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Discussion Paper Series

IZA DP No. 19071

November 2026

Do Transfer Tax Cuts Create Homeowners?

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Do Transfer Tax Cuts Create Homeowners?*

Abstract

We study whether financial assistance to prospective homebuyers succeeds in promoting homeownership by analyzing a prominent real-estate transfer tax exemption for young homebuyers in the Netherlands. The reform offers quasi-experimental variation across time, buyers, and units, which we exploit using granular administrative data and various complementary reduced-form approaches. The two-percentage-point transfer tax cut substantially raised transactions among the young, but we can rule out even modest medium-run increases in homeownership. Instead, we document large timing and bunching responses. Prices of qualifying units increased by between 1.2% and 1.8%, suggesting that the tax cut mostly benefitted sellers. Welfare simulations reveal that, while the tax cut reduced transfer tax distortions and increased overall economic surplus, gains accrued primarily to upper-middle-income households.

JEL classification

R21, R28, H22, H31

Keywords

housing markets, homeownership, transfer taxes, welfare

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* We thank Marta Cota, Katharina Erhardt, Jarkko Harju, Marlene Koch, Matthijs Korevaar, Jos van Ommeren, Sofie Waltl, and seminar and conference participants at Cambridge, CESifo, CPB, Helsinki GSE, FIT Tampere, NED 2026, RES Newcastle, SAFE Frankfurt, Tilburg, Tinbergen Institute, UEA Berlin, the Verein für Socialpolitik, ZEW, and the Dutch Ministry of Finance for valuable comments. Gregorio Rossi provided outstanding research assistance. Löffler gratefully acknowledges financial support from the Dutch Research Council (NWO, file number VI.Veni.221E.037 and grant DOI: 10.61686/YSPRB94911).

1. Introduction

Homeownership among young households has been falling in many advanced economies in recent decades (Paz-Pardo, 2024, Balestra, Caisl, and Hermida, 2025). To counter this trend and promote homeownership, many countries have implemented financial assistance programs for prospective homebuyers. These programs take various forms, such as tax relief, direct cash transfers, or mortgage support, but they all aim to lower the upfront cost due to down payments, fees, and taxes.[1] However, such policies come at great fiscal cost, and whether they succeed in promoting homeownership is theoretically ambiguous. On the one hand, reducing homebuyers' financial barriers should increase their chances to enter the market. On the other hand, demand-side subsidies may just capitalize and raise prices. Empirically identifying the effect of such programs is difficult because it requires administrative panel data on ownership and exogenous treatment variation.

In this paper, we provide novel evidence on the effects of homebuyer assistance programs by studying the abolishment of the real-estate transfer tax for young homebuyers. Transfer taxes are widely cited as a key barrier to homeownership (see, e.g., Han, Ngai, and Sheedy, 2026), and various countries feature some favorable transfer tax treatment for starters (see Appendix Table A.1 for an overview). We study the impact of a transfer tax cut on purchase behavior, homeownership, house prices, and welfare. To this end, we exploit detailed administrative microdata and rich quasi-experimental variation across time, individuals, and housing units, stemming from a recent tax reform in the Netherlands. In 2021, Dutch legislators introduced an exemption from the 2% real-estate transfer tax for homebuyers under the age of 35. After three months, a maximum price per unit was added to qualify for the exemption, creating a notch in the tax code. This setting offers an ideal laboratory, as it allows us to study the effects of the transfer tax cut over time, with and without a price limit, and separately for first-time and repeat buyers, lower vs. higher income, and more vs. less cash-constrained households, as well as across segments of the housing market.

Using several complementary quasi-experimental approaches, we present five main findings. First, the transfer tax exemption stimulated home purchases. The effect was strongest during the initial quarter, but remained positive in the medium run after the introduction of the price limit. Second, additional transactions raised the ownership rate, but only temporarily. We find a small but significant increase in the first year after the policy's introduction, but we can reject even such small increases only a few years later. Instead, we document substantial (re-)timing responses and additional purchases by repeat buyers trading up. Third, the tax notch due to the price limit distorted

[1] Prominent examples include the American Dream Downpayment Initiative and the First-Time Homebuyer Credit offered in the US in the 2000s, zero-interest loans for French first-time buyers, the First Home Owner Grant for new units in Australia, and the UK Stamp Duty Land Tax relief for first-time buyers. We briefly summarize 64 such national policies from 34 countries in Appendix Table A.1. Warden and Kadzielawski (2026) list more than 2,000 additional regional programs for the US alone.

purchases towards the lower-priced segment and led to a strong bunching response below the cutoff. Fourth, we estimate that prices of qualifying units increased by 1.2%–1.8%, suggesting that sellers captured the majority of the tax benefit. Fifth, the tax cut still mitigated existing tax distortions and thus raised economic surplus. The largest gains accrued to upper-middle-income households, casting doubt on the positive impact on social welfare.

Reduced-Form Analyses. We start by providing causal evidence on the effect of the transfer tax cut on home purchases. In a difference-in-differences setup, we compare the purchase behavior of young, treated individuals with that of slightly older cohorts who were never eligible. Both groups move in parallel before the policy and diverge sharply just after the announcement. Transactions by young buyers doubled in the first quarter, when no price limit applied. In the medium run, the policy raised purchases among younger cohorts by around 13%, relative to pre-reform levels. Short-run responses are driven by individuals with higher liquid assets and incomes, whereas medium-run effects stem from those with lower assets and incomes. Comparing time trends across market segments with varying policy exposure suggests little crowding out of older buyers.

However, an increase in purchases does not necessarily translate into higher homeownership rates. A first indication is that the majority of additional purchases generated by the policy were made by existing homeowners deciding to trade up. We investigate this further by considering various forms of timing responses to the policy. The specific design of the tax exemption creates strong incentives to optimize home purchases around cutoff dates—such as the policy introduction in January, the introduction of the price cap in April 2021, and the 35th birthday. Dynamic treatment estimates suggest that approximately 60% of the spike in transactions in the initial three-month period is attributable to time bunching around policy cutoff dates. We complement the evaluation with a regression discontinuity analysis of home purchases around the 35th birthday. While behavior is smooth around the cutoff before the policy, a sharp discontinuity emerges after its implementation. Most of this discontinuity is driven by individuals just around their 35th birthday. Using a donut approach that excludes those months reduces the estimated discontinuity by at least 80%, rendering it only marginally significant.

We then turn to the Dutch wealth statistics and compare two treated and one untreated birth-year cohorts at the same age in a complementary difference-in-differences approach. Comparing individuals from similar cohorts at the same age circumvents most of the secular differences in ownership profiles across the life cycle and over time, such that ownership rates develop similarly across the three cohorts before the tax reform. In the initial year of the policy, we find a small and significant increase in the ownership rate of up to 0.6 pp for treated individuals. But this effect vanishes as soon as we zoom out and compare cohorts a few years before and after policy introduction. The estimates for 2024, our last year of data and a few years after the tax exemption was implemented,

exclude ownership effects exceeding 0.3 pp. We confirm this small, but only short-lived increase in ownership also via a regression discontinuity approach around treatment eligibility using birth-year-month cohorts as the running variable. Individuals born in January 1986 or later—who were at least briefly eligible for the exemption—postponed home purchases in 2020 and saw somewhat higher ownership rates after the first year of the policy. However, in 2023, two years into the policy, the discontinuity returned to its pre-reform level and is no longer significantly different from zero.

On the intensive margin, we find strong responses to the incentives created in the different phases of the policy. Eligible homebuyers used the absence of a price limit in the first three months to buy significantly more expensive units in higher-income neighborhoods. After the introduction of the price limit, this effect reverses, as treated buyers opt for significantly lower-quality units to remain eligible. In addition, we find substantial bunching by eligible buyers just below the price limit, and provide suggestive evidence that these excess transactions would have traded for prices above the threshold absent the reform's tax notch.

Finally, we investigate the policy's price effects. Theory predicts that the tax exemption will partly capitalize into higher prices unless housing supply is perfectly elastic. Using an auxiliary difference-in-differences analysis comparing units with ex ante assessed value below versus above the price limit, we find that qualifying units experience stronger medium-run price increases. Our estimates suggest that prices of qualifying units rose by between 1.2% and 1.8%. This effect is sizable, given that the 2% tax reduction applied only to a subgroup of buyers. It is consistent with young, credit-constrained buyers being the marginal buyers of lower-priced units (Ortalo-Magné and Rady, 2006). The results imply that sellers were the main beneficiaries of the policy. Eligible buyers retained only a small share of the exemption, while older buyers of lower-priced units lost out, facing higher prices without any compensation.

Efficiency and Welfare. Equipped with the reduced-form evidence, we employ a simple, static assignment model with indivisible housing to quantify the utility consequences of financial support for young homebuyers at the individual level (as in Määttänen and Terviö, 2022). We then take a sufficient-statistics approach (see Chetty, 2009, Kleven, 2021) and show that the utility consequences of a small change in the transfer tax are governed by three empirical objects: (i) the effect of the tax cut on house prices, (ii) the policy's impact on home purchases, and (iii) its impact on transfer tax revenues. Individual behavioral responses, in contrast, have no first-order utility effect because of the envelope theorem.

In the final part of the paper, we quantify the utility consequences of the transfer tax cut for the entire Dutch adult population. We group individuals into different subpopulations to distinguish between young and old, eligible and ineligible, marginal and inframarginal buyers and sellers, as well as the large majority of never takers.

Relying on the reduced-form estimates, we then calculate household-level money-metric utility changes because of the transfer tax cut. We show that the policy increased economic surplus and enhanced efficiency by mitigating existing distortions due to the real-estate transfer tax. The gains of the transfer tax cut accrue predominantly to upper-middle-income households, since many eligible homebuyers (i.e., those under 35 buying units below the price limit) and many sellers are from this group. In contrast, there are few homebuyers in the lowest income quartile, even after the tax cut. Top-income households more often buy units above the price limit and therefore rarely qualify for the tax exemption. This distributional pattern makes it unlikely that the transfer tax cut improved social welfare.

Related Literature. This paper contributes to the literature studying financial assistance programs for homebuyers. A key strand in that literature has suggested that reducing real-estate transfer taxes across the board can promote homeownership, especially among young households (see the quantitative models in Kaas et al., 2021, Cho, Li, and Uren, 2024, and Han, Ngai, and Sheedy, 2026). Our quasi-experimental results show that a transfer tax cut for young buyers in the Netherlands stimulates purchase activity, consistent with evidence that transfer taxes reduce transactions and mobility (Hilber and Lyytikäinen, 2017, Eerola et al., 2021), but is mainly capitalized into prices and does not generate a lasting increase in the homeownership rate. In addition, our granular analysis of buyers' behavioral response contributes to the broader literature studying the effects of transfer taxes on housing market outcomes (Besley, Meads, and Surico, 2014, Kopczuk and Munroe, 2015, Slemrod, Weber, and Shan, 2017, Best and Kleven, 2018, Dolls et al., 2025). The rich heterogeneity in the microdata combined with the variation in treatment enables us to investigate various aspects of the transfer tax as well as heterogeneous responses to the tax cut.

We also add to a large literature studying various forms of demand-side subsidies in the housing market. Most closely related are papers investigating direct subsidies for young and first-time homebuyers, including refundable tax credits (Floetotto, Kirker, and Stroebel, 2016, Hembre, 2018, Berger, Turner, and Zwick, 2020) and subsidized government loans (Gobillon and Le Blanc, 2008, Benetton et al., 2022, Carozzi, Hilber, and Yu, 2024). In the literature on purchase subsidies, ours is, to the best of our knowledge, the first paper that uses administrative panel data to trace individual-level responses to purchase subsidies at both the extensive and the intensive margin.

Studies of upfront buyer support are closely related to the literature examining fiscal subsidies for existing owners. Most prominent among those are the mortgage interest deductions, which offer a substantial tax benefit for homeowners in many countries. Reduced-form and quantitative analyses find that the mortgage interest deduction has a negligible or even negative effect on homeownership, while mainly affecting buyers' leverage decisions and raising prices (Glaeser and Shapiro, 2003, Hilber and Turner,

2014, Sommer and Sullivan, 2018, Gruber, Jensen, and Kleven, 2021). While upfront subsidies differ in design, our findings suggest that they share similar shortcomings, namely that benefits are capitalized into prices and that they hardly generate additional homeowners.

To assess the external validity of our findings, it is important to acknowledge institutional differences. In other countries, there exist various non-transfer-tax instruments, often with slightly different target populations (see Appendix Table A.1). However, both the maximum benefit amount (€8,000) and the economic mechanism of our policy—a one-off reduction in the out-of-pocket costs faced by buyers—are comparable to most other subsidies, suggesting that our findings may extend to other settings. Moreover, the Dutch market features a statutory LTV limit of 100% and rather low real-estate transfer taxes. Therefore, it may be the case that only a few prospective buyers are constrained by upfront costs, even though transfer taxes need to be financed out-of-pocket. The effects of purchase subsidies in countries with larger down payment requirements may therefore be more pronounced. On the other hand, the institutional environment made buy-to-let purchases simultaneously increasingly unattractive in the Netherlands. In a market that is not fully segmented, the resulting retreat of investors frees up units for owner-occupiers, making it easier for a buyer subsidy to raise homeownership even when supply is inelastic in the short run (Greenwald and Guren, 2025). This feature may again lead to larger ownership effects relative to other settings. While we cannot quantify these two aspects, it is not obvious that purchase subsidies would be systematically more effective in raising homeownership elsewhere.

The remainder of this paper is organized as follows. Section 2 details the institutional background and the data used. Section 3 presents the reduced-form results. Section 4 introduces the theoretical setup to evaluate the efficiency and welfare implications of transfer tax cuts and presents simulation results. Section 5 concludes.

2. Institutional Setting and Data

2.1. Background

Homeownership rates in the Netherlands, at around 60%, are close to the European average. One defining institutional feature is the well-developed mortgage market, which supports broad availability. Dutch mortgage debt levels are the highest in Europe (DNB, 2021), in part due to historically generous loan-to-value (LTV) limits. While lending standards were somewhat tightened in the aftermath of the global financial crisis, the statutory LTV limit has remained at 100%. In theory, this makes homeownership more accessible for young, liquidity-constrained households than in other countries with binding down payment requirements. However, tight limits on loan-to-income (LTI) ratios constrain the borrowing capacity of many prospective homebuyers. Since these limits are

based on current, rather than permanent, income, they are particularly binding for young households with short employment histories. As in many other advanced economies, the sharp increase in house prices over the past decade has therefore put homeownership out of reach for many young households. Over the decade from 2010 to 2020, the average age of first-time homebuyers increased by more than a year, from 28.6 to 29.8 years (Tweede Kamer, 2020).

In response to these trends, the Dutch government has introduced a range of housing market interventions aimed—directly or indirectly—at supporting young and first-time buyers. These include tax exemptions for inter-generational wealth transfers related to housing (Bos, Kok, and Wogh, 2024), mortgage assistance schemes, and restrictions on buy-to-let transactions in certain municipalities (Francke et al., 2025). Overall, the Dutch housing market saw an increasing share of buy-to-own as opposed to buy-to-let transactions in recent years (see Appendix Figure B.1 for a corresponding time series).

2.2. The Starter Tax Exemption of 2021

In September 2020, the Dutch parliament passed a real-estate transfer tax exemption for homebuyers under the age of 35 (in Dutch: *startersvrijstelling*). Despite its name, the transfer tax cut applied to both first-time and repeat buyers, allowing us to investigate the effects on each group separately. Older buyers continued to pay the statutory 2% tax rate. The policy was first mentioned in public in July 2020 and finally implemented as of January 2021, shortly before the Dutch housing market reached its peak. If individuals made a joint purchase, the exemption applied in proportion to ownership shares. For instance, a co-purchasing couple with one partner under 35 and one partner aged 35 or older paid 1% of the price in tax if ownership was split equally. The tax exemption was granted only if the acquired unit was used for owner-occupied living, whereas the applicable tax rate for buy-to-let transactions increased concurrently from 6% to 8%, irrespective of the buyer's age.

In November 2020, prior to the policy's introduction, legislators announced a price limit for qualifying transactions that would later lead to a notch in the tax code. As of April 2021, three months after the policy took effect, only units trading for up to €400,000 qualified for the tax exemption, while units priced above this limit were again subject to the regular 2% tax rate, regardless of the buyer's age. At the time of introduction, the cutoff value corresponded roughly to the median transaction price in the Netherlands. In January 2023, the price limit was raised to €440,000, to reflect increased property prices.

The Dutch starter tax exemption policy shares many features with other support schemes for young buyers worldwide, such as outright cash grants, tax credits, or down payment assistance. While these policies differ in their specific designs, they all provide financial assistance to prospective buyers by reducing upfront costs, intending to facilitate earlier access to homeownership.

2.3. Data

Our main data source is anonymized administrative records on the Dutch population provided by Statistics Netherlands (CBS). These data include demographic characteristics, residential location, and homeownership status for every person living in the Netherlands. One limitation of the data is that birth dates are often missing for people born outside the country. As we heavily rely on birth-date information, we restrict the sample to individuals born in the Netherlands. The CBS data also provide various household-level income and balance sheet variables, which we utilize in heterogeneity analyses to group households into subsamples of the population. In addition to the individual-level data, the CBS records track various characteristics of the residential housing stock, including first and second owners, as well as tax-assessed values (*WOZ* values).[2]

We match the CBS records with two housing transaction datasets. First, we use data from the Dutch Land Registry (*Kadaster*), which covers the universe of existing home sales in the Netherlands but includes only a limited set of variables. Because the identities of buyers and sellers are not directly recorded in the transaction data, we impute home purchases from changes in property ownership records. Since these records capture both the first and second owners of a property, we identify all purchases made individually or with a partner. Second, we augment these data with information from the country's largest association of real-estate brokers (*NVM*). These auxiliary data cover only around 60% of all transactions, but provide more detailed information on each transaction, including property characteristics and listing prices.

Main Sample. Combining these sources, we construct a person-by-month panel dataset spanning the period from January 2019 to December 2023. We track several key variables for Dutch-born adults, most importantly, their home purchases and ownership, as well as transaction prices, tax-assessed values, and characteristics of purchased units. Throughout the analyses, we restrict the sample to individuals near the policy cutoff at age 35, dropping individuals under 30 and those 43 or older. As a result, we work with a semi-balanced panel in which individuals enter and exit the sample over time as they approach the lower or pass the upper age thresholds. Each month, the sample covers around 1.7 million individuals.

3. Reduced-Form Evidence

In this section, we provide empirical evidence on the effects of transfer tax cuts, using quasi-exogenous variation from the Dutch starter tax exemption. In Section 3.1, we first analyze the policy's impact on home purchase behavior. Sections 3.2 and 3.3 present

[2] The Dutch ownership register reported only the first owner until 2018. We therefore perform most of our analyses only from 2019 onwards to ensure a consistent ownership register over time.

analyses of timing and extensive-margin responses. We evaluate intensive-margin responses and bidding behavior in Section 3.4. Finally, we study the policy's effect on aggregate house prices in Section 3.5.

3.1. Home Purchase Behavior

We begin the empirical investigation by analyzing home purchases of young individuals around the introduction of the transfer tax exemption. Each month before the reform, we observe around 4,500 home purchases in our baseline sample. This corresponds to an individual monthly purchase probability of 0.28%. To distinguish the policy's causal effect from overall housing market trends, we compare eligible and ineligible individuals using a difference-in-differences framework. Concretely, individuals aged 30–34 form the treatment group and those aged 38–42 form the control group.[3] The identifying assumption is that the home purchase behavior of both groups would continue to move in parallel, absent the introduction of the tax exemption for young buyers.

In Figure 1, we plot the difference in monthly purchase probabilities between the two groups (blue bars), normalized to zero in the second quarter of 2020.[4] Despite some differences between the two groups, we find no systematic deviations from parallel trends prior to the policy change being first discussed. In 2020Q3, just around the official announcement of the policy change, there was increased activity by younger buyers. We attribute this temporary divergence to the market turmoil around the Covid-19 pandemic, rather than to a causal anticipation effect.[5] In 2020Q4, immediately before the policy was implemented, individuals under 35 signed substantially fewer contracts. In turn, we observe a strong spike in purchases in 2021Q1, when the transfer tax exemption was enacted but did not yet feature the €400,000 price limit. After a cooldown period following the introduction of the price limit in 2021Q2, the purchase volume of younger individuals remained systematically elevated until the end of our study period in December 2023.

We formalize this graphical comparison by running the following dynamic difference-in-differences regression on the above sample:

$$Purchase_{it} = \sum_{k \neq 20Q2} \beta_k \cdot \mathbb{1}_{(Age_{it}<35)} \cdot \mathbb{1}_{(t=k)} + \gamma_t + \delta \cdot \mathbb{1}_{(Age_{it}<35)} + \varepsilon_{it}, \tag{1}$$

[3] We exclude individuals aged 35–37 from this analysis as individuals in that age range switch from the treatment to the control group during our study period. Consider, e.g., an individual turning 35 in January 2023. This person was treated for two years, but is no longer eligible from then onward, and would erroneously become part of the control group if we included individuals aged 35–37.

