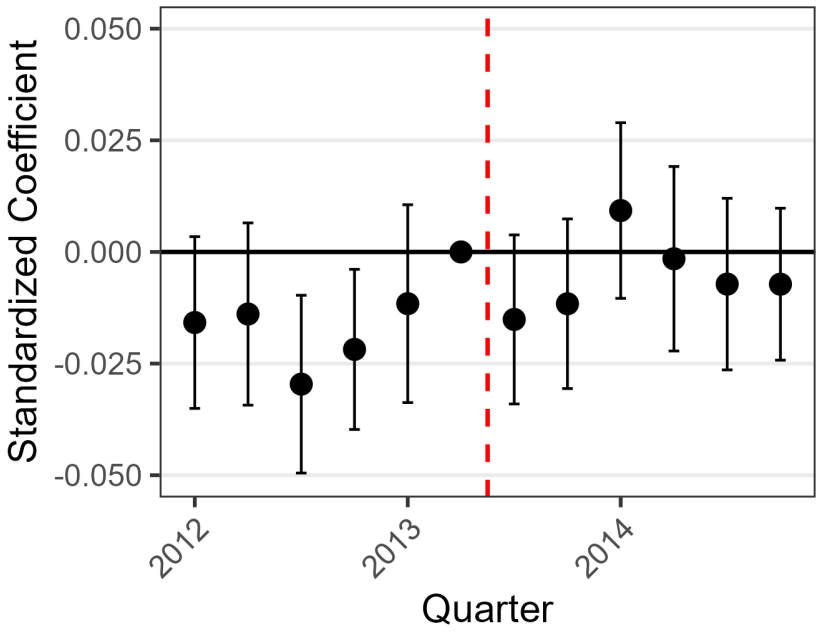


Notes: This figure visualizes the coefficients from a static difference-in-differences model, studying the effect of a higher share of transfer recipients among buyers on price spreads. Here, the continuous treatment variable is replaced with dummies indicating the respective decile of treatment exposure. Error bars represent the 95% confidence interval.

driver behind our findings.

We implement this placebo test by replacing, in (4), the dependent variable with the natural logarithm of the current assessed value of property i. The resulting coefficients are plotted in Figure 6. While estimates are noisier than those from the baseline model, we do not find any evidence for a rise in tax-assessed values in postcodes that are more exposed to transfer recipients. Taken together, our findings therefore suggest that in exposed markets, average bids increase relative to list prices, and that this increase cannot be explained by rising property values in these markets.

Figure 6: Tax-Assessed Values in Exposed Markets



Notes: This figure visualizes the coefficients from a dynamic difference-in-differences model, studying the effect of a higher share of transfer recipients among buyers on average tax assessed values. Error bars represent the 95% confidence interval. The dashed red line indicates the announcement of the policy.

6 Conclusion

A substantial number of homebuyers are relying on wealth transfers as a means of financing their down payment (Brandsaas, 2021). In this paper, we take a first step towards understanding the effect of wealth transfers on housing market outcomes. To do so, we rely on an exogenous tax-exemption policy for wealth transfers in the Dutch housing market. For a limited period of time, transfers up to €100,000 were exempt from taxation, as long as the funds were used for housing-related expenditures. We are able to identify home buyers who received a transfer and compare them to others, buying a house at the

same time but without financial support. To overcome endogeneity concerns, we match treated and untreated buyers and instrument for the receipt of a wealth transfer using the share of liquid assets in a buyer's parents wealth portfolio.

At the individual level, we document two main findings. First, we estimate that at least half of the total net transfer a buyer receives is spent on buying more expensive homes. Across the different models, we estimate that transfer recipients choose homes which are 20% - 35% more expensive than other buyers, all else equal. Since there are no direct positive externalities associated with larger homes, we conclude that exempting housing-related transfers from taxation is mainly a subsidy for those who are relatively well-off to begin with. Second, we find that transfer recipients pay a premium of 0.5% - 2% for a given home, relative to other buyers. This tendency to overpay is driven by the tightness of the local market in which the purchase takes place, suggesting that transfer recipients use their increased budget to avoid a lengthy search process. Therefore, we hypothesize that transfer recipients could be overpaying more aggressively as housing markets "heat up" and become more competitive.

In addition, we establish a local spillover effect in bidding behavior: as transfer recipients move into a local market, other buyers tend to increase their bids as well. To show this effect, we calculate each postcode's exposure to the policy, based on the share of transfer recipients among all buyers during the policy period. We compare local markets that vary in their exposure to the policy before and after it has been introduced, controlling for postcode- and month-fixed effects. We find that, for a one standard deviation increase

in policy exposure at the postcode level, the sale prices increases by 0.7%, relative to list prices. This effect is increases in a non-linear way with exposure, suggesting that spillovers require a "critical mass" of transfer recipients among homebuyers.

Taken together, our findings suggest that intra-family wealth transfers could have important, and potentially distortionary effects on housing markets. As housing affordability continues to decline, the share of first-time buyers relying on family resources will likely to grow even further. More research needs to be done in order to understand the implications of this along a number of dimensions, including wealth inequality, housing market outcomes and, ultimately, optimal policy design.

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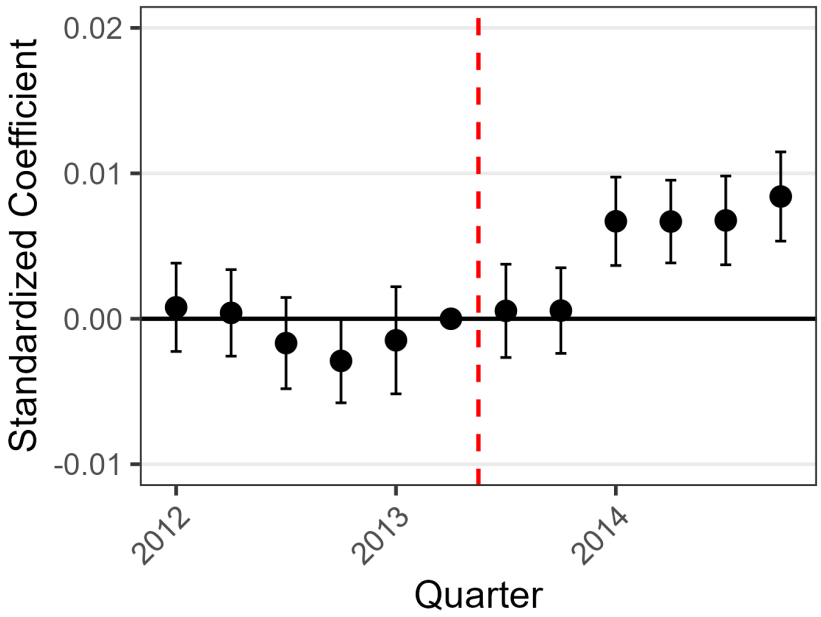
A Appendix

Table A.1: IV First Stage: Predicting Wealth Transfers

	Dep. Variable: Transfer		
	(1)	(2)	(3)
log(Liquid Assets Parents)	0.022^{***}	-0.163^{***}	-0.160^{***}
	(0.001)	(0.005)	(0.005)
log(Liquid Assets Parents)^2		0.010^{***}	0.009^{***}
		(0.000)	(0.000)
log(List Price)			0.034^{***}
			(0.003)
log(Total Assets Parents)	-0.001	0.020^{***}	0.019^{***}
	(0.001)	(0.002)	(0.001)
Net Wealth Perc. Parents	0.002^{***}	0.001^{***}	0.001^{***}
	(0.000)	(0.000)	(0.000)
Homeowners Parents	-0.055^{***}	0.072^{***}	0.070^{***}
	(0.005)	(0.005)	(0.005)
Matching Cell FE	Yes	Yes	Yes
Kleibergen-Paap F-statistic	587.0	747.3	738.3
Observations	63,777	63,777	63,777
Adjusted R^2	0.123	0.165	0.167

Notes: This table reports estimates of the first stage regression. The endogenous variable is a dummy indicating whether the buyer has received a tax-exempt transfer. The instrument is the natural log of parental liquid assets (linear and squared). Additional exogenous variables include the log of parental (total) assets, parental net wealth (percentile rank) and a dummy indicating parental homeownership. All models include matching-cell fixed effects, to compare buyers within the same stratum for wealth, income, age and parental wealth. Heteroskedasticity-robust standard errors are reported in parantheses, and stars indicate 10% (*), 5% (**) and 1% (***) significance levels.

Figure A.1: Price Spreads in Exposed Markets: Excluding Transfer Recipients



Notes: This figure repeats the baseline difference-in-differences analysis from Figure 4, excluding transfer recipients. Error bars represent the 95% confidence interval. The dashed red line indicates the announcement of the policy.