[4] Before the reform, the younger group is around 0.12 pp more likely to purchase a home in any given month. Appendix Figure B.2 depicts the raw time series of monthly home purchases for both groups.

[5] The transaction date in our data reflects the signing of the notarial deed, which typically occurs weeks after negotiations. It is therefore unlikely that the announcement in Q3 affects purchases in the same quarter. If buyers were anticipating the tax exemption, we would expect a decline in purchases, as observed in 2020Q4. The uptick in young buyers' purchases during the early months of the pandemic is also consistent with patterns observed in other countries, suggesting a Covid-19 rather than a policy effect (see, e.g., Appendix Figure B.3 for corresponding evidence based on US data).

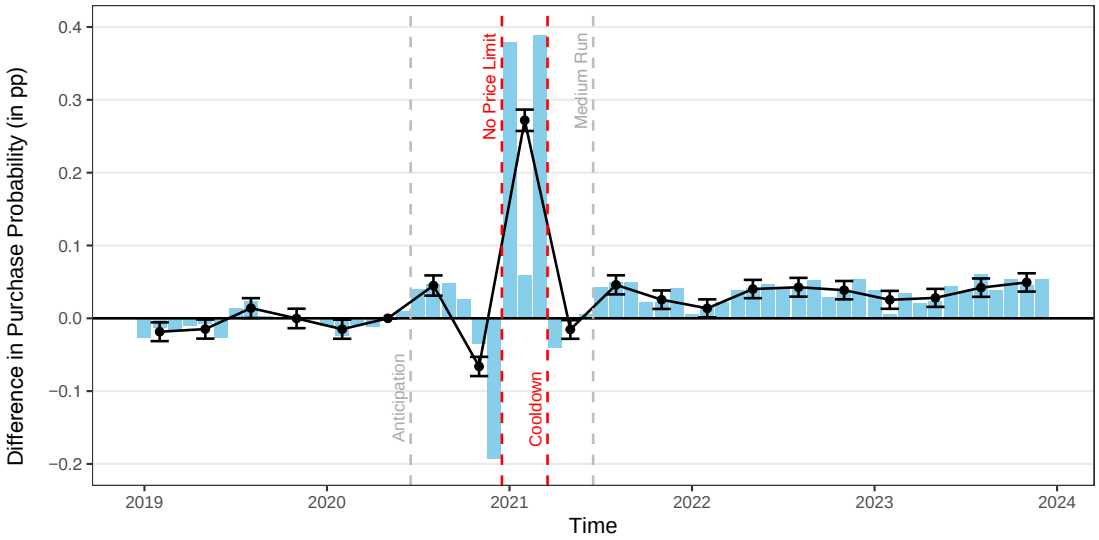


Figure 1: The Effect of the Transfer Tax Exemption on Home Purchases

Notes: This figure shows the difference in home purchase probabilities (in percentage points) between individuals aged 30–34 and those aged 38–42 between January 2019 and December 2023. Blue bars show raw, monthly differences in the share of homebuyers among both groups (normalized to zero in 2020Q2). Black dots depict quarterly averages in these differences estimated based on Equation (1). Differences are normalized to their 2020Q2 level, before the announcement of the policy. Black vertical bars indicate 95% confidence bounds. Raw time series of the monthly home purchasing behavior in both groups and depicted regression results for each quarter can be found in Appendix B.1.

where the dependent variable is an indicator equal to one if an individual i purchased a home in a given period t, and zero otherwise. The explanatory variable of interest is a binary indicator for those aged 30–34 in a given month, and γ_t is a period fixed effect informed by the purchase behavior of individuals aged 38–42. The treatment estimates $\widehat{\beta}_k$ capture the difference in average monthly home purchase probabilities between younger and older individuals, relative to 2020Q2, the omitted reference period. Standard errors are clustered at the individual level.

The resulting estimates are shown in black in Figure 1. To summarize the policy's impact, we further aggregate the estimates for the five subperiods indicated in the figure. Column (1) of Table 1 presents the resulting coefficients. As the figure suggests, pre-trends are largely flat before the policy announcement. After the announcement, but prior to implementation, the average monthly purchase probability of eligible buyers declines by 0.011 pp. In the initial period without a price limit, purchases by young buyers rise sharply by 0.272 pp. Their purchase probability thus doubles during this period. After a short cooldown in 2021Q2, following the introduction of the price limit, we estimate a medium-run effect of the policy of around 0.035 pp. This translates into a 12.5% increase, relative to the reference period.

Heterogeneous Effects. Financial relief is likely most beneficial to those young homebuyers facing a binding borrowing constraint. Some countries explicitly target this group by offering support only to low-income homebuyers. In the Netherlands, loan-to-income

Table 1: The Effect of the Transfer Tax Exemption on Home Purchases

		By Liquid Assets		By Income		By Ownership	
	Base-line	First Quartile	Fourth Quartile	First Quartile	Fourth Quartile	Non-Owner	Exist. Owner
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated ×							
Pre-Policy Period (2019Q1–2020Q1)	−0.007	−0.017**	0.027**	−0.031***	−0.002	−0.006	−0.006
	(0.005)	(0.008)	(0.012)	(0.008)	(0.012)	(0.007)	(0.008)
Anticipation Period (2020Q3–2020Q4)	−0.011*	−0.006	−0.036***	−0.001	−0.020	−0.018**	−0.001
	(0.006)	(0.010)	(0.013)	(0.009)	(0.013)	(0.009)	(0.008)
No Price Limit Period (2021Q1)	0.272***	0.133***	0.350***	0.175***	0.401***	0.294***	0.250***
	(0.008)	(0.012)	(0.016)	(0.011)	(0.017)	(0.011)	(0.010)
Cooldown Period (2021Q2)	−0.015**	0.019*	−0.069***	0.038***	−0.105***	0.007	−0.048***
	(0.007)	(0.011)	(0.014)	(0.010)	(0.014)	(0.010)	(0.008)
Medium Run (2021Q3–2023Q4)	0.035***	0.043***	0.007	0.064***	0.010	0.016**	0.038***
	(0.005)	(0.008)	(0.011)	(0.007)	(0.011)	(0.008)	(0.007)
Person-by-Quarter Observ.	33.3mn	8.3mn	8.3mn	8.3mn	8.3mn	13.9mn	17.8mn

Notes: This table summarizes the estimated effect of the transfer tax exemption on the monthly purchase probability among individuals aged 30–34. Coefficients are to be interpreted as percentage point changes relative to the omitted quarter 2020Q2. Estimated treatment effects $\widehat{\beta}_k$ are based on the difference-in-differences model specified in Equation (1). The estimation is based on a person-by-month panel dataset from January 2019 until December 2023. Standard errors, clustered at the individual level, are reported in parentheses (significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

limits are strict, such that individuals with low incomes are most likely to be constrained. Similarly, although Dutch loan-to-value limits are relatively generous, we expect those with few liquid assets to be constrained, as transfer taxes and other transaction costs must be paid out of pocket. To investigate heterogeneity in treatment effects across these dimensions, we identify each individual's disposable household income and bank balance, respectively, and classify individuals into quartiles based on the 2019 distribution of both measures. We then re-estimate Equation (1) via sample splits for each population quartile.

The resulting estimates are reported in columns (2)–(5) of Table 1. We first note that pre-trends become somewhat unstable when cutting along these margins.[6] After the introduction of the policy, two patterns stand out. On the one hand, the spike in purchases during the initial period without a price limit (2021Q1) is most pronounced among individuals with higher assets and income. On the other hand, we observe a lasting medium-run increase in purchases only among those with fewer financial resources. Taken together, these findings suggest that price limits—a common feature of many young-buyer support policies around the world—can be effective in targeting financial support to the more-constrained individuals.

[6] The quarter-by-quarter estimates still exhibit clear trend breaks and sharp changes in purchase behavior exactly around the policy cutoff dates despite these precisely estimated small deviations before the reform. We relegate event study plots and further regression results, as well as details on the temporal stability of these population quartiles relative to their 2019 distributions, to Appendix B.2.

Finally, despite its name, the Dutch starter tax exemption was not restricted to first-time homebuyers. Therefore, next to promoting access for aspiring homeowners, it offered existing owners the chance to trade up. We examine the policy's impact on these two groups separately, by splitting the sample into individuals who already owned a property in the previous quarter (existing owners) and those who did not (non-owners). The resulting estimates are reported in columns (6) and (7) of Table 1. Strikingly, we find that the majority of the policy's medium-run effect is driven by existing owners making another purchase. Whereas the monthly volume of first-time purchases increased by only 0.016 pp, repeat purchases rose by 0.038 pp. This suggests that the policy gave a strong incentive for existing owners to trade up.[7]

Crowding Out of Older Homebuyers. A potential concern with our identification strategy is that individuals from both the treatment and the control group may compete for similar homes. Subsidizing the younger group could then lead to a crowding out of older homebuyers from the market if supply is sufficiently inelastic. In that case, the estimated treatment effect ($\widehat{\beta}_k$) would be biased upward. We address this concern by exploiting cross-sectional policy variation stemming from the €400,000 price limit that was introduced on top of the age restriction in 2021Q2. As the tax exemption applied only to units in the lower segments of the market, potential crowding out would be expected to be most pronounced in those segments as well.

To investigate this prediction, we rerun the difference-in-differences analysis from Equation (1) but replace the dependent variable $Purchase_{it}$ for any purchase with indicators $Purchase^s_{it}$ that are equal to one only if an individual purchased a home in a given price segment s. We assign segments by splitting all transacted units into two groups based on their 2019 (i.e., pre-policy) tax-assessed value. We define units with tax values below €250,000 as low-value and those with higher tax values as high-value. This value corresponds roughly to the median tax value in 2019, and therefore provides a useful ex ante proxy for the policy's €400,000 price limit. Whereas 87% of low-value units sold below the price limit—and therefore qualified for the tax exemption—only 19% of high-value units did. The distinction between low- and high-value units will be revisited in Section 3.5.

In Figure 2, we compare the policy's effect on purchases between low- and high-value units.[8] In contrast to the baseline results, we visualize not only the estimated treatment effect (in black), but also quarterly fixed effects (in grey, normalized to 2020Q2), which capture average purchase probabilities among older individuals. This is central to our test of crowding out: if the subsidy for young homebuyers has a direct negative effect on older ones, then their purchase activity should decline most in the lower-price segment, where the policy had the most bite. However, the results do not support this. Whereas

[7] We provide further details on the purchases of both groups in Appendix Figure B.7.
[8] Appendix B.3 provides more detailed results from this analysis.

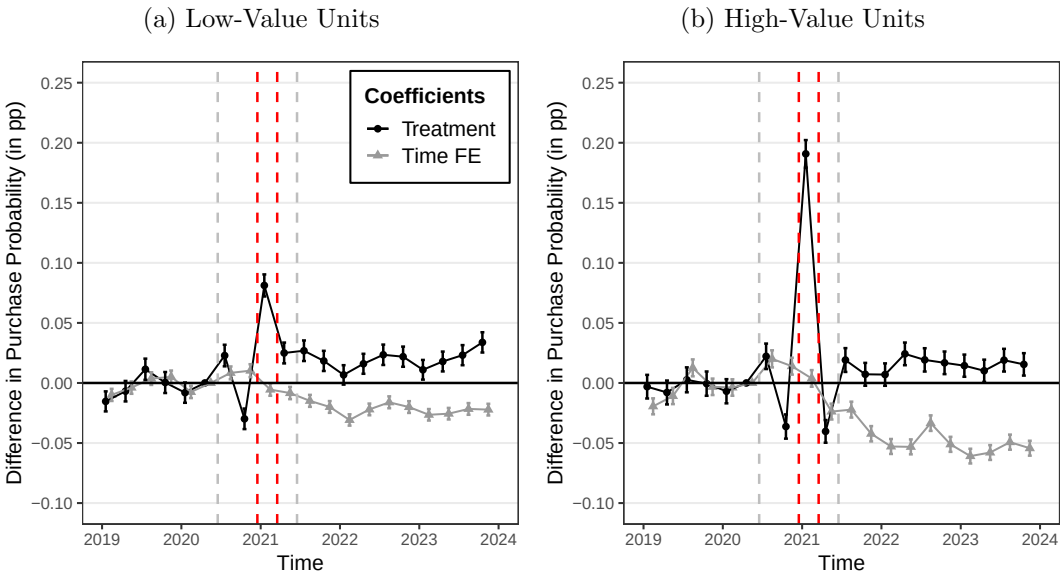


Figure 2: Testing Crowding Out by Comparing Market Segments

Notes: This figure shows differences in home purchase probabilities separately for low- vs. high-value units, as defined by their 2019 tax-assessed value. The coefficients are based on Equation (1). However, in contrast to Figure 1, we here also visualize time fixed effects (in grey), normalized to their 2020Q2 level. These capture the purchase behavior of individuals in the control group, i.e., those aged 38–42. Vertical bars indicate 95% confidence bounds.

purchases by older buyers decrease somewhat among lower-value and qualifying units (left panel), the decrease is much more pronounced and statistically larger among higher-value, non-qualifying units (right panel). Although we cannot disentangle changes in the purchasing behavior of older buyers from overall secular trends in home purchases, these patterns suggest that the primary effect of the exemption was to increase purchases among eligible buyers, rather than to displace purchases by ineligible buyers. Such an expansion of the buy-to-own segment is also consistent with the rising share of buy-to-own transactions on the Dutch housing market in recent years (cf. Appendix Figure B.1).

3.2. Timing Responses

The difference-in-differences analysis reveals that the transfer tax cut causally increased home purchases among eligible individuals. A central follow-up question is whether the additional transactions correspond to an extensive-margin response—and therefore to a lasting increase in homeownership rates—or merely reflect short-term timing responses. Making this distinction is crucial for policy and welfare implications. For individuals who would have only bought slightly later (or earlier) without the tax exemption, the main benefit is a financial windfall. In contrast, those who would never have purchased absent the policy—or only much later—may gain additional utility from homeownership and higher housing consumption. We will investigate timing responses more closely in this subsection. Section 3.3 focuses on identifying extensive-margin responses.

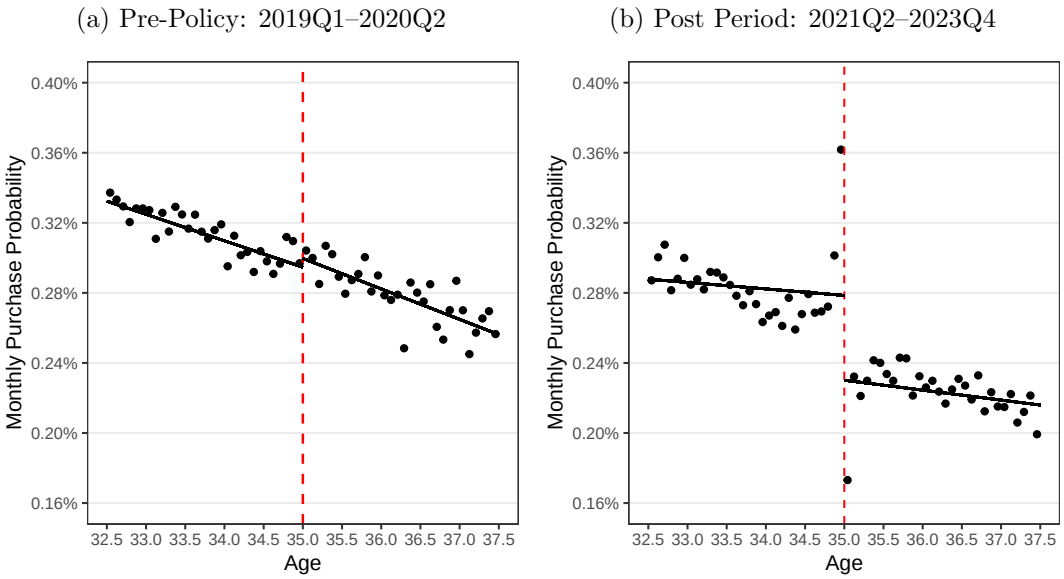


Figure 3: Regression Discontinuity Evidence on the Transfer Tax Exemption

Notes: This figure shows the average monthly home purchase probability by age in months during our sample period. Panel (a) shows pre-policy evidence for the period from 2019Q1 until 2020Q2. Panel (b) is based on the period from 2021Q2 until 2023Q4. The solid black lines represent estimates from a linear RD model as specified in Equation (2). Corresponding estimation results are provided in Table 2. Complementary visualizations for regression-discontinuity models that exclude specific age-in-month groups around the threshold are presented in Appendix Figure B.9 ("donut RDs").

Bunching Around Policy Dates. The first piece of evidence for timing responses comes from Figure 1 above, suggesting that already-planned home purchases are retimed around policy cutoff dates. As discussed in Section 3.1, purchases by young buyers declined just before the introduction of the tax exemption and rose sharply immediately afterward, indicating substantial bunching in the initial three months when no price limit applied. A back-of-the-envelope calculation of the monthly differences in purchases between November 2020 and June 2021 suggests that at least 57% of the strong policy response in the initial quarter is attributable to timing effects within this narrow eight-month window.[9] It is likely that some individuals brought forward their home purchases by even longer periods, given that the end of the initial period and the introduction of the price limit in April 2021 were known well in advance.

Bunching Around Birthdays. Another cutoff date that young buyers may use to optimize the timing of their purchases is their 35th birthday, which marks the eligibility threshold. We investigate this optimization strategy in a regression-discontinuity design. Figure 3 plots the monthly purchase probabilities for people aged between 32.5 and 37.5, i.e., thirty months on either side of the age threshold. Purchase probabilities are smooth before the tax exemption is introduced (panel (a)), whereas we find a sharp discontinuity in purchases when turning 35 after the policy and the price limit are in

[9] The details of this calculation can be found in Appendix B.4, Table B.2.

Table 2: Timing Responses to the Transfer Tax Exemption

	2021Q2–2023Q4				2021Q1	≤'20Q2
	Baseline	±2 Months Omitted	±4 Months Omitted	±6 Months Omitted	No Limit Period	Pre Policy
	(1)	(2)	(3)	(4)	(5)	(6)
Treated ($Age < 35$)	0.048***	0.021***	0.014***	0.010*	0.213***	−0.005
	(0.004)	(0.004)	(0.005)	(0.006)	(0.017)	(0.006)
Person-by-Month Obs.	28.9mn	26.2mn	24.3mn	22.5mn	2.5mn	14.6mn

Notes: This table summarizes the estimated effect of the transfer tax exemption on the purchase probabilities of eligible individuals. All coefficients are to be interpreted as percentage point differences. We report discontinuities in the monthly purchase probability around the 35th birthday (cf. Figure 3). Estimated treatment effects $\widehat{\beta}$ are based on the regression discontinuity model specified in Equation (2), with the running variable being an individual's current age, measured at a monthly frequency and recentered around the 35th birthday. The sample includes 30 monthly cohorts on either side of the cutoff. Standard errors, clustered at the individual level, are reported in parentheses (significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

place (panel (b)). We complement the graphical evidence with a linear RD model, estimated on the pooled individual-by-month panel, separately for the periods before and after the policy introduction:

$$Purchase_{it} = \alpha + \beta \cdot \mathbb{1}_{(Age_{it}<35)} + \gamma \cdot Age_{it}^{(35)} + \delta \cdot \mathbb{1}_{(Age_{it}<35)} \cdot Age_{it}^{(35)} + \varepsilon_{it}, \qquad (2)$$

where the dependent variable is a purchase indicator and $Age_{it}^{(35)}$ denotes an individual's current age, recentered around their 35th birthday. The coefficient of interest, β, captures the discontinuity in purchasing behavior at the threshold. Estimation results are reported in Table 2. The baseline model, reported in column (1), suggests that the purchase probability drops significantly by 0.048 pp when turning 35.

However, as is evident from Figure 3, the discontinuity is largely driven by the purchasing behavior a few months around the 35th birthday.[10] We quantify the impact of retimed purchases in this narrow age range by implementing a set of "donut" RD models. Estimated discontinuities shrink when excluding two or four months on either side of the cutoff. Excluding half a year on the left and on the right of the cutoff lowers the linear RD estimate to 0.01 pp with a t-statistic of only 1.76 (see Table 2, columns (2)–(4); corresponding donut RD plots are relegated to Appendix Figure B.9). We conclude that at least 80% of the policy's effect on purchase activity around the 35th birthday is driven by short-lived retiming of purchases rather than by additional extensive-margin transactions.

These estimates likely still overestimate the additional transactions resulting from the policy. An RD analysis of the tax exemption faces the inherent challenge that individuals move along the running variable over time. Each period, some individuals switch from the treatment to the control group as they turn 35 and cross the age threshold.

[10] More flexible functional forms in individuals' age would thus yield even larger RD estimates.

The longer the policy has been active, the greater the number of individuals aged 35 or older who had been eligible before. Their home purchases may thus understate the counterfactual homebuying activities of older cohorts if they had never qualified for the policy. Although we find significant discontinuities in each quarter since policy implementation in 2021Q1, these estimates likely overstate the true discontinuity, especially in later quarters (see Appendix Figure B.10 for quarter-by-quarter RD graphs).

3.3. Extensive-Margin Responses

The preceding analyses suggest that the policy created strong retiming responses. We proceed by analyzing homeownership responses directly. A first piece of evidence against extensive-margin responses comes from the split between first-time and repeat purchases. As documented in Table 1, the majority of additional transactions resulting from the policy involve existing homeowners, who, by definition, cannot contribute to higher ownership rates. Nevertheless, even among new entrants, Table 1 suggests a detectable lasting increase in homeownership. Cumulating coefficients suggests that ownership would rise 0.35 pp (se = 0.03) above the counterfactual level for the 1986 cohort, who were eligible for the tax exemption between 1 and 12 months. For the 1987 cohort, eligible between 13 and 24 months, ownership should rise by 0.46 pp (se = 0.07).[11]

Differences in Homeownership by Cohort and Age. To investigate ownership responses, we now take a life-cycle perspective and follow individuals from the same birth-year cohort over time to track changes in homeownership status. This cannot be done in our baseline difference-in-differences framework specified in Equation (1), since individuals from different cohorts are at different stages of their life cycle in any given year, leading to non-parallel trends when it comes to stock variables like ownership.[12]

In Figure 4, we depict the homeownership rate for the three birth-year cohorts 1985, 1986, and 1987 over their life cycle using administrative wealth records.[13] Ownership rates rise from close to zero in people's early twenties to around 70% in their late thirties

[11] Based on average monthly treatment effects from column (6) of Table 1, a person born in January 1986 was exposed to one month of "no price limit" period, from which we subtract a three-month anticipation effect: $3\cdot(-0.018)+0.294=0.24$. People born in February have $3\cdot(-0.018)+2\cdot0.294=0.534$. Those born in December 1986 are exposed to three months of "no price limit" period, followed by three months of cooldown period and six months medium-run: $3\cdot(-0.018)+3\cdot0.294+3\cdot0.007+6\cdot0.016=0.945$. Averaging over the year yields an effect of 0.797 pp. Using the share of not-yet-owners from Table 1, this suggests a cohort-wide increase in ownership of 0.349 pp. A corresponding calculation for the 1987 cohort (exposed between 13 and 24 months) suggests an imputed increase of 0.46 pp.

[12] Appendix Figure B.11 illustrates these trends among birth-year cohorts throughout the period of our study. The previous analyses assigned treatment based on current age and thus did not follow the same individuals over time. In contrast, treatment is stable once assigned based on birth dates, which allows us to follow (ever) treated individuals over their life cycle.

[13] Wealth records date back further than the official ownership register, but unfortunately do not contain individual-level information on homeownership. Instead, we identify people as owners if they (i) live outside the parental home and (ii) belong to an owner-occupied household. Ownership rates defined that way are similar to those obtained from the ownership register.

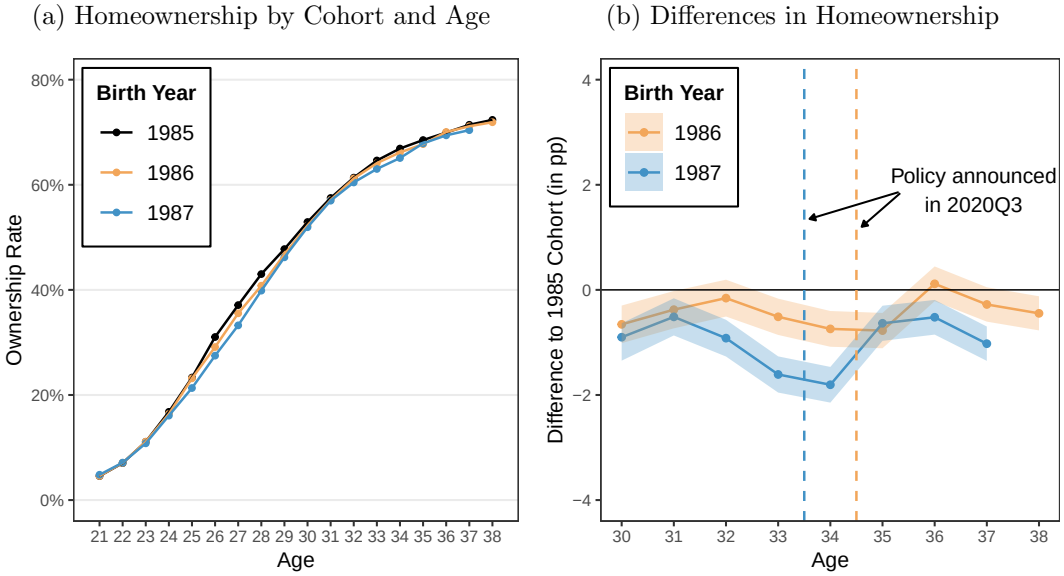


Figure 4: Homeownership Effects of the Transfer Tax Exemption

Notes: This figure shows life-cycle patterns of homeownership for birth-year cohorts close to the policy's eligibility cutoff. Individuals born in 1985 (black line) were never eligible for the policy, while those born in 1986 (orange line) and 1987 (blue line) were eligible for up to one and two years, respectively. While panel (a) plots raw ownership rates, panel (b) tracks differences in ownership among the two eligible cohorts, relative to the 1985 cohort, for different ages. Estimates are based on Equation (3), with blue and orange bands indicating 95% confidence intervals. Corresponding estimation results are provided in Table 3.

(panel (a)). These trends are similar for all three cohorts. Deviations across cohorts are most pronounced during their late twenties.

In panel (b), we zoom in on the years around the policy introduction when the cohorts were between 30 and 38 years old. We then depict the difference in ownership rates by age for the two later-born, treated cohorts relative to the never-treated 1985 birth-year cohort. At any given age in their thirties, later-born cohorts have around 0.5 pp–1 pp lower ownership rates. This gap largely disappears for both cohorts in the year the transfer tax exemption is introduced, indicating a significant increase in homeownership attributable to the policy. However, the ownership gap widens again in the years after, indicating that this extensive-margin effect was only short-lived.

To formally quantify these cohort differences, we run the following regression on the administrative wealth panel with individuals from the three birth-year cohorts:

$$Owner_{it} = \sum_{a=30}^{38} \alpha_a \cdot \mathbb{1}_{(Age_{it}=a)} + \sum_{a=30}^{38} \sum_{b='86}^{'87} \beta_a^b \cdot \mathbb{1}_{(Age_{it}=a)} \cdot \mathbb{1}_{(Birthyear_i=b)} + \varepsilon_{it}, \tag{3}$$

where the dependent variable is an ownership indicator. Estimates $\widehat{\beta_a^b}$ measure the ownership gap between birth-year cohort b and the 1985 control cohort at age a (as depicted in panel (b) of Figure 4). Using these estimates, we conduct post-estimation difference-in-differences comparisons. Consider the birth years 1985 and 1986: the former cohort is never eligible for the exemption, whereas the latter cohort is eligible for up to

Table 3: Extensive-Margin Responses to the Transfer Tax Exemption

	Cohort 1985 vs. 1986			Cohort 1985 vs. 1987		
	Age 33 vs. 36 (1)	Age 32 vs. 37 (2)	Age 31 vs. 38 (3)	Age 32 vs. 35 (4)	Age 31 vs. 36 (5)	Age 30 vs. 37 (6)
Treated (Cohort 1986 or 1987) × Post	0.628***	−0.121	−0.070	0.283**	−0.008	−0.033
	(0.126)	(0.150)	(0.168)	(0.132)	(0.158)	(0.176)
Person-by-Year Obs.	3.4mn					

Notes: This table summarizes the estimated effect of the transfer tax exemption on homeownership rates of eligible individuals. All coefficients are to be interpreted as percentage point differences. We present difference-in-differences comparisons between the birth-year cohorts 1985 (never treated), 1986, and 1987 (treated for up to one and two years, respectively) at given ages (cf. Figure 4, panel (b)). We report differences in estimates, i.e., $\widehat{\beta_k^b} - \widehat{\beta_j^b}$, for each treated cohort b and various age pairs (j, k) in percentage points, based on the model specified in Equation (3). We select age pairs around the policy introduction of varying length with $j \in [30, 33]$ and $k \in [35, 38]$. Standard errors, clustered at the individual level, are reported in parentheses (significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

one year. We now compare the cohort differences in ownership rates at age 33, before the policy is announced, and age 36, when neither cohort is eligible anymore.

The difference-in-differences estimate is given by $\widehat{\beta_{36}'^{86}} - \widehat{\beta_{33}'^{86}} = 0.63$ pp, which is highly significant as depicted in column (1) of Table 3. However, widening the observation window and comparing the same cohorts at ages 32 vs. 37 (column (2)), or 31 vs. 38 (column (3)), entirely removes the estimated difference. Columns (4)–(6) of the table illustrate a similar pattern when comparing the 1987 to the 1985 cohort. The 95% confidence intervals around the estimates presented in columns (3) and (6) exclude lasting ownership effects exceeding 0.26 pp and 0.31 pp for the 1986 and 1987 cohorts, respectively. These numbers are significantly below the implied homeownership effects that result from aggregating monthly purchase estimates for both cohorts ($p < 0.01$).

Note that the 1987 cohort was eligible for up to two years, but the increase in ownership rates is still almost fully concentrated in the first year of eligibility (see blue curve in panel (b) of Figure 4). We interpret these results as further evidence of retiming responses, particularly because of the initial quarter without a price limit. The transfer tax exemption raised homeownership rates among treated individuals, but only in the first year of the policy and only for a short time. A few years after policy introduction, ownership rates are back to their pre-reform counterfactual.

These cohort comparisons have to be interpreted with caution, since trends differed somewhat in the years immediately before the policy was enacted. We argue that these divergences were still roughly within the normal fluctuations as seen in a longer-run comparison, despite their statistical significance. Ownership rates diverged much more during ages 25–29, but caught up a few years later. Also, from an ex ante perspective, we do not expect substantially different long-term trends between these three cohorts of almost the same age absent the tax exemption.

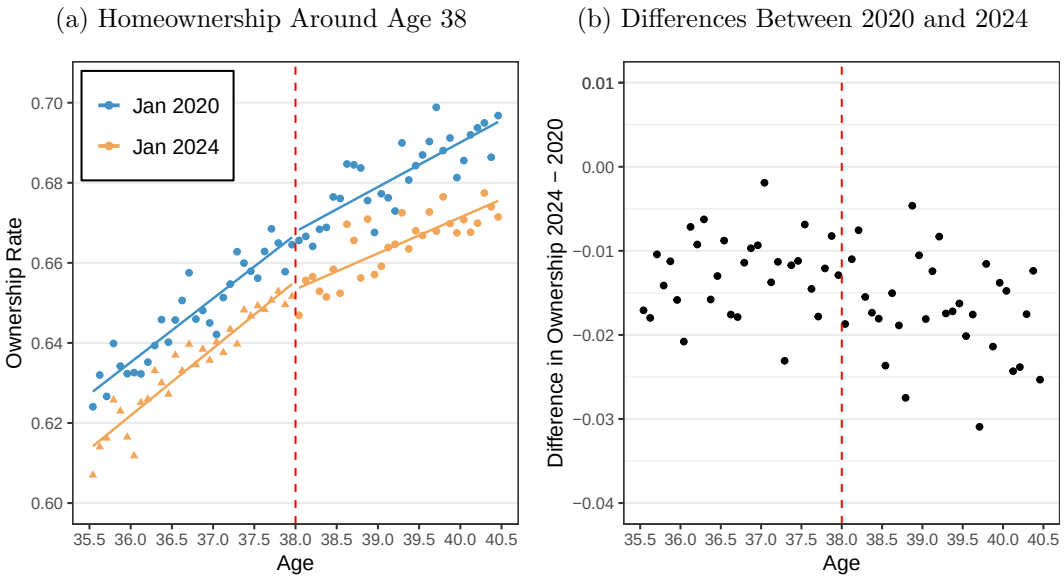


Figure 5: Homeownership Effects of the Transfer Tax Exemption II

Notes: This figure shows homeownership rates by age for birth-year cohorts close to the policy's eligibility cutoff. We depict homeownership rates for individuals aged between 35.5 and 40.5 in 2024 (in orange) and 2020 (in blue). Individuals to the left of the cutoff at age 38 in 2024 were eligible for some time to the starter tax exemption. Older individuals were never eligible. The 2020 data yield a placebo, since the data refer to January 1, when the policy had not yet been announced. Panel (a) plots raw ownership rates, panel (b) tracks differences by age between the two years. Fitted lines correspond to linear RDD models. Corresponding estimation results are provided in Appendix Table B.3, panels B and C.

Discontinuities in Homeownership. A similar picture emerges when analyzing ownership responses in the administrative homeownership register, which records the owner(s) of each unit in the Netherlands as of January 1 in a given year. Using annual snapshots of this dataset, we compare birth-year-month cohorts born just before the eligibility cutoff (December 1985 or earlier) to those born slightly later. Panel (a) of Figure 5 shows the corresponding ownership rates for 2024, the last data year, around age 38 (in orange). Younger individuals on the left-hand side of the figure were eligible for the starter tax exemption for some time. Individuals to the right were already older than 35 when the policy was introduced and thus never eligible (birth years up to 1985). Fitted linear lines exhibit both a small jump and a small kink in ownership rates, suggesting a policy effect. However, when analyzing the same threshold in the 2020 snapshot, when the transfer tax exemption had not yet been passed or announced, we find a similar pattern for individuals aged 35–38 (in blue). It is thus likely that the curvature by age reflects merely the secular concave pattern of homeownership over the life cycle instead of a policy effect. This is also suggested when plotting the difference between both years around age 38, which exhibits neither jump nor kink (see panel (b)).

We scrutinize this intuition more carefully by tracking the birth-year-month cutoff at January 1, 1986 back in time, and running auxiliary RD models for discontinuities at that threshold. The estimate from January 1, 2024 amounts to 0.24 pp with a standard error of 0.21 (corresponding to the gap between the orange lines in Figure 5). Before the

policy, estimates are positive at around 0.4 pp, at the margin of statistical significance. On January 1, 2021, the gap decreases to 0.2 pp, which mirrors the observation that soon-eligible individuals delayed their purchases in late 2020 to benefit from the subsidy a few weeks later. By January 2022, one year into the policy, we observe an 0.5 pp increase in ownership among treated individuals, indicating a causal response to the policy. The estimate is statistically significant at the 5% level but not significantly different from the pre-policy discontinuity of 0.4 pp. Moreover, this increase vanishes by January 1, 2023, when the estimated gap returns to and even stays below its pre-policy magnitude. Taken at face value, these estimates imply (i) an anticipation response, and (ii) a small, temporary increase in the first year, but (iii) a relatively precisely estimated null effect of the tax exemption on homeownership in the medium run. More flexible quadratic or cubic lines of fit hardly add explanatory power, but introduce noise. We relegate detailed estimation results to Appendix Table B.3.[14]

Longer-Term Perspective. We complement this picture with the longer-term perspective for all birth-year cohorts 1981–1990 in each calendar year since 2006. In line with the previous analyses, we find a small, temporary dip in 2021 followed by a corresponding uptick in 2022 among treated cohorts, but no long-term effect on ownership (see Appendix Figure B.11). Definitively evaluating the policy's long-term effect on homeownership would require data spanning a longer period after the policy's introduction, enabling a comparison between treated and untreated individuals later in life—once most homeownership decisions have been made. However, we argue that it is unlikely that the effect on homeownership emerges only years or decades after the policy's introduction, especially given that most activity occurs during the brief initial policy period without a price limit. We therefore conclude that the primary impact of the tax exemption for young buyers was to accelerate the timing of home purchases for individuals who would have bought only slightly earlier or later in the absence of the policy.

3.4. Intensive-Margin Responses

We next turn to changes in the type of homes bought. To analyze such intensive-margin responses, we re-estimate the difference-in-differences model from Equation (1) for various transaction characteristics. Table 4 reports the estimation results aggregated to the five subperiods that were introduced above.[15]

[14] The institutional setting carries the notion of a regression kink design by birth-year-month cohorts, since individuals born in January 1986 were eligible for one month, the February cohort for two months, those born in March for three months, etc. We argue that the long anticipation period and the strong initial response make it likely that the January cohort had sufficient time to make use of the policy. Moreover, we consider it implausible that the use of the policy increases linearly with each month of eligibility. Still, we also provide regression kink estimates in Appendix Table B.3, finding no change in slope for the younger cohorts between 2020 and 2024.

[15] Appendix B.5 provides the corresponding event-study graphs and examines heterogeneous effects among first-time and repeat buyers.

Table 4: Intensive-Margin Responses to the Transfer Tax Exemption

	Purchase Amount (1)	Tax Value (2)	Floor Area (3)	Postcode Income (4)	Over-bidding (5)
Treated ×					
Pre-Policy Period (19Q1–20Q1)	−40.5*	0.000	−0.001	−0.002	−0.002
	(21.5)	(0.009)	(0.009)	(0.003)	(0.002)
Anticipation Period (20Q3–20Q4)	−44.0*	0.005	0.016	0.000	−0.002
	(25.0)	(0.010)	(0.010)	(0.003)	(0.002)
No Price Limit Period (21Q1)	1,270.7***	0.055***	−0.004	0.008**	−0.002
	(33.8)	(0.012)	(0.011)	(0.003)	(0.002)
Cooldown Period (21Q2)	−151.6***	−0.121***	−0.040***	−0.029***	0.009***
	(28.1)	(0.012)	(0.012)	(0.004)	(0.003)
Medium Run (21Q3–23Q4)	240.1***	−0.021**	0.014	−0.007***	−0.003**
	(21.0)	(0.009)	(0.009)	(0.003)	(0.002)
Number of Observations	33mn	238k	116k	244k	116k

Notes: This table summarizes the estimated effect of the transfer tax exemption on home purchase behavior among individuals aged 30–34 along various intensive-margin dimensions. Corresponding event-study graphics are shown in Appendix Figure B.12. Estimated treatment effects $\widehat{\beta}_k$ are based on the difference-in-differences model specified in Equation (1). Column (1) is based on the baseline person-by-quarter panel dataset. Estimates in columns (2) and (4) come from a transaction-level dataset including all purchases involving buyers aged 30–34 and those aged 38–42. Estimates in columns (3) and (5) are based on a subset of transactions for which we have additional broker data from NVM. Treatment is defined either at the individual level (column (1)) or at the buyer level (columns (2)–(5)). Dependent variables are: (1) an individual's monthly home purchase volume in euros, (2) the natural logarithm of a unit's tax-assessed value as of January 2019, (3) the natural logarithm of a unit's floor area, (4) the natural logarithm of median postcode income in 2018, and (5) the log ratio between a unit's final sale price and the initial list price. Appendix Table B.4 provides additional evidence from splitting the sample between first-time and repeat buyers and estimating heterogeneous effects. Standard errors, clustered at the individual/transaction level, are reported in parentheses (significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

We begin on the baseline individual-level panel and examine how the policy affected the monetary home purchase volume of younger, eligible individuals relative to older ones. In line with the estimates for the binary purchase indicator (Section 3.1), we find that average purchase amounts spike significantly by €1,271 in the initial three-month period without a price limit, and stabilize around an additional €240 in the medium run (see column (1) of the table). In addition to the increased likelihood of making a purchase at all, this effect reflects changes in the selection of homes, as well as price changes.

To investigate these additional margins of adjustment, we shift attention away from purchase probabilities and turn to the transaction-level data, comparing purchases by individuals 30–34 with those of individuals 38–42 for the remainder of this section. The resulting sample includes around 240,000 transactions during our study period. In column (2), we present estimates for differences in the average (2019-level) tax-assessed value as a proxy for the quality of a unit. We find a highly significant 6% increase in the average tax value in the initial period, suggesting that eligible homebuyers systematically purchased higher-value units in the three months while those units were also exempt

from transfer taxation. In contrast, we find a significant 2% drop in average tax values among young buyers in the medium run. This finding most likely reflects the incentive for eligible homebuyers to stay below the price limit after April 2021 (see also paragraph "The Role of the Price Limit" below).

In column (3), we investigate a unit's floor area as an additional quality measure (based on a subsample with broker data). We find little systematic evidence for any policy responses outside the brief cooldown period from April to June 2021. In column (4), we shed light on the type of neighborhood in which treated homebuyers purchase their home. Using pre-policy postcode-level median income as a proxy for neighborhood quality, we find a qualitatively similar pattern to that observed for unit-level tax values. Young homebuyers move their attention to higher-income neighborhoods during the initial period without a price limit. In contrast, they are significantly more likely to buy in lower-income areas after the introduction of the price limit.

Finally, we examine bidding behavior, again relying on the broker data, which include both sale prices and initial list prices for each available transaction. To capture overbidding, we calculate the log-difference between these two prices. We find little evidence for changes in bidding behavior by eligible homebuyers in response to the tax exemption.

The Role of the Price Limit. The estimated intensive-margin effects for the assessed value and the neighborhood income align with the incentive created by the tax notch at €400,000 introduced in April 2021. Young buyers have a strong incentive to stay below the price limit since their tax liability jumps from zero to more than €8,000 at the threshold. The behavioral response to the transfer tax notch becomes clearly visible when analyzing bunching around the cutoff. Panel (a) of Figure 6 depicts the average monthly total of transactions by young homebuyers for €5,000 bins around the exemption limit (in a €50,000 window below and above the price limit). Before the transfer tax exemption, the number of transactions declined slightly but smoothly throughout the price window. Since 2021Q3, the price bin just before the cutoff exhibits substantial excess mass; transaction numbers have more than doubled.[16] The histogram also exhibits missing mass just above the price limit, suggesting that the excess transactions below the cutoff came from above.

We further corroborate the hypothesis that the prices of excess transactions were artificially low in panel (b) of the same figure. To test this, we predict the expected sale price for each traded unit based solely on its pre-determined tax-assessed value and postcode-by-quarter fixed effects. We then sort units into €5,000 bins by expected price (rather than realized price) and calculate the share of units that eventually trade below their predicted price within each bin. Panel (b) illustrates that around 50–60% of units purchased by young buyers were sold at prices below the expected price, both

[16] An analysis by the Dutch Ministry of Finance documents this bunching in more recent years as well, using tax administration data (Kuiper and Maandag, 2025). They also document that transaction prices adjust with some temporal lag to recent changes in the price limit.

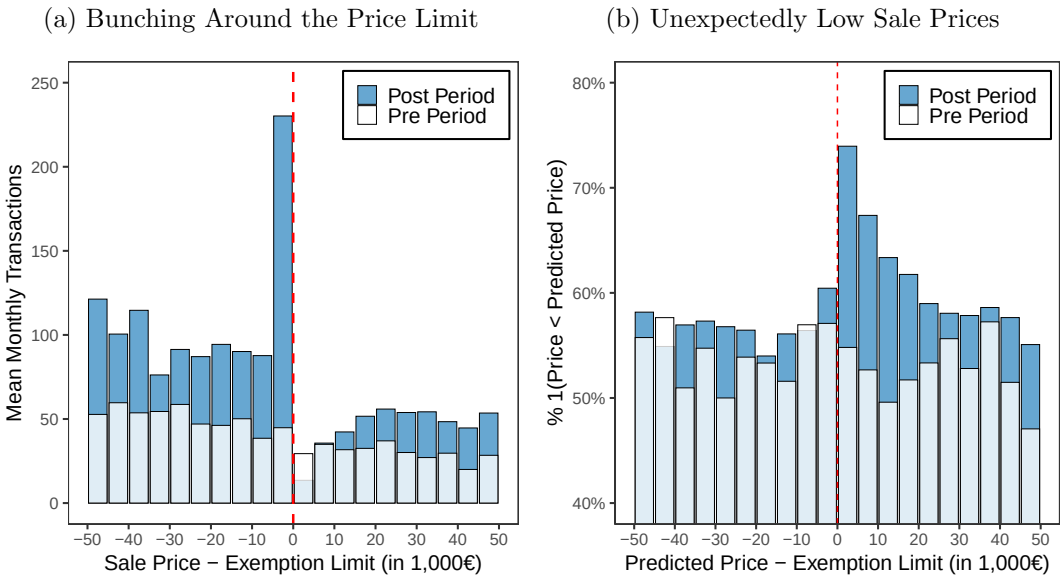


Figure 6: Young Buyers' Behavioral Responses to the Transfer Tax Notch

Notes: This figure shows the average monthly count of home purchases of buyers below age 35 for the periods 2019Q1–2020Q1 (pre) and 2021Q3–2023Q4 (post). Panel (a) groups transactions into €5,000 bins according to their eventual sale price. Panel (b) groups transactions into €5,000 bins using a predicted price. We impute these price predictions by running the following regression on the transaction dataset: $\ln P_{it} = \beta \cdot \ln WOZ_{it} + \gamma_{c(i),t} + \varepsilon_{it}$, with WOZ denoting the most recent tax value and postcode-by-quarter fixed effects $\gamma_{c(i),t}$. Transactions with realized prices within −€25,000 and €24,999 of the exemption limit are removed from the estimation. We then forecast prices based on the estimates $\widehat{\beta}$ and $\widehat{\gamma}_{c,t}$.

before and after the policy. However, young buyers' purchases with a predicted price between €400,000 and €420,000 are 10–15 pp more likely to trade at a discount after the tax exemption and the price cap were introduced, mirroring the missing mass in the distribution of actual prices.

In Appendix Figures B.13 and B.14, we further document that bunching is absent among older buyers, and that observed discounts do not reflect lower listing prices by sellers of units with a predicted price just above €400,000. Therefore, we conclude that discounts are a direct result of negotiations between tax-exempt buyers and sellers. Anecdotes suggest that a common reason for bunching is purchase price manipulation, implemented by reporting artificially low tax-liable unit prices and exaggerated additional payments for non-taxable items such as furniture, kitchens, or other interior items (see, e.g., the report by the Dutch notary association in Schijf, 2023).[17]

[17] We tested for various additional channels that could explain the bunching response, but found little support for differential bunching. First, we tested for the relative market power of buyers vs. sellers by investigating whether transactions involving cash-constrained sellers exhibit more bunching. In a similar vein, we tested whether time-on-market could explain the heterogeneity. We also studied differences between sellers who bought a house themselves shortly before the transaction. Second, we investigated the role of non-arm's-length transactions by identifying intra-family transactions and buyers and sellers from the same neighborhood. Third, we explored regional market differences using the average overbidding rate and time-on-market at the postcode level, as well as a distinction between metropolitan and rural areas, as proxies for housing market tightness. We found bunching in all these subsamples; none of the sample splits can thus explain young buyers' bunching response.

3.5. Aggregate Price Responses

Finally, we investigate to what extent the tax exemption for young homebuyers has been capitalized into property prices. Theory suggests that stimulating housing demand raises equilibrium prices unless housing supply is perfectly elastic. Part of the economic incidence of the tax exemption may thus fall on sellers rather than buyers.

Price effects cannot be identified with the preceding research designs, given the policy's intensive-margin effects, infrequent sales of a given home, and secular market trends. To address these challenges and estimate the impact of the transfer tax exemption on prices, we propose an auxiliary difference-in-differences analysis exploiting variation in treatment exposure at the dwelling level. More specifically, we group units below and above the policy's price limit as treatment and control units, respectively. As in Section 3.1, we use the units' tax-assessed values from January 2019 for this classification, coding all units with tax assessments below €250,000 as treated. Working with ex ante values rather than realized prices avoids conditioning on an outcome variable, which is particularly problematic in the presence of bargaining around the cutoff (Slemrod, Weber, and Shan, 2017). This strategy, however, comes at the cost of misclassification: some units with lower tax values will be classified as treated but may ultimately transact above the price limit, and conversely. The estimates thus have to be interpreted as intention-to-treat (ITT) effects.

We estimate the following difference-in-differences model on the transaction dataset:

$$\ln P_{it} = \sum_{k \neq 20Q2} \beta_k \cdot \mathbb{1}_{(TaxValue<250k)} \cdot \mathbb{1}_{(t=k)} + \gamma_{c(i),t} + \delta \cdot \mathbb{1}_{(TaxValue<250k)} + \varepsilon_{it}, \quad (4)$$

where $\ln P_{it}$ is the natural logarithm of property i's transaction price at time t. We include flexible postcode-by-period dummies, $\gamma_{c(i),t}$, to compare only units from the same local market c. The treatment indicator equals one for units with low tax-assessed values and zero for those with higher ones. In contrast to preceding transaction-level analyses, we now include transactions irrespective of the buyer's age, but restrict the sample to units with tax assessments of €200,000–€230,000 (treatment) and €270,000–€300,000 (control group) in the baseline specification. Selecting the range for treated and control units involves a trade-off between the comparability of units on the one hand and differences in exposure on the other. We return to this issue below and present results for various alternative ranges. Standard errors are clustered at the postcode level.

Estimation results are reported in Table 5. From column (1) to (4), we tighten the comparisons by including more fine-grained regional time trends. Starting with uniform national trends, we then control for MSA-by-quarter and city-by-quarter dummies, respectively, and add postcode-by-quarter fixed effects in the baseline specification presented in column (4). In all specifications, pre-trends are small and statistically insignificant. After the price limit was introduced in 2021Q2, the price of lower-valued units increased sharply, suggesting that the subsidy is immediately capitalized into prices. Ef-

Table 5: The Effect of the Transfer Tax Exemption on House Prices

	Different Time Trend Specifications				Varying Bandwidth	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated ×						
Pre-Policy (19Q1–20Q1)	−0.003	−0.004	−0.003	−0.005	−0.006*	−0.013***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Anticipation (20Q3–20Q4)	0.006*	0.005	0.004	0.003	0.001	−0.002
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
No Price Limit (21Q1)	−0.007*	−0.006	−0.006	−0.002	−0.003	−0.011**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
Cooldown (21Q2)	0.020***	0.019***	0.019***	0.020***	0.008	0.020***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Medium Run (21Q3–23Q4)	0.019***	0.015***	0.015***	0.013***	0.009***	0.012***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Adjusted-R^2	0.580	0.598	0.621	0.728	0.687	0.761
Number of Transactions	214,652	213,102	214,652	214,652	217,587	211,016
Region × Quarter Fixed Effects	National	MSA	City	Postcode	Postcode	Postcode
Treated Price Range	200–230k				210–240k	190–220k
Control Price Range	270–300k				260–290k	280–310k

Notes: This table summarizes the estimated effect of the transfer tax exemption on aggregate house prices. Coefficients are to be interpreted as price growth relative to the omitted quarter 2020Q2. Appendix Figure B.15 visualizes the corresponding quarterly estimates. Estimated treatment effects $\widehat{\beta}_k$ are based on the difference-in-differences model specified in Equation (4), which compares lower-value units (which likely qualify for the exemption) with higher-value units (which likely do not qualify). The estimation is based on the repeated cross-section of all Dutch property transactions from 2019Q1 until 2023Q4 in the respective price ranges. Price ranges refer to tax-assessed values as of January 2019. Column notes indicate the spatial level of the included time fixed effects. MSAs refer to the 40 Dutch metropolitan statistical areas (known as COROP or NUTS 3 regions), cities to the 360 municipalities (*gemeenten*), and postcodes to the 3,788 four-digit postcodes. Standard errors, clustered at the postcode level, are reported in parentheses (significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

fect sizes decrease in the medium run, but prices in the treated group remain significantly above their counterfactual trend. In the first specification, including only quarterly fixed effects, we estimate a 1.9% price increase in the medium run. However, narrower local time trends lower the effect size. In our preferred specification with postcode-by-quarter fixed effects, we find a medium-run price increase of 1.3% relative to 2020Q2.

Given the ITT nature of this analysis, we relate the estimate to the difference in policy exposure. 95% of the units in the lower price range eventually transact below the price limit and qualify for the tax exemption. In the control group, 23% of units with relatively high tax assessments remain below the price limit. Extrapolating the baseline estimate from column (4), we obtain a price response of $0.013/(0.95 - 0.23) \approx 1.8\%$.

Assigning treatment based on ex ante tax-assessed values involves a critical trade-off. On the one hand, choosing treatment and control units from a narrow band just around a cutoff value ensures high comparability between both groups. On the other hand, a narrow band around a cutoff also increases the risk of misclassification, as it raises the likelihood that treated (control) units will eventually transact above (below) the

price limit. In columns (5) and (6) of Table 5, we demonstrate that the medium-run effects are largely robust to using alternative price ranges for assigning treatment and control groups. However, treatment effects and, in particular, pre-trends become noisier, especially when increasing the distance between treatment and control groups.

The price analyses have to be interpreted with caution, as the medium-run policy period includes a major shock to the macroeconomic environment. Central bank rates rose strongly, and mortgage interest rates tripled between early 2022 and early 2023, as the European Central Bank responded to inflationary pressure related to the outbreak of the war in Ukraine. Since interest rate shocks may exert heterogeneous effects across housing market segments (Hacamo, 2024), we cannot rule out that the medium-run estimate is partly confounded by these shocks after 2022Q1, when the interest rate hike cycle began. We address such concerns by calculating a lower bound for the medium-run price response that is identified only by the nine-month period from 2021Q3 until 2022Q1. Based on this subperiod, we estimate an ITT price increase of around 0.9%, which translates into a 1.2% effect when rescaled by the difference in the realized share of tax-exempt units in both groups. Overall, we conclude that the prices of qualifying units rose by 1.2%–1.8% because young buyers were exempt from transfer taxes.

Considering that only a subset of all buyers were eligible for the 2% tax exemption, this is a sizable increase. Specifically, buyers under 35 accounted for only 48.2% of buyers among treated units in the pre-period. Even if supply is perfectly inelastic in the short run, the estimated price effect can be reconciled with theory only if the demand of young buyers is more elastic than that of older ones. This is plausible in our setting: Ortalo-Magné and Rady (2006) show in a life-cycle framework featuring a property ladder that young, borrowing-constrained households are the marginal buyers. They argue therefore that the ability of young households to afford the down payment on their first home is a key driver of aggregate house prices. Taken together, our findings show that targeted financial assistance for young buyers has divergent consequences for different groups of market participants. Sellers of qualifying units are the main beneficiaries, even though they are not directly targeted. Eligible buyers maintain a small net benefit due to the tax exemption. In contrast, ineligible buyers lose out, as they bear the full cost of rising prices without any compensating tax relief.

4. Welfare Implications

The empirical analysis revealed that additional purchases caused by the transfer tax cut mainly reflect short-lived retiming rather than longer-term reoptimization or ownership responses. This motivates a simple static theoretical framework to learn about efficiency and welfare, which we present in Section 4.1. Section 4.2 derives the utility consequences of the transfer tax cut. Section 4.3 introduces efficiency and distributional aspects. Section 4.4 explains the implementation, and Section 4.5 presents simulation results.

4.1. Theoretical Setup

Consider an economy with a large set of individuals indexed by $i \in \mathcal{I}$ at a given point in time. Individuals differ in preferences, characteristics, incomes, and endowments (including real estate). They receive utility from living in a housing unit, $h \in \mathcal{H}$, and consuming amounts c of a non-housing composite good. Housing units are indivisible and differ in dimensions like size, location, quality, and housing tenure, i.e., whether they are owner-occupied or offered for rent. We denote the annual cost of renting or owning a unit, such as rent or maintenance payments, by $r(h)$. We assume disposable incomes consisting of exogenous earnings y_i and a uniform lump-sum tax rebate R.

Utility is summarized by the function $u_i : \mathbb{R}_+ \times \mathcal{H} \to \mathbb{R}$. We allow $u_i(c, h)$ to be heterogeneous across individuals, and assume it is concave and differentiable in c. As is standard in assignment models, we do not impose differentiability with respect to housing since we treat units as indivisible (see, e.g., Määttänen and Terviö, 2014, and Landvoigt, Piazzesi, and Schneider, 2015).

Household Problem. Individual i maximizes utility subject to the budget constraint:

$$y_i + R \geq c_i + r(h_i), \tag{5a}$$

which applies to both renters and homeowners, provided they don't buy or sell a unit. Buyers have to finance the price of their dwelling, $p(h)$, and real-estate transaction taxes with tax rate $\tau > 0$, net of potential subsidies with rate $s \geq 0$:

$$y_i + R \geq c_i + r(h_i) + (1 + \tau - s \cdot \mathbb{1}_{i \in \mathcal{J}})p(h). \tag{5b}$$

We model the tax cut as an offsetting subsidy rather than an outright reduction in the transfer tax (which would be mathematically equivalent), since the transfer tax exemption only applied to a subset of individuals $\mathcal{J} \subseteq \mathcal{I}$. It also keeps the model more generic and applicable to other forms of financial assistance for homebuyers.

Sellers' budget constraint entails additional income and is given by:

$$y_i + R + p(k) \geq c_i + r(h_i), \tag{5c}$$

where $k \in \mathcal{H}$ denotes the unit individual i sells out of her endowment. Tax revenues are redistributed lump-sum such that: $\sum_i R = \sum_{k \in \mathcal{S}} (\tau - s \cdot \mathbb{1}_{i(k) \in \mathcal{J}})p(k)$ with $\mathcal{S} \subseteq \mathcal{H} \cup \{\varnothing\}$ referring to the set of transacted units and subscript $i(k)$ denoting the buyer of unit k. Individuals take prices $p(\cdot)$ and $r(\cdot)$, income y_i, and transfer R as exogenously given.[18]

[18] Lump-sum redistribution of tax revenues is a standard assumption in public finance, but could easily be replaced by any other form of balancing the budget.

Equilibrium. Given the little structure we imposed on the economy, we cannot solve for the equilibrium. Instead, we follow the sufficient-statistics literature and assume that individuals have optimized utility (given budget constraints), and that the economy is in equilibrium (see, e.g., Chetty, 2009, and Kleven, 2021). Observed choices thus reflect individually optimal bundles $\{(c_i^*, h_i^*)\}_{i\in\mathcal{I}}$. We then focus our analysis on small perturbations in the tax and subsidy code (τ, s). An alternative approach in assignment models of the housing market is to impose more structure on the problem in order to characterize the equilibrium analytically (see, e.g., Määttänen and Terviö, 2022).

4.2. Financial Assistance for (Young) Homebuyers: Transfer Tax Cuts

Against the backdrop of the institutional setting and the empirical results from Section 3, we are interested in the utility consequences of a small increase in the purchase subsidy, $\mathrm{d}s > 0$ (equivalent to a small cut in the transfer tax τ). By the envelope theorem, utility changes $\mathrm{d}u_i/\mathrm{d}s$ are governed solely by price changes, whereas behavioral responses have no first-order utility consequences as individuals are already at their optimum. We argue this is a plausible approximation in the context of the Dutch starter tax exemption, since (i) timing responses operated within a very small period and (ii) we did not find evidence for lasting extensive-margin effects on homeownership.

As in Levy (2024), it is useful to distinguish between different subpopulations:[19]

1. Never takers are individuals who do not buy or sell units, neither with nor without the subsidy. They do not benefit from the subsidy but must finance its costs—the reduction in tax revenues—through changes to the lump-sum rebate. Their money-metric utility change can thus be expressed as:

$$\Delta c^{\mathrm{NT}} := \frac{\mathrm{d}u_i/\mathrm{d}s}{\partial u_i/\partial c} = \frac{\mathrm{d}R}{\mathrm{d}s} \qquad \forall i \in \mathcal{I}^{\mathrm{NT}}, \tag{6a}$$

   which is negative as long as the economy is on the bright side of the Laffer curve.

2. We next consider always buyers, i.e., individuals who would have bought and switched home to the same unit also absent a reform. A small increase in the subsidy s affects always buyers' money-metric utility by:

$$\Delta c^{\mathrm{AB}} := \frac{\mathrm{d}u_i/\mathrm{d}s}{\partial u_i/\partial c} = p \cdot \mathbb{1}_{i\in\mathcal{J}} - (1+\tau-s)\frac{\mathrm{d}p}{\mathrm{d}s} + \frac{\mathrm{d}R}{\mathrm{d}s} \qquad \forall i \in \mathcal{I}^{\mathrm{AB}}, \tag{6b}$$

   where the first term on the right-hand side of the equation represents the direct gain from the subsidy and only accrues to eligible buyers. The second term represents price effects that may partially offset the direct effect. The third term gives the change in the lump-sum transfer, which is, again, likely negative.

[19] We deliberately focus on mutually exclusive groups to simplify the exposition. In reality, some people buy and sell units around the same time. We take this into account when quantifying welfare effects.

The intuition is that always buyers would have switched to bundle (c', h') even absent a reform and can now afford $(c' + \Delta c^{\text{AB}}, h')$, which still yields the utility from living in unit h' but gives higher composite good consumption. This difference in consumption, Δc^{AB}, measures their money-metric utility gain.

3. For always sellers, money-metric utility effects depend only on the change in transaction prices and the effect on the lump-sum rebate:

$$\Delta c^{\text{AS}} := \frac{\mathrm{d}u_i/\mathrm{d}s}{\partial u_i/\partial c} = \frac{\mathrm{d}p(k)}{\mathrm{d}s} + \frac{\mathrm{d}R}{\mathrm{d}s} \qquad \forall i \in \mathcal{I}^{\text{AS}}. \tag{6c}$$

   The sign of this expression is theoretically undetermined as the former term is likely positive and the latter likely negative. The larger the price effect, i.e., the capitalization of the subsidy in sale prices, the bigger the gain for always sellers.

4. New buyers wouldn't trade under the old regime but reconsider with the (increased) subsidy. Their change in money-metric utility is bounded by the polar cases of always buyers and never takers, and therefore in between Δc^{NT} and Δc^{AB} (Milgrom and Segal, 2002). Always buyers prefer purchasing even in the absence of the policy, which implies $u(c', h') > u(c, h)$. In contrast, never takers prefer their initial bundle (c, h) over any (c', h') even at the new, lower tax-inclusive prices. Both groups are inframarginal with and without the subsidy.

   New buyers are in between: the marginal person would already switch to h' for a tiny compensating payment ϵ, such that $u(c', h') < u(c, h)$ but $u(c' + \epsilon, h') > u(c, h)$ absent the subsidy. Her corresponding money-metric utility gain from the tax cut is $\Delta c^{\text{AB}} - \epsilon$, and thus almost as much as for always buyers. The last switcher considers buying only for a large compensation of $\Delta c^{\text{AB}} - \epsilon$, thereby gaining marginally higher money-metric utility than never takers, i.e., $\Delta c^{\text{NT}} + \epsilon$. She was inframarginal before and becomes the marginal person with the subsidy. The subsidy largely goes into making her consider moving house from h to h'.

5. Finally, new sellers are bounded by never takers and always sellers $(\Delta c^{\text{NT}}, \Delta c^{\text{AS}})$.

These utility consequences are governed by a few empirical objects ("sufficient statistics"): (i) the pass-through of the subsidy to prices, $\mathrm{d}p/\mathrm{d}s$, (ii) its impact on transactions and the distribution of switchers, and (iii) its impact on tax revenues, $\mathrm{d}R/\mathrm{d}s$. Importantly, behavioral responses enter only via their impact on tax revenues, but have no first-order effect on individual utility because of the envelope theorem.

Timing Responses. We deliberately simplify the setup by sticking to a static model and ignoring timing responses. However, such responses are quantitatively important, as shown in Sections 3.1 and 3.2. The simulated money-metric utility changes Δc_i for new buyers (and sellers) are thus somewhat underestimated, since they ignore the

additional utility gain from switching home from h to h' already today rather than in the future.[20] We argue that the static model yields a reasonable approximation given that reoptimizations happen within a short period of time. In fact, our estimates suggest that the supposedly additional purchases would have occurred within one or two years also absent the transfer tax cut, since we can reject even small extensive-margin increases in homeownership a few years after the reform. A fully dynamic model would allow us to quantify these differences more precisely at the cost of higher complexity and additional assumptions (e.g., on discount rates). We thus keep a static perspective.

4.3. Efficiency and Distribution

We assume that policymakers would want to support homebuyers via higher subsidies whenever such an increase $\mathrm{d}s > 0$ raises social welfare:

$$\mathrm{d}W/\mathrm{d}s := \sum_i g_i \frac{\mathrm{d}u_i/\mathrm{d}s}{\partial u_i/\partial c} = \sum_i g_i \Delta c_i > 0, \tag{7}$$

with exogenously given, fixed social marginal welfare weights denoted by g_i (Saez and Stantcheva, 2016). Equation (7) captures both efficiency and equity concerns.

Efficiency. Efficiency or aggregate welfare effects are given by the sum $\sum_i \Delta c_i$ over all individual money-metric utility changes defined above (as in Määttänen and Terviö, 2022). This measure corresponds directly to the deadweight loss logic of the Harberger triangle. Raising the subsidy s allows more buyers and sellers to realize mutually beneficial transactions, thereby reducing the distortion caused by the real-estate transfer tax τ in the first place. The larger the groups of new buyers and new sellers, and the larger the additional transaction volume, the greater the aggregate welfare gain.

A standard measure for this efficiency gain is the tax base elasticity, which can be calculated using the reduced-form effect on purchase volumes as estimated in Section 3.4 (Table 4, column (1)). However, treating $\sum_i \Delta c_i$ as a measure of social welfare gains (rather than efficiency) would require the restrictive assumption $g_i = 1$.

Distribution. In the absence of behavioral responses, the above formulas (6a)–(6c) reduce to pure resource redistribution between individuals. Such redistributive effects are governed by price changes, $\mathrm{d}p/\mathrm{d}s$, and thus tax incidence. Although tax incidence would not affect the overall surplus in society, it may still affect social welfare when individuals differ in their welfare weights g_i. Jacobs, Jongen, and Zoutman (2017) provide corresponding evidence that social preferences of Dutch political parties differ significantly across household types and throughout the income distribution.

[20] Conversely, we overestimate utility changes for buyers delaying their purchase from late 2020 to early 2021 in order to become eligible for the announced tax cut. These buyers benefit from higher lifetime composite good consumption, but only move to their new residence a few weeks or months later than planned. The former, positive effect is captured in Δc_i, the latter, negative effect is not.

To examine the distributional aspect, we simulate the utility consequences of the starter tax exemption for each household in the Netherlands and calculate average money-metric utility changes for various quantiles of the income distribution. This allows us to shed light on the second dimension of social welfare, namely, distributional effects among buyers, sellers, and never takers across the income distribution. Household-level utility effects could then further be aggregated to social welfare changes $\mathrm{d}W/\mathrm{d}s$ using any arbitrary set of desired weights g_i.

4.4. Empirical Implementation

We base the simulation on the 2022 snapshot of the administrative data, including all Dutch-born individuals aged 18 or older. This represents the status quo after the introduction of the starter tax exemption. Therefore, utility changes due to the policy are expressed as money-metric compensating variation. For each individual in the sample, we identify household-level disposable income, which we normalize to account for household size based on the OECD equivalence scale.

First, individuals are classified into the five groups introduced in Section 4.2. Based on their observed housing market activity during the one-year period from 2021Q3 until 2022Q2, we label them as buyers, sellers (or both) or never takers.[21] Next, we introduce two additional distinctions. First, we distinguish buyers above and below the age of 35, as only the latter benefit from the tax exemption. Second, we identify all transactions—both purchases and sales—below €400,000, the policy's price limit. To simulate new transactions generated by the policy, we first split the sample of all young buyers into quartiles, based on their normalized household income. Then, we randomly select a subset of young buyers *within* each quartile as new (i.e., policy-induced) buyers based on the estimated treatment effects by income quartile (using the estimates from Section 3.1). For instance, in the first quartile, the policy is estimated to increase purchase probabilities by 32.5%.[22] Therefore, we label 75.5% ($= 1/(1 + 0.325)$) of all observed young buyers in this quartile as always buyers, and the remaining 24.5% as new buyers. Conversely, we label as new sellers those individuals who sell to a new buyer, whereas all other sellers are labeled as always sellers.

We then calculate individual utility changes based on Equations (6a)–(6c), using the reduced-form estimates from Section 3. In the baseline scenario, we set $\mathrm{d}p/\mathrm{d}s = 0.012p$, which equals the estimated policy's medium-run price effect before the 2022 interest rate shock. To obtain $\mathrm{d}R/\mathrm{d}s$, we calculate the total forgone transfer tax revenue due to the exemption of (always) buyers, and divide it by the size of the adult population.

[21] We select transactions during this window, as they are most representative of the medium-run effect of the policy, i.e., least affected by timing responses or confounding factors due to interest rate changes.

[22] As shown in column (4) of Table 1, the estimated monthly effect in the medium run in the first quartile equals 0.064 pp. This corresponds to a 32.5% increase relative to the average monthly purchase probability in 2020Q2 among individuals in the first income quartile, which was equal to 0.197%.

This corresponds to a per-person tax rebate change of −€20.36.[23] For new buyers and sellers, we draw utility changes from a uniform distribution, bounded between the utility changes of never takers and those of always buyers or sellers, respectively. This follows the intuition, outlined above, that individuals who change their behavior due to the subsidy must have a lower utility gain than those for whom the subsidy is a pure windfall.[24] For individuals who are both buyers and sellers, we sum the respective utility changes from both activities. After calculating utility changes for each individual in the sample, we sum over all (adult) members of a household and depict all results at the household level. Then, we aggregate households to different quantiles of the standardized household income distribution.

4.5. Simulation Results

We find that the transfer tax cut for young homebuyers induced efficiency gains of $\sum_i \Delta c_i =$ €11.4 mn in the aggregate, or on average €0.83 per Dutch adult and €1.45 per household, respectively. This improvement in economic surplus is to be expected, given that the removal of distortionary transfer taxes allows a number of welfare-enhancing transactions that would not have occurred otherwise.

The detailed administrative microdata allow us to go beyond these aggregate numbers and also identify households winning and losing from the transfer tax cut. Figure 7 visualizes the results of our simulation across the eight million households. In all four panels, households are ordered along the horizontal axis based on their standardized household income, which we aggregate into ventiles (i.e., five-percentile groups). Panel (a) shows the baseline results, revealing a hump-shaped pattern. Low-income households lose utility due to the tax exemption for young homebuyers.[25] With rising income, the policy becomes increasingly beneficial, and its positive impact peaks around the 70th percentile. Utility gains decline again for the top quartile, and for households at the very top of the distribution, average utility changes turn even negative. The negative impact of the tax cut for many households across the distribution reflects the lump-sum redistribution of the loss in tax revenues. An alternative reading of the simulation results is therefore that the transfer tax cut causes a budget deficit of €281 mn and benefits mainly upper-middle-income households.[26]

[23] New buyers are not considered in the calculation of forgone tax revenue, as they would not have made a purchase absent the subsidy.

[24] We resort to a uniform distribution since the data do not allow us to distinguish between marginal and almost inframarginal new buyers and sellers. We assume positive assortative matching (Chade, Eeckhout, and Smith, 2017): for the new buyer with the smallest utility gain, the corresponding seller is also assigned the smallest utility gain, etc.

[25] The lowest percentiles appear to deviate from the overall trend. This is likely due to mismeasured incomes, or driven by low-income, high-wealth households (e.g., wealthy pensioners). Evidence for this is seen in panel (c): at the bottom of the distribution, the share of observed home sellers increases.

[26] Many alternative refinancing mechanisms other than lump-sum redistribution could be thought of. We abstain from simulating alternative proposals, since the arbitrary choice among these would drive the welfare effects along the income distribution and conceal the impact of the transfer tax cut.

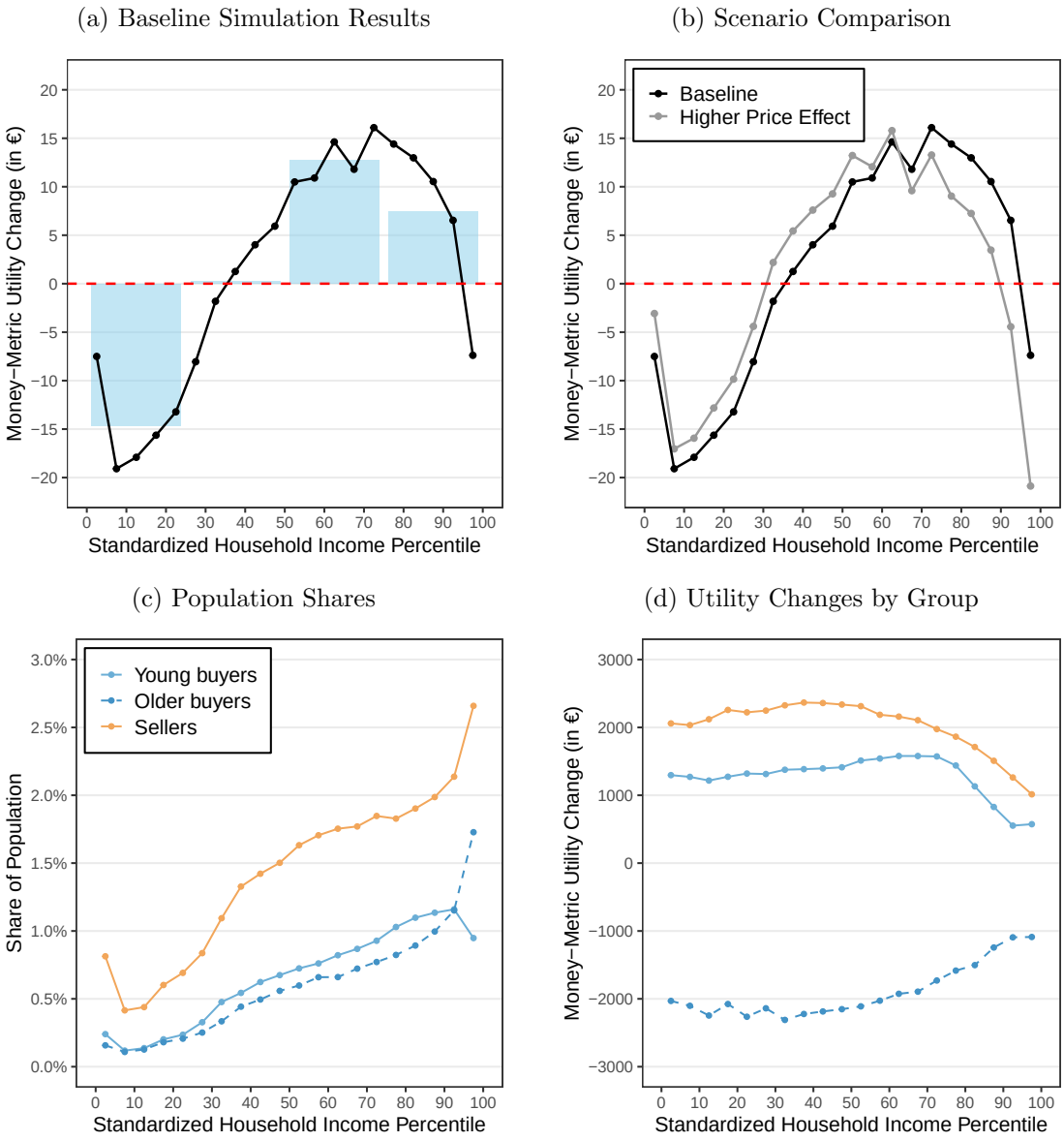


Figure 7: Welfare Simulation of the Transfer Tax Exemption

Notes: This figure shows the result of simulating the welfare effects of the starter tax exemption for the Dutch adult population, along the household income distribution. Panel (a) visualizes the baseline simulation result. Average utility changes due to the policy (in €) per ventile are shown in black, and averages per quartile are represented by blue bars. In panel (b), the baseline simulation is contrasted with a higher price effect (1.8% increase) scenario in grey. Panel (c) shows the shares of sellers, young buyers, and older buyers along the income distribution. Panel (d) shows average utility changes for the same groups. For never takers, who are omitted from the figure, utility changes are always equal to $\mathrm{d}R/\mathrm{d}s = -€20.36$ in the baseline scenario.

An even stronger increase in prices due to the subsidy would not change this pattern qualitatively. This is shown in panel (b), where we contrast the baseline with a second scenario that is based on a 1.8% price increase (in grey). Compared to the baseline, a higher price effect would—perhaps unintuitively—make the tax exemption slightly more desirable for lower-income households, at the expense of those with very high incomes. This is due to the fact that with a higher price effect, some of the policy's negative effect

on the tax rebate is recaptured via higher tax receipts from ineligible homebuyers. As a consequence, never takers—i.e., those who neither buy nor sell—would have a smaller utility loss in this scenario.

To better understand what drives the hump-shaped effect of the policy on money-metric utility changes, we separately examine the different societal groups. Intuitively, the average utility change per income percentile depends only on two factors: the share of (in-)eligible buyers, sellers, and never takers in this percentile, and their respective average utility change. We disentangle these two components in panels (c) and (d) of Figure 7. Panel (c) reveals that the share of individuals who participate in the housing market is steadily increasing with income. Conversely, the bottom of the income distribution comprises almost exclusively never takers. These are not directly affected by the transfer tax cut, but nonetheless have to finance it via the tax rebate, as captured by Equation (6a). With rising incomes, the share of buyers and sellers increases. In addition, the distinction between younger and older buyers is noteworthy, as only the former group benefits from the subsidy. Across the distribution, there are somewhat more younger buyers than older ones. Only among those with very high incomes does this pattern reverse, as the share of older buyers increases sharply, whereas the share of younger buyers decreases. These trends are relevant since—as shown in panel (d)—the policy's utility consequence varies drastically by group. Whereas young homebuyers benefit from the tax cut and sellers benefit from higher prices, older buyers suffer from price increases. This is most pronounced among older buyers with lower incomes, who are most likely to buy lower-value units, which are subject to the price increase. In contrast, buyers and sellers with higher incomes tend to be less affected, as their activity is focused on higher-value units, which are unaffected by the policy.

In sum, our welfare simulation suggests that, while the transfer tax exemption did increase the overall economic surplus, it mainly benefitted the upper-middle part of the income distribution. We conclude that—under any common set of welfare weights (see, e.g., Jacobs, Jongen, and Zoutman, 2017)—tax cuts for young homebuyers in the Netherlands are unlikely to promote social welfare.

5. Conclusion

Financial assistance programs for homebuyers are common in many developed economies, but their impact on homeownership, efficiency, and welfare remains unclear. In this study, we analyze the effects of a transfer tax cut for young homebuyers aimed at reducing the upfront purchase costs and thus easing their access to homeownership.

Using various complementary identification strategies, we show that the transfer tax exemption raised purchase activity among treated individuals. We also document small short-run increases in homeownership. These effects are short-lived, however, and we can exclude even modest increases in the homeownership rates of eligible individuals a

few years after policy introduction. The tax cut introduced new distortions in when and what type of unit eligible individuals buy. It also led to significant increases in house prices, such that the majority of the benefit accrued to sellers. Finally, we illustrate that the policy increased efficiency, but benefitted mainly upper-middle-income households. Overall, our results suggest that upfront financial assistance is unlikely to meaningfully promote homeownership among young households, particularly in markets where housing supply is inelastic.

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A. Overview of Homebuyer Assistance Policies

Table A.1: Homeownership Assistance Policies for Young and First-Time Buyers

Country	Policy	Eligibility	Details
Panel A – Direct subsidies			
Costa Rica	Family Housing Grant for Young People	Young (under 36) buyers; income limit; no other property owned; property requirements; other criteria.	Grant for purchase, construction or improvement, paid to the seller or builder. Amount based on individual needs assessment; no fixed maximum.
Lithuania	Financial Incentive for Young Families Acquiring a First Home	Young (under 36) first-time buyers; regional location restriction; mortgage required; other criteria.	Subsidy on the housing loan or downpayment, restricted to regional municipalities. Increasing with the number of children; up to €13,050.
	State Partially Compensated Housing Credit	Young (under 36) or first-time buyers; income limit; asset limit; property requirements; mortgage required.	Means-tested subsidy attached to a housing credit, amount differs by household type, up to €26,100.
Malta	First-Time Buyer Scheme	First-time buyers; price cap; bank home loan required; owner-occupancy; other criteria.	Post-purchase grant paid over time. €10,000 in total, €1,000 per year over ten years.
	Grant for First-Time Buyers	First-time buyers; residency requirement; property requirements; owner-occupancy; other criteria.	Purchase grant tied to heritage and vacancy policy: eligibility depends on the building, not the buyer. Up to €15,000, or €40,000 in Gozo.
Russia	Young Family Program	Young (under 36) buyers; registered housing need; income requirement; property requirements; other criteria.	Subsidy calculated based on administratively calculated housing cost. 30% of that cost, or 35% with children or as a single parent.
South Africa	First Home Finance	First-time buyers; income limit; citizenship or permanent residence; approved home loan; other criteria.	Purchase subsidy decreasing continuously with income. Up to R169,265.

Continued on next page

Table A.1: Homeownership Assistance Policies for Young and First-Time Buyers (Continued)

Country	Policy	Eligibility	Details
United States	HomeReady Very Low-Income Purchase Credit (Fannie Mae & Freddie Mac)	First-time buyers; income limit; loan-product requirements; property requirements; other criteria.	GSE-based subsidy at closing for borrowers at or below half of area median income. Up to $2,500 toward downpayment or closing costs.
Panel B – Tax relief / tax credit / tax refund			
Bulgaria	Tax relief for young married couples	Young (under 35) buyers; sole family dwelling; property requirements; other criteria.	Deduction of mortgage interest from personal income tax. Applies to interest on the first BGN 100,000 of principal.
Canada	Home Buyers' Amount	First-time buyers; owner-occupancy; property requirements; other criteria.	Non-refundable income-tax credit, identical for all qualifying buyers. Maximum claim equals C$10,000, benefit depends on marginal tax rate.
	First-Time Home Buyers' GST/HST Rebate	First-time buyers; new-build only; price cap; other criteria.	Rebate of federal sales tax on new homes, decreasing with home value. Full rebate up to C$1m, falling to zero at C$1.5m; maximum benefit is C$50,000.
Croatia	Support for the Purchase of Residential Property	Young (under 45) first-time buyers; no other adequate housing; owner-occupancy; other criteria.	Refund of transaction tax covering both existing units and new constructions. Full refund of transfer tax or up to 50% of VAT.
Hungary	Duty allowance on the acquisition of a first residential property	Young (under 35) first-time buyers; price cap; property requirements.	Partial exemption from transfer tax, applied to a capped value. 50% reduction of the 4% duty; up to HUF 300,000.
Iceland	Half stamp duty on a first purchase	First-time buyers; no further requirements.	Halving of the transfer tax from 0.8% to 0.4%.
Ireland	Help to Buy	First-time buyers; new-build or self-build only; price cap; minimum mortgage share; other criteria.	Refund of the buyer's own past income tax payments. Lowest of €30,000, 10% of the purchase price, or four years' tax payments.
Japan	Housing Loan Tax Reduction	Young (under 40) buyers; income limit; property requirements; other criteria.	Raised limit on qualifying loan balance for mortgage interest deduction. 0.7% of the year-end balance for up to 13 years.

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Table A.1: Homeownership Assistance Policies for Young and First-Time Buyers (Continued)

Country	Policy	Eligibility	Details
Malta	First-Time Buyer Stamp Duty Exemption	First-time buyers; owner-occupancy.	Transfer tax exemption. 5% duty waived on the first €200,000; up to €10,000.
Portugal	IMT Jovem	Young (under 36) first-time buyers; price cap; owner-occupancy; other criteria.	Combined exemption from two separate transaction taxes. Avoids transfer tax plus 0.8% stamp duty, maximum subsidy around €2,500 at the price limit.
Saudi Arabia	State bearing of the real-estate transaction tax	First-time buyers; citizenship requirement; price cap; other criteria.	State payment of the transaction tax to the authority. 5% tax covered on the first SAR 1,000,000; up to SAR 50,000.
Slovakia	Tax Bonus on Paid Interest	Young (under 36) buyers; income limit; one qualifying loan; property requirements; other criteria.	Refundable tax bonus for households with low incomes. 50% of interest paid, up to €1,200 per year.
South Korea	Acquisition-tax reduction for a first-time home purchase	First-time buyers; price cap; household non-ownership; other criteria.	Acquisition-tax relief capped in absolute terms. Up to KRW 2m.
United Kingdom	Stamp Duty Land Tax: First-Time Buyers' Relief	First-time buyers; price cap; all purchasers must qualify.	Raised tax-exemption threshold, 0% on the first £300,000; up to £5,000 saved.
Panel C – Mortgage access, guarantees and LTV support			
Australia	Australian Government 5% Deposit Scheme	First-time buyers; regional price caps; owner-occupancy; minimum deposit; other criteria.	Public guarantee replacing lenders' mortgage insurance. Guarantee of up to 15% of value, enabling 95% LTV ratio.
Canada	Preferential mortgage amortization term for first-time home buyers	First-time buyers; insured mortgage required; price cap; other criteria.	Relaxation of a regulatory term limit, reducing installments but raising total interest. Extension from 25-year to 30-year amortization.
Czechia	Czech National Bank LTV limit for applicants under 36	Young (under 36) buyers; owner-occupancy; regular DSTI and DTI limits apply.	Macroprudential exemption applied by the central bank. Increase of maximum LTV ratio from 80% to 90%.

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Table A.1: Homeownership Assistance Policies for Young and First-Time Buyers (Continued)

Country	**Policy**	**Eligibility**	**Details**
Estonia	Housing loan guarantee	Young (under 36) first-time buyers; education and employment requirement; other criteria.	Loan guarantee tied to occupational status. Up to 24% of property value, capped at €20,000.
Italy	Fondo Prima Casa (CONSAP)	Young (under 36) buyers; income and asset test; mortgage cap; property requirements; other criteria.	Two-tier mortgage guarantee, higher tier reserved for priority groups. Up to €125,000 (50% of the cap) or €200,000 (80%).
Latvia	ALTUM housing guarantee for young specialists	Young (under 36) buyers; education requirement; other criteria.	Public loan guarantee, increasing with loan size up to the cap. 20% of principal, capped at €50,000.
Portugal	Public guarantee for housing credit for young people	Young (under 36) first-time buyers; income limit; price cap; owner-occupancy; other criteria.	Public loan guarantee. Up to €67,500 (15% of the transaction cap), financing up to 100% of the price.
Saudi Arabia	Facilitated Mortgage Program	First-time buyers; citizenship requirement; eligibility assessment; other criteria.	Mandated lending terms for eligible borrowers, reduction of the downpayment requirement to 5%.
Spain	ICO guarantee line for first-home purchase	Young (under 36) first-time buyers; income limit; wealth limit; regional price caps; residency requirement; other criteria.	Public loan guarantee. Covering 20% of the mortgage, or 25% with an energy rating of D or better.
United States	FHFA enterprise first-time homebuyer upfront fee relief	First-time buyers; income limit; loan-product requirements; other criteria.	Waiver of upfront pricing fees for GSE loans. Maximum benefit depends on loan size and borrower risk.
	Fannie Mae Standard 97% LTV Option and Freddie Mac HomeOne	First-time buyers; owner-occupancy; loan limit; homebuyer education; other criteria.	Conforming mortgage lower downpayment requirement. Raising LTV limits up to 97%.
Panel D – Subsidized mortgages, public loans and preferential credit			
Colombia	Generación FNA	Young (under 29) buyers; fund affiliation required; other criteria.	Interest rate discount on loan from the public savings fund. Reduction of 0.5 pp, financing up to 90% of property value.

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Table A.1: Homeownership Assistance Policies for Young and First-Time Buyers (Continued)

Country	**Policy**	**Eligibility**	**Details**
France	Prêt à taux zéro	First-time buyers; income limit; new-build or major-renovation only; owner-occupancy.	Interest-free second mortgage alongside the bank loan, with a 25-year repayment period. 0% on up to 50% of the price.
Hungary	Otthon Start Program	First-time buyers; minimum contribution history; price caps; price per square meter cap; minimum own funds; other criteria.	Public loan fixed below market rate for the full term, large enough to cover most of a qualifying purchase. Up to HUF 50m at 3% for 25 years.
Indonesia	KPR Tapera	First-time buyers; income limit; scheme membership required; price cap; owner-occupancy.	Subsidized mortgage linked to a contributory savings mandate. Allowing for 100% LTV ratios, fixed at 5% interest rate for up to 30 years.
Ireland	Local Authority Home Loan Scheme	First-time buyers; income limit; price cap; prior refusal by two lenders; owner-occupancy; other criteria.	Public mortgage of last resort, available only after refusal by banks. Up to €373,500 at 90% LTV ratio, fixed at 4.00–4.05%.
Japan	Flat 35 Child-Rearing Plus	Young (under 40) buyers; property requirements; other criteria.	Interest rate reduction deepening with each additional child. Maximum reduction of 1 pp for the first five years.
Malta	10% Deposit Scheme	Young (under 40) first-time buyers; income limit; price cap; bank deposit loan required; other criteria.	State payment of interest on an additional mortgage to cover 10% downpayment requirement.
Mexico	Programa de Vivienda para el Bienestar	First-time buyers; income limit; new-build only; owner-occupancy.	Interest-free public financing of the entire purchase price, restricted to eligible new units.
New Zealand	Kāinga Ora First Home Loan	First-time buyers; income limit; owner-occupancy; minimum deposit; other criteria.	Government-sponsored mortgage with reduced downpayment requirement. Raises maximum LTV ratio from 80% to 95%.
South Korea	First-Time Homebuyer Bogeumjari Loan	First-time buyers; income limit; price cap; household non-ownership; other criteria.	Long-term fixed-rate policy mortgage limiting exposure to rate volatility. Up to KRW 420m at 80% LTV, with fixed interest rates between 4.9% and 5.2%.

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Table A.1: Homeownership Assistance Policies for Young and First-Time Buyers (Continued)

Country	Policy	Eligibility	Details
	Didimdol Home Purchase Loan – first-time-buyer terms	First-time buyers; income limit; asset limit; price cap; household non-ownership.	More means-tested but cheaper alternative to Bogeumjari. Up to KRW 240m at 80% LTV, with fixed interest rates between 2.85% and 4.15%.
Panel E – Shared-equity and shared-risk loans			
Iceland	Equity loans	First-time buyers; income limit; asset limit; new-build only; other criteria.	Public equity stake of up to 25% of the price, or 35% for lower-income buyers; available for up to 25 years.
Ireland	First Home Scheme	First-time buyers; income limit; price cap; new-build or self-build only; minimum mortgage share; owner-occupancy.	Public equity stake of up to 30% of the price, or 20% when combined with Help to Buy.
South Korea	Profit-and-Loss-Sharing Mortgages	First-time buyers; income limit; asset limit; price cap; property requirements; other criteria.	Equity-like public mortgages sharing only price gains, or gains and losses, at resale. 1.8% rate (profit-sharing) or 1.3% rising to 2.3% (profit-and-loss sharing).
Panel F – Savings accounts and mobilization of own savings			
Australia	First Home Super Saver Scheme	First-time buyers; voluntary contributions only; property requirements; other criteria.	Conversion of pension savings account into deposit-saving vehicle. Up to A$50,000 of voluntary contributions plus earnings, A$15,000 per year.
Canada	First Home Savings Account	First-time buyers; age limits; qualifying home required; other criteria.	Deduction or tax-free withdrawal of pension account exclusively for deposit-saving. Up to C$40,000 lifetime, C$8,000 per year.
	Home Buyers' Plan	First-time buyers; repayment obligation; qualifying home required; other criteria.	Loan from one's own pension savings account. Up to C$60,000 tax-free withdrawal, repayable over 15 years.
Finland	ASP system	First-time buyers; age limits; minimum savings requirement; other criteria.	Savings and borrowing support scheme. Bonus deposit interest up to 4%; state pays 70% of loan interest above 3.8% for ten years; guarantee to €60,000.

Continued on next page

Table A.1: Homeownership Assistance Policies for Young and First-Time Buyers (Continued)

Country	Policy	Eligibility	Details
Iceland	Private pension savings for first-time home buyers	First-time buyers; qualifying use of funds; other criteria.	Tax-exempt withdrawal from retirement savings account to finance downpayment or full purchase. Up to ISK 500,000 per year and ISK 5,000,000 in total.
Norway	BSU – young people's housing savings	Young (under 34) buyers; must not own a home; annual and lifetime savings caps; qualifying use of funds.	Annual tax credit to reward saving for downpayment. Up to NOK 2,750 per year on savings up to NOK 27,500; NOK 300,000 lifetime cap.
Poland	Konto Mieszkaniowe	First-time buyers; age limits; minimum saving period; qualifying use of funds; other criteria.	Interest premium for downpayment savings. Premium based on inflation and house price growth. Deposits capped at PLN 2,000 per month, PLN 240,000 over ten years.
Turkey	Konut Hesabı ve Devlet Katkısı	First-time buyers; minimum saving period; sole home requirement; other criteria.	Proportional top-up on accumulated downpayment savings. Up to 20% of savings, capped at TRY 270,690 in 2026.
United Kingdom	Lifetime ISA	Young (under 40) first-time buyers; price cap; mortgage required; other criteria.	Flat-rate top-up on downpayment savings, independent of the saver's tax position. 25% on annual savings up to £4,000; up to £1,000 benefit per year.
United States	Qualified first-time homebuyer distributions (Internal Revenue Code § 72(t))	First-time buyers; lifetime cap; qualifying use of funds; other criteria.	Waiver of the early-withdrawal penalty from IRA account. Up to US$10,000 exempt from the 10% penalty; maximum saving US$1,000.
Panel G – Other ownership-support mechanisms			
Ireland	Starter Home Purchase Scheme	First-time buyers; income limit; price cap; owner-occupancy; other criteria.	Discounted sale by the local authority itself, which retains an equity stake equal to the discount. Discount equals difference between market value and affordable price.

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Table A.1: Homeownership Assistance Policies for Young and First-Time Buyers (Continued)

Country	**Policy**	**Eligibility**	**Details**
Netherlands	Nationaal Fonds Betaalbare Koopwoningen	Young (under 35) first-time buyers; income limit; price cap; new-build only; other criteria.	Revolving fund financing buyer discounts on newly built units; discount and part of the value increase repaid on resale. Support capped at €70,000 per dwelling.
South Korea	First-Time Home Buyer Special Housing Supply (Rules on Housing Supply, Art. 43)	First-time buyers; income limit; asset limit; subscription savings account; household non-ownership.	Allocation of rationed new supply through the housing subscription system. 15% of the relevant public-housing supply volume reserved.
Spain	Plan Estatal de Vivienda 2026–2030, Sec. 5a – rental with an option to purchase protected housing	Young (under 36) buyers; income limit; protected housing only; other criteria.	Conversion of rental payments into equity over the tenancy. Purchase price reduced by rent that is already paid; benefit up to €30,000.
United Kingdom	First Homes scheme (England only)	First-time buyers; income limit; price cap; other criteria.	30–50% discount relative to market value for eligible units. Discount is forwarded to the next buyer upon subsequent sale.
	Rent to Buy (England only)	First-time buyers; employment and affordability checks; scheme-designated homes only; other criteria.	Rental discount of up to 20% to save for downpayment, combined with option to buy the unit after a maximum of five years.
United States	HCV Homeownership Program	First-time buyers; existing voucher holder; minimum employment income; other criteria.	Redirection of an existing rental voucher into monthly ownership assistance. Benefit amount depends on household income and local housing costs.

B. Additional Reduced-Form Analyses

B.1. More Details on Home Purchases

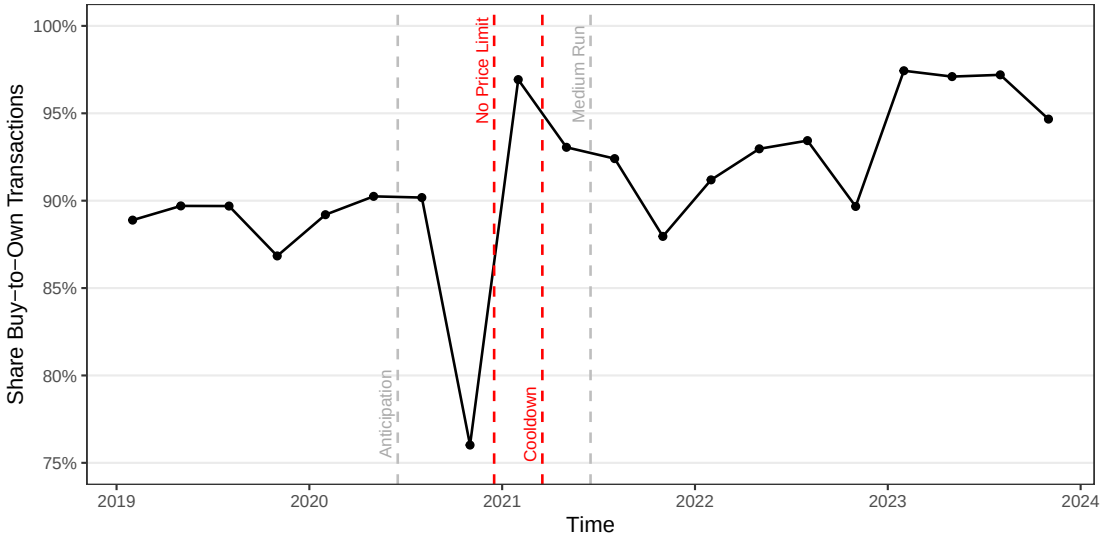


Figure B.1: Share of Buy-to-Own Transactions

Notes: This figure shows the share of buy-to-own transactions as a fraction of all residential transactions in a quarter. A transaction is labeled as buy-to-own when it is bought by a natural person who uses the property as main residence in the year after the transaction.

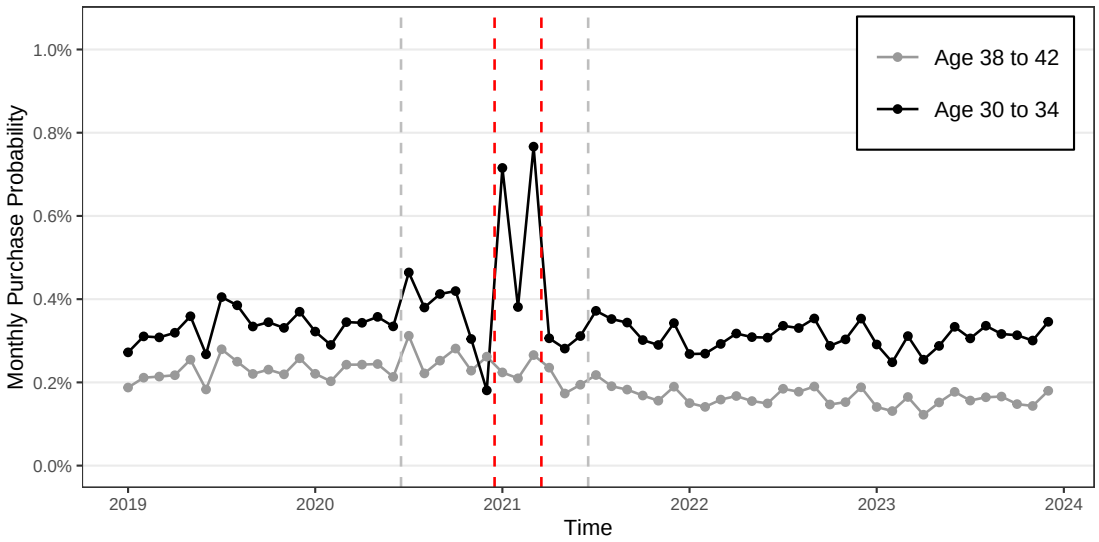


Figure B.2: Unadjusted Home Purchase Rates Among Younger and Older Individuals

Notes: This figure shows the share of individuals aged 30–34 and 38–42 buying a home in a given month between January 2019 and December 2023, including both first-time and repeat purchases. This unadjusted time series serves as the basis for the difference-in-differences model shown in Figure 1.

Table B.1: Monthly Difference-in-Differences Estimates

	Raw Data		Estimated		Raw Data		Estimated
Month	Younger 30–34 (1)	Older 38–42 (2)	Normalized Difference (3)	Month	Younger 30–34 (1)	Older 38–42 (2)	Normalized Difference (3)
January 2019	0.272	0.188	−0.027	July 2021	0.372	0.218	0.042
February 2019	0.311	0.211	−0.012	August 2021	0.352	0.191	0.050
March 2019	0.308	0.214	−0.017	September 2021	0.344	0.183	0.049
April 2019	0.319	0.217	−0.010	October 2021	0.302	0.168	0.022
May 2019	0.359	0.255	−0.007	November 2021	0.290	0.156	0.022
June 2019	0.267	0.183	−0.027	December 2021	0.343	0.190	0.041
July 2019	0.405	0.280	0.014	January 2022	0.268	0.150	0.006
August 2019	0.385	0.250	0.024	February 2022	0.269	0.141	0.016
September 2019	0.334	0.220	0.002	March 2022	0.292	0.159	0.022
October 2019	0.345	0.231	0.002	April 2022	0.317	0.167	0.038
November 2019	0.331	0.219	0.000	May 2022	0.309	0.155	0.042
December 2019	0.370	0.258	0.000	June 2022	0.308	0.149	0.046
January 2020	0.322	0.221	−0.010	July 2022	0.336	0.185	0.040
February 2020	0.290	0.203	−0.025	August 2022	0.331	0.178	0.041
March 2020	0.345	0.243	−0.009	September 2022	0.354	0.190	0.052
April 2020	0.343	0.243	−0.011	October 2022	0.288	0.147	0.029
May 2020	0.357	0.244	0.002	November 2022	0.303	0.153	0.039
June 2020	0.335	0.213	0.010	December 2022	0.353	0.188	0.053
July 2020	0.464	0.312	0.040	January 2023	0.291	0.141	0.038
August 2020	0.380	0.222	0.047	February 2023	0.248	0.131	0.006
September 2020	0.412	0.252	0.049	March 2023	0.311	0.165	0.035
October 2020	0.419	0.281	0.026	April 2023	0.255	0.122	0.021
November 2020	0.304	0.228	−0.036	May 2023	0.288	0.152	0.024
December 2020	0.181	0.262	−0.193	June 2023	0.334	0.177	0.045
January 2021	0.715	0.224	0.380	July 2023	0.306	0.156	0.038
February 2021	0.381	0.210	0.060	August 2023	0.336	0.164	0.060
March 2021	0.767	0.266	0.389	September 2023	0.316	0.166	0.039
April 2021	0.306	0.235	−0.041	October 2023	0.313	0.148	0.054
May 2021	0.281	0.173	−0.004	November 2023	0.301	0.143	0.045
June 2021	0.311	0.194	0.005	December 2023	0.346	0.180	0.054

Notes: This table reproduces monthly difference-in-differences estimates from Figure 1 (blue bars). Columns (1) and (2) in each row reproduce the raw monthly purchase probability in each group, as shown in Figure B.2. Column (3) shows the difference between the two groups, normalized to the average difference between April and June of 2020.

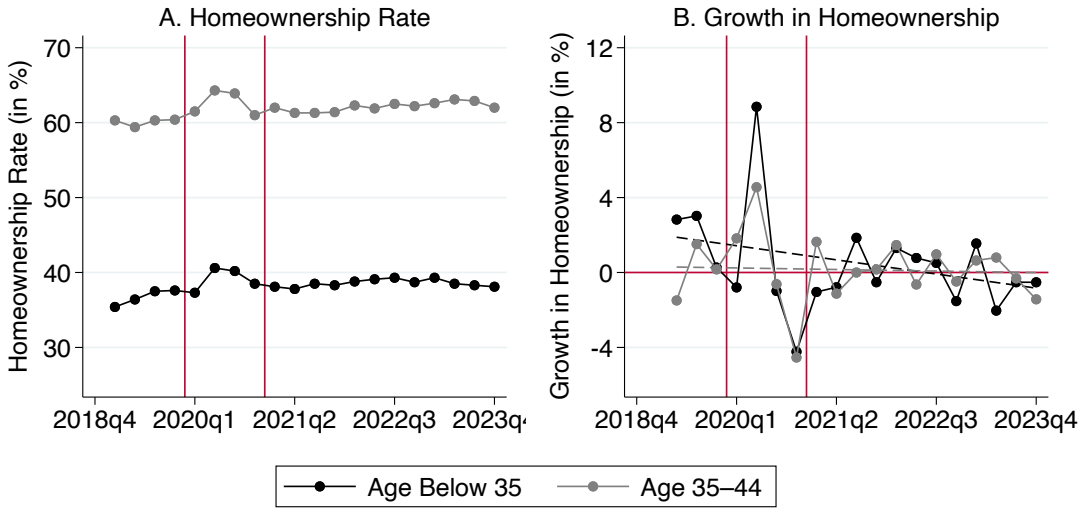


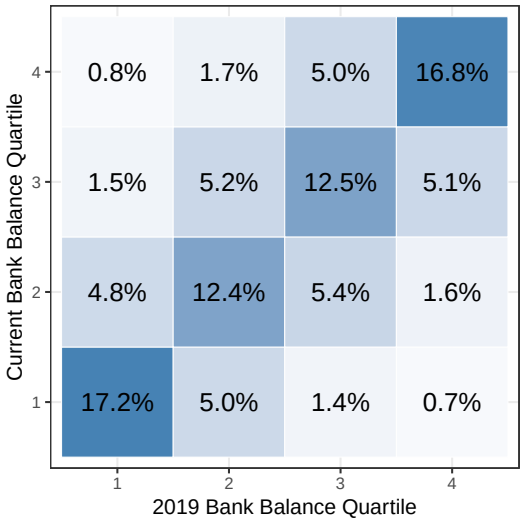
Figure B.3: Homeownership Rates and Growth in the US

Notes: This figure depicts trends in homeownership among young individuals in the US over the period 2019–2023. Panel A contrasts homeownership rates among individuals under 35 with those aged 35–44. Panel B shows quarter-to-quarter growth rates in homeownership for the two groups. The underlying time-series data come from the Current Population Survey (IPUMS CPS, 2025).

B.2. Heterogeneity by Income, Liquid Assets, and Buyer Type

Using administrative data, we identify each individual's disposable household income and household bank balances for each year. Individuals are then grouped into quartiles based on their 2019 values for both variables. This approach ensures that the covariates are measured prior to the policy and remain fixed over time. One limitation is that some individuals may still have been living with their parents in 2019, in which case income and assets reflect those of the parental household. To assess the stability of these classifications, we compare 2019 quartile assignments with those in later years (2020–2023) at the individual-year level. As shown in Figure B.4, most individuals remain in the same quartile over time. We therefore proceed by classifying individuals based on their 2019 income and liquid asset levels.

(a) Household Liquid Assets



(b) Household Income

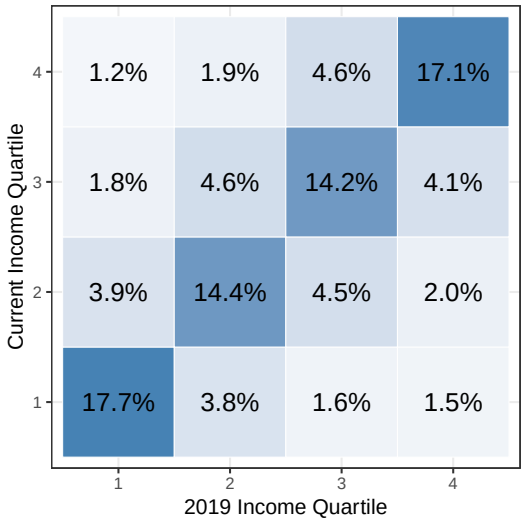


Figure B.4: Income and Liquid Assets: 2019 vs. Current

Notes: This figure shows confusion matrices that compare individuals' liquid assets (income) in 2019 to their liquid assets (income) in all later years. Cells on the diagonal from bottom-left to top-right denote individuals who remain in the same liquid asset (income) quartile as in 2019.

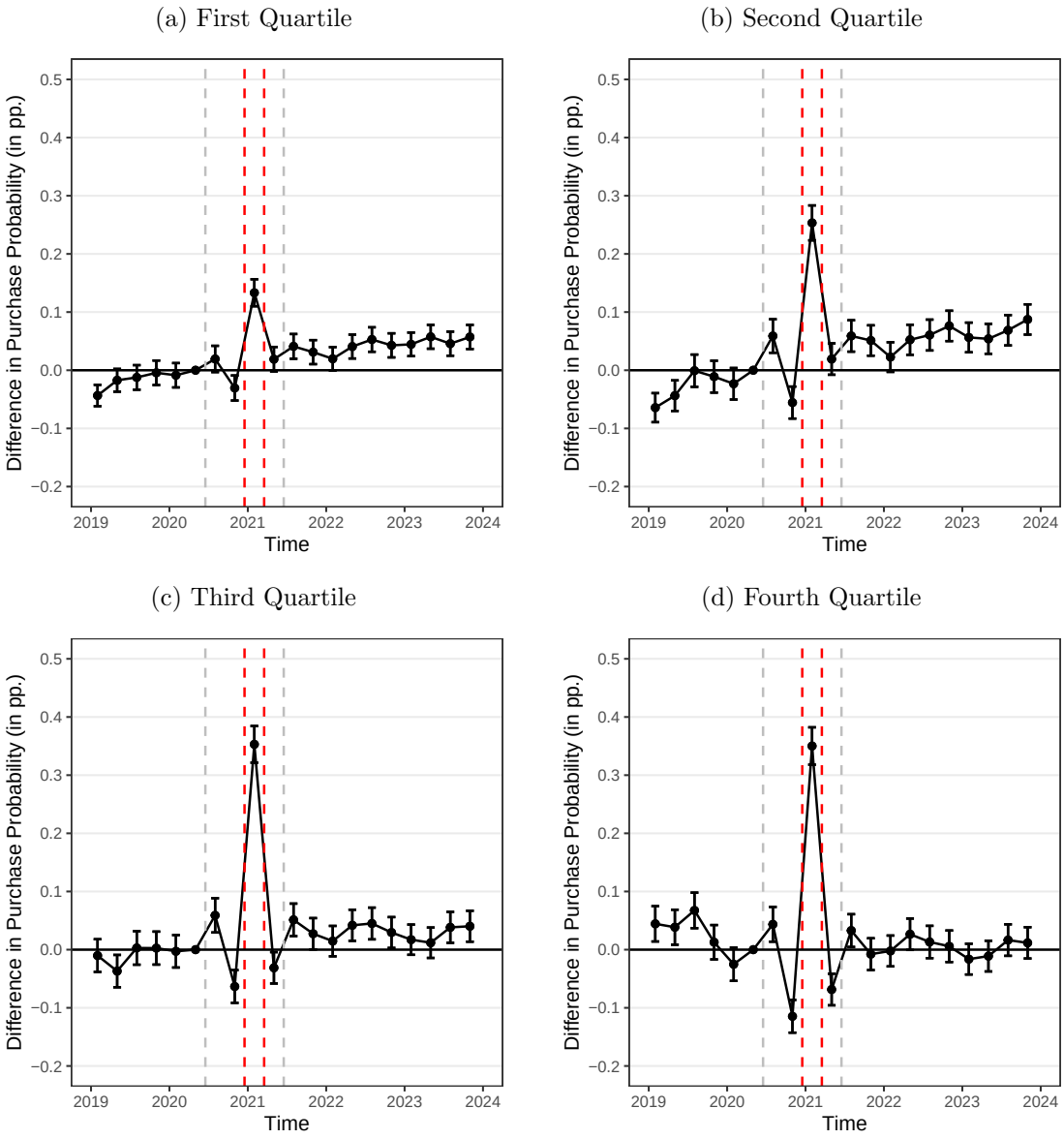


Figure B.5: The Effect of the Transfer Tax Exemption by Liquid Asset Quartiles

Notes: This figure shows the difference in home purchase probabilities between individuals aged 30–34 and those aged 38–42 across different household liquid asset quartiles. In each subsample, differences are normalized to their 2020Q2 level. Black vertical bars indicate 95% confidence bounds.

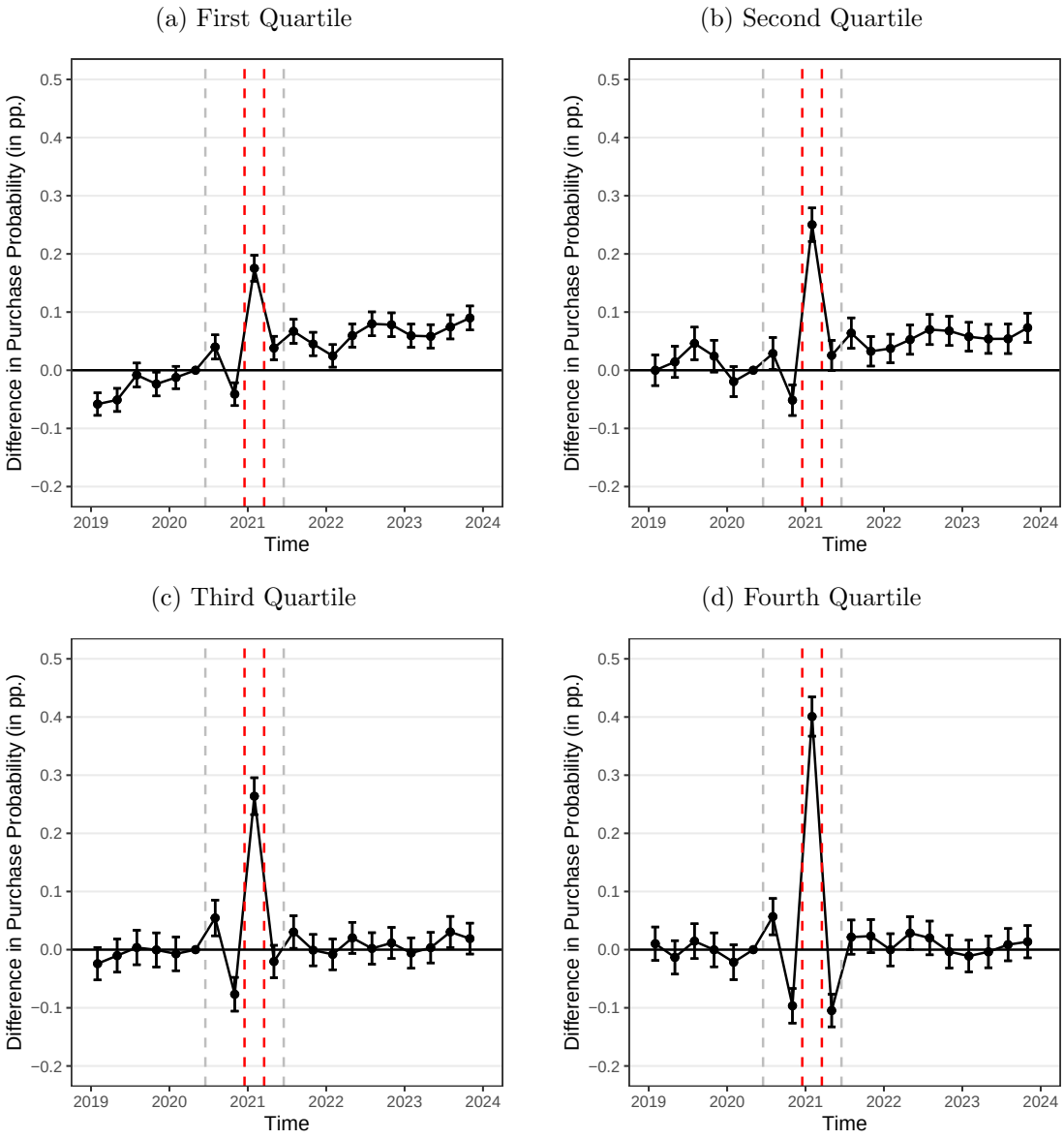
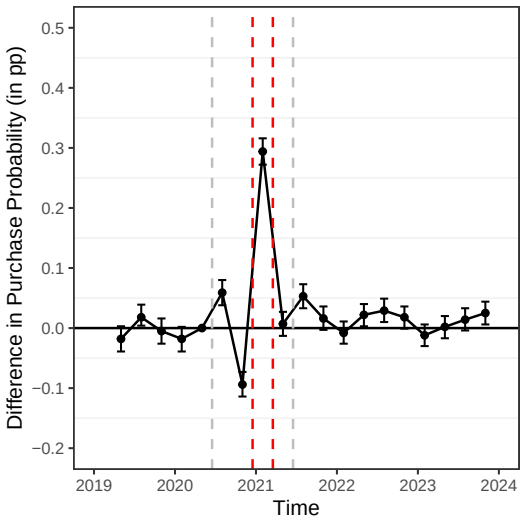


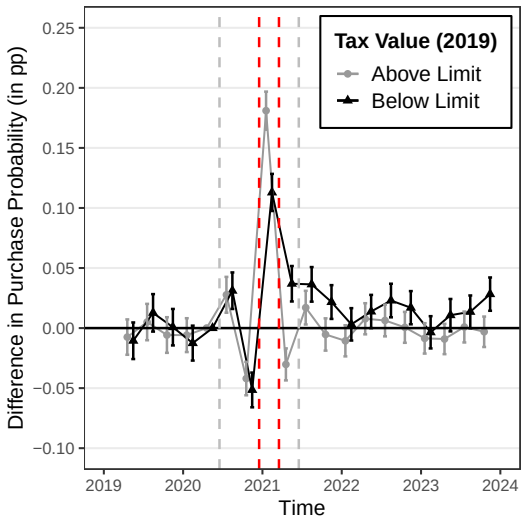
Figure B.6: The Effect of the Transfer Tax Exemption by Income Quartiles

Notes: This figure shows the difference in home purchase probabilities between individuals aged 30–34 and those aged 38–42 across different household income quartiles. In each subsample, differences are normalized to their 2020Q2 level. Black vertical bars indicate 95% confidence bounds.

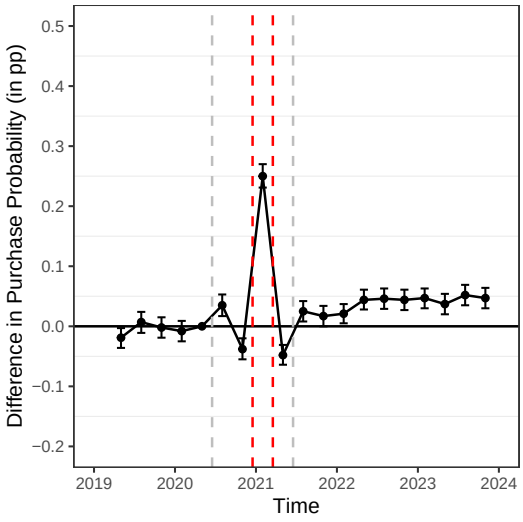
(a) First-Time Buyers: All Purchases



(b) First-Time Buyers by Unit Type



(c) Repeat Buyers: All Purchases



(d) Repeat Buyers by Unit Type

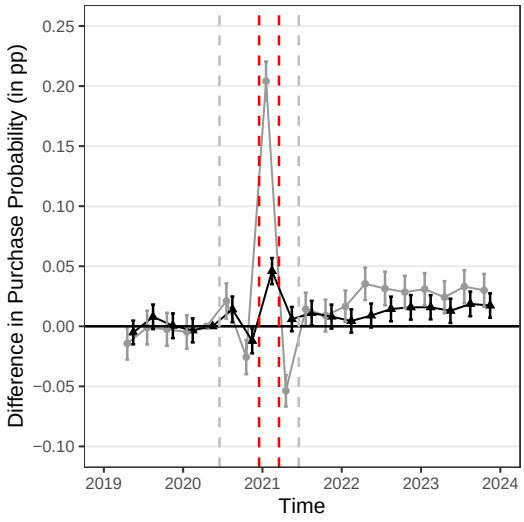


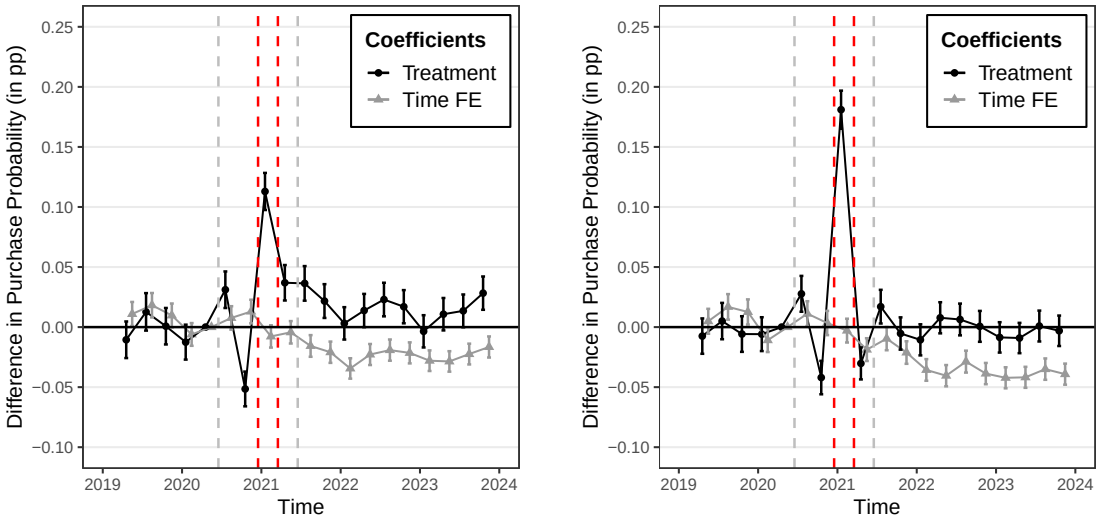
Figure B.7: The Effect of the Transfer Tax Exemption on First-Time vs. Repeat Purchases

Notes: This figure shows the effect of the transfer tax exemption on home purchase behavior, separately for non-owners (i.e., first-time buyers; panels (a) and (b)) and existing homeowners (i.e., repeat buyers; panels (c) and (d)). Black vertical bars indicate 95% confidence bounds. Panels (a) and (c) on the left-hand side of the figure present results for all purchases, whereas the right-hand side panels (b) and (d) illustrate effects for purchases in the eligible, lower-value vs. ineligible, higher-value segment of the housing market. The evidence suggests that the effect on repeat buyers' purchase behavior is not a direct response to the transfer tax exemption (for which high-value units did not qualify), but is a second-order effect, whereby price increases in the lower segment allowed them to trade up.

B.3. More Details on Crowding Out

(a) First-Time: Low-Value Units

(b) First-Time: High-Value Units



(c) Repeat: Low-Value Units

(d) Repeat: High-Value Units

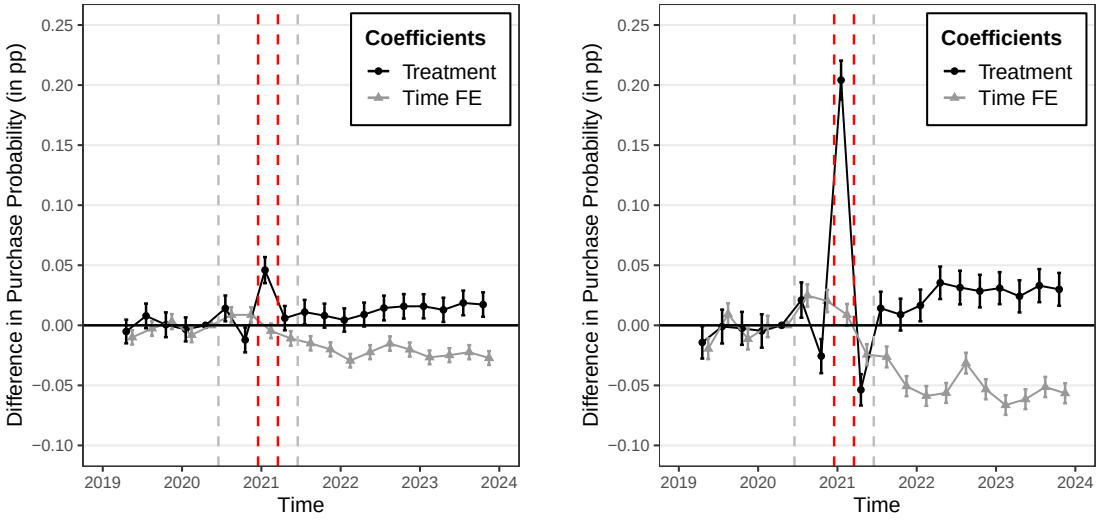


Figure B.8: Crowding Out by Buyer Type and Housing Market Segment

Notes: This figure analyzes crowding out of older homebuyers by distinguishing between unit types (low- vs. high-value) and buyer types (first-time vs. repeat buyers). As in Figure 2 in the main text, the plots show not only estimated treatment effects, but also trace the purchase behavior of older individuals (in grey). In this group, the largest decline is found among existing owners buying high-value units (panel (d)). This contradicts a crowding-out interpretation of the policy, which would suggest a particularly strong decline in purchases among low-value, eligible units.

B.4. Additional Results for Timing and Ownership Analyses

Table B.2: The Importance of Timing Responses for the 2021Q1 Effect

	Estimate (Table B.1 column (3)) (1)	Assumed Counter-factual (2)	Implied Missing Transactions (3)	Implied Excess Transactions (4)
Pre Policy Introduction – Anticipation Period				
November 2020	−0.036	0.000	−0.036	
December 2020	−0.193	0.000	−0.193	
After Policy Introduction – No Price Limit Period				
January 2021	0.380	0.035		0.345
February 2021	0.060	0.035		0.025
March 2021	0.389	0.035		0.354
After Policy Introduction – Cooldown Period				
April 2021	−0.041	0.035	−0.076	
May 2021	−0.004	0.035	−0.039	
June 2021	0.005	0.035	−0.030	
			$\sum = 0.350$	

	Estimate (Table 1, column (1)) (5)	Additional Transactions ($= \sum /3$) (6)	Share of Additional Transactions (7)	Share of Retimed Transactions (8)
No Price Limit Period (21Q1)	0.272	0.117	42.95%	57.05%

Notes: This table illustrates our back-of-the-envelope calculation for the relative importance of time reoptimization around policy reform dates vs. extensive-margin responses in explaining the strong spike in purchases in the first quarter of 2021, i.e., the period without a price limit. Column (1) shows monthly treatment effect estimates, which underlie the blue bars in Figure 1 and are presented in column (3) of Appendix Table B.1. As a counterfactual for this exercise, we assume that the pre-policy treatment effects for November and December 2020 would be zero if young homebuyers had not postponed their purchases to the initial period without a price limit (see the upper block of column (2)). Post-policy, we assume a counterfactual difference between both groups equal to the medium-run treatment effect as given by our medium-run estimate in column (1) of Table 1 (see lower two blocks of column (2)). We calculate the implied missing and excess transactions due to the policy in columns (3) and (4), respectively. The sum over these two columns gives the additional home purchases compared to our assumed counterfactual scenario. We then scale this number to three months and compare it to our initial-period estimate from column (1) of Table 1. Columns (7) and (8) present the relative importance of additional vs. retimed transactions, respectively.

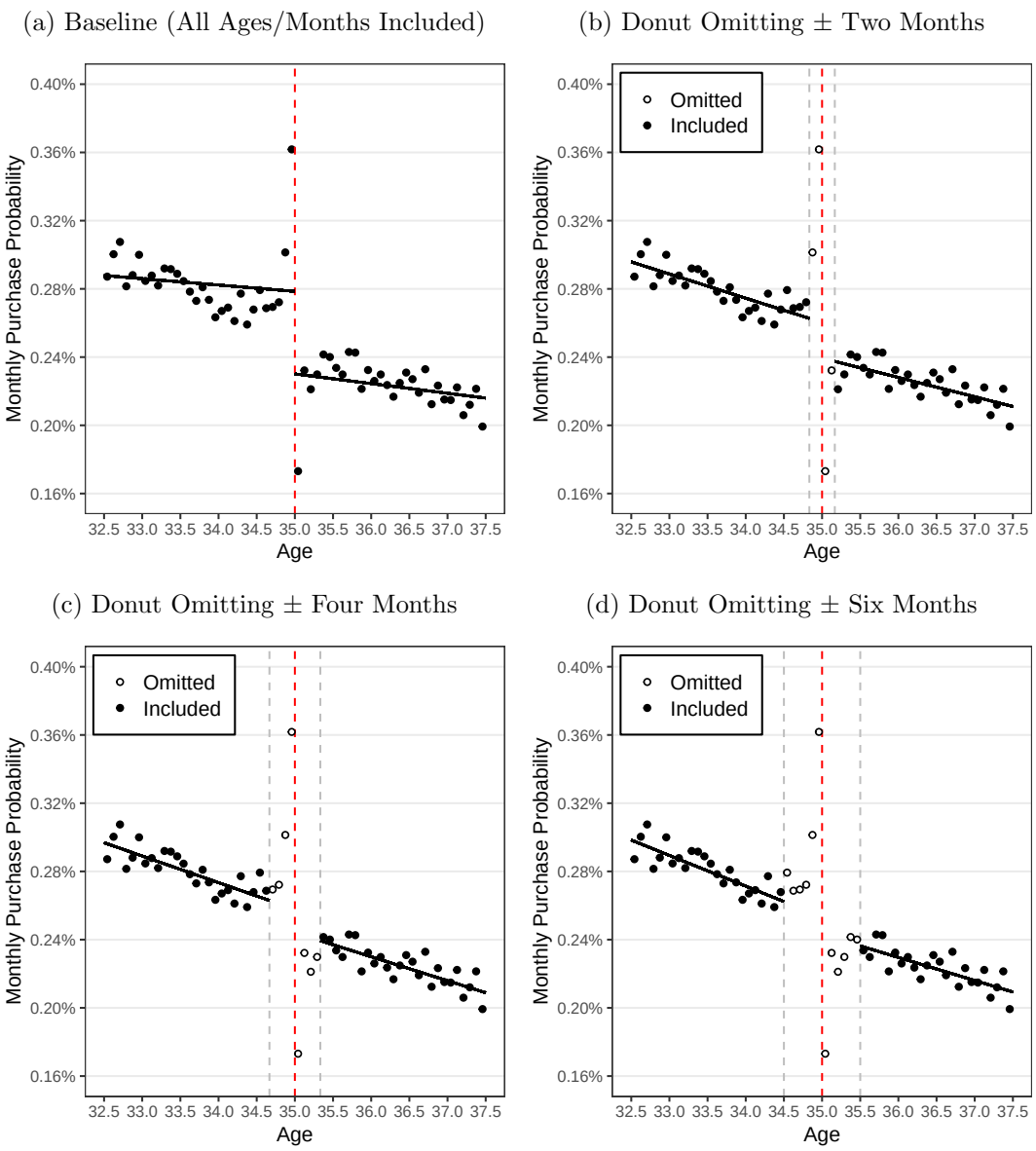


Figure B.9: Donut RD Evidence on the Transfer Tax Exemption

Notes: This figure shows the result of estimating the linear RD model from Equation (2) and the effect when excluding individuals close to the age cutoff ("donut RD"). Panel (a) replicates the baseline post-policy RD from Figure 3. In panels (b)–(d), two, four, and six months are excluded from either side of the cutoff, respectively. In each panel, the solid black line depicts the corresponding linear RD estimates. Additional details on the estimation are given in the main text.

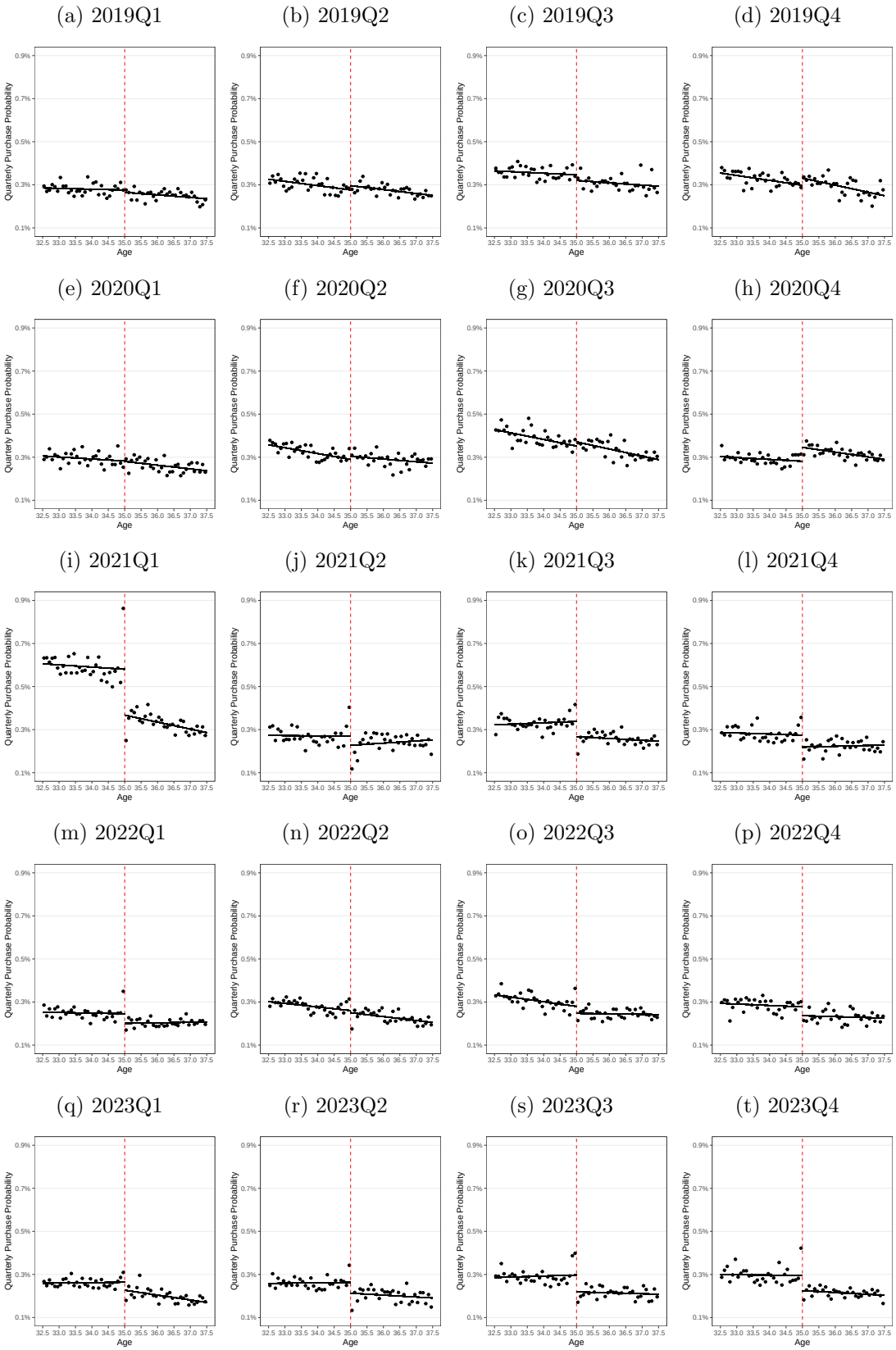


Figure B.10: Quarterly RD Evidence on the Transfer Tax Exemption

Notes: This figure shows the results from estimating the pooled RD model from Figure 3, separately for each quarter of the sample. All details on the estimation are given in the main text.

Table B.3: Extensive-Margin Responses to the Transfer Tax Exemption – RDD and RKD Results

Panel A – Discontinuity in Homeownership by Year and Month of Birth

	1.1.2019 (1)	1.1.2020 (2)	1.1.2021 (3)	1.1.2022 (4)	1.1.2023 (5)	1.1.2024 (6)
Young ($MoB_i \geq$ Jan'86)	0.411*	0.350	0.181	0.500**	0.286	0.238
	(0.216)	(0.214)	(0.213)	(0.211)	(0.209)	(0.208)
Young $\times$ $MoB_i^{(\text{Jan'86})}$	−1.801***	−1.572***	−1.411***	−1.107***	−0.961***	−0.778***
	(0.150)	(0.149)	(0.148)	(0.147)	(0.146)	(0.145)
$MoB_i^{(\text{Jan'86})}$	−2.144***	−1.820***	−1.553***	−1.279***	−1.031***	−0.899***
	(0.106)	(0.106)	(0.105)	(0.104)	(0.103)	(0.103)
Intercept	57.668***	59.912***	61.508***	63.280***	64.483***	65.376***
	(0.150)	(0.149)	(0.148)	(0.146)	(0.145)	(0.145)
Age at Cutoff	33	34	35	36	37	38
Person-by-Year Obs.	837k	837k	837k	837k	837k	837k
Adjusted-R^2	0.008	0.006	0.004	0.003	0.002	0.001

Panel B – Homeownership at Age 38 in Year 2024 (Post Policy)

	Regression Discontinuity			Regression Kink Design		
	1st Ord. (7)	2nd Ord. (8)	3rd Ord. (9)	1st Ord. (10)	2nd Ord. (11)	3rd Ord. (12)
Young ($Age_i \leq 38$)	0.238	0.232	0.397			
	(0.208)	(0.314)	(0.428)			
Young $\times$ $Age_i^{(38)}$	0.778***	−0.628	−0.536	0.777***	−0.662	−0.782
	(0.145)	(0.580)	(1.472)	(0.145)	(0.578)	(1.446)
$Age_i^{(38)}$	0.899***	1.644***	1.987**	0.829***	1.476***	1.515*
	(0.103)	(0.400)	(0.967)	(0.083)	(0.330)	(0.825)
Squared $Age_i^{(38)}$		−0.309*	−0.669		−0.252*	−0.284
		(0.160)	(0.935)		(0.140)	(0.841)
Young $\times$ Squared $Age_i^{(38)}$		0.053	0.826		−0.071	−0.124
		(0.225)	(1.362)		(0.150)	(0.897)
Cubic $Age_i^{(38)}$			0.099			0.008
			(0.254)			(0.234)
Young $\times$ Cubic $Age_i^{(38)}$			0.007			−0.031
			(0.357)			(0.355)
Intercept	65.376***	65.087***	65.023***	65.491***	65.190***	65.177***
	(0.145)	(0.209)	(0.266)	(0.104)	(0.156)	(0.208)
Person-by-Year Obs.	837k	837k	837k	837k	837k	837k
Adjusted-R^2	0.001	0.001	0.001	0.001	0.001	0.001

(continued on next page)

Table B.3 (Continued)

Panel C – Homeownership at Age 38 in Year 2020 (Pre Policy Placebo)

	Regression Discontinuity			Regression Kink Design		
	1st Ord. (13)	2nd Ord. (14)	3rd Ord. (15)	1st Ord. (16)	2nd Ord. (17)	3rd Ord. (18)
Young ($Age_i \leq 38$)	−0.058	0.059	0.085			
	(0.211)	(0.320)	(0.437)			
Young × $Age_i^{(38)}$	0.481***	−0.319	−1.293	0.482***	−0.331	−1.352
	(0.146)	(0.588)	(1.494)	(0.146)	(0.584)	(1.460)
$Age_i^{(38)}$	1.114***	1.677***	2.279**	1.130***	1.636***	2.181***
	(0.102)	(0.396)	(0.961)	(0.082)	(0.329)	(0.824)
Squared $Age_i^{(38)}$		−0.233	−0.861		−0.219	−0.781
		(0.157)	(0.925)		(0.139)	(0.833)
Young × Squared $Age_i^{(38)}$		0.142	0.417		0.110	0.214
		(0.227)	(1.375)		(0.150)	(0.900)
Cubic $Age_i^{(38)}$			0.173			0.154
			(0.250)			(0.231)
Young × Cubic $Age_i^{(38)}$			−0.263			−0.273
			(0.360)			(0.356)
Intercept	66.828***	66.606***	66.493***	66.801***	66.632***	66.525***
	(0.143)	(0.209)	(0.267)	(0.105)	(0.158)	(0.211)
Person-by-Year Obs.	805k	805k	805k	805k	805k	805k
Adjusted-R^2	0.002	0.002	0.002	0.002	0.002	0.002

Notes: This table summarizes the estimated effect of the transfer tax exemption on homeownership rates of eligible individuals. All coefficients are to be interpreted as percentage point differences. In panel A, we depict discontinuities in the homeownership rate around the treatment cutoff at birth year 1986 as reported in the annual cross-sections of the homeownership register. Treatment effects refer to $\widehat{\beta^t}$ estimates from the following regression: $Owner_i^t = \alpha^t + \beta^t \cdot \mathbb{1}_{(MoB_i \geq \text{Jan'86})} + \gamma^t \cdot MoB_i^{(\text{Jan'86})} + \delta^t \cdot \mathbb{1}_{(MoB_i \geq \text{Jan'86})} \cdot MoB_i^{(\text{Jan'86})} + \varepsilon_i^t$, which we estimate $\forall\ t \in [2019, 2024]$. The running variable is an individual's birth year and month (MoB_i), recentered around January 1986. We include 30 month-cohorts on either side of the discontinuity. In panels B and C, we present estimates from various regression discontinuity and regression kink designs for the years 2024 (latest post-policy data year) and 2020 (pre policy). In both panels, we check for discontinuities and kinks, respectively, in the homeownership register at age 38, the age of the oldest cohort that received treatment at some point in their lives. Discontinuities refer to $\widehat{\beta^t}$ estimates from the following regression: $Owner_i^t = \alpha^t + \beta^t \cdot \mathbb{1}_{(Age_{it} \leq 38)} + \gamma^t \cdot Age_{it}^{(38)} + \delta^t \cdot \mathbb{1}_{(Age_{it} \leq 38)} \cdot Age_{it}^{(38)} + \varepsilon_{it}$, which we estimate for $t = 2020$ and $t = 2024$. Regression kink results refer to $\widehat{\delta}$ estimates from a similar regression that omits the separate intercept for the younger group. Standard errors, clustered at the individual level, are reported in parentheses (significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

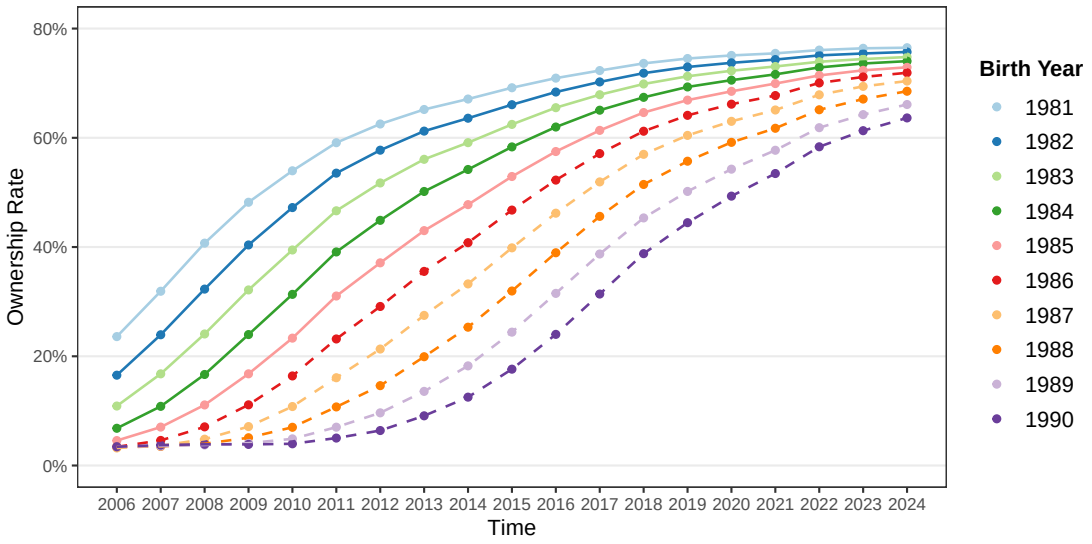


Figure B.11: The Impact of the Transfer Tax Exemption on Homeownership

Notes: This figure descriptively tracks the life-cycle pattern of homeownership across birth-year cohorts. Dashed lines depict "ever treated" cohorts, i.e., individuals who were eligible for the tax exemption at some point in their life. Ownership information is based on household-level wealth records. Specifically, the plotted variable indicates whether an individual belonged to an owner-occupant household that was not the parental household.

B.5. Auxiliary Results for Intensive-Margin Analyses

Table B.4: Intensive-Margin Responses by Buyer Type

	Purchase Amount (1)	Tax Value (2)	Floor Area (3)	Postcode Income (4)	Over-bidding (5)
Panel A – First-Time Buyers					
Treated ×					
Pre-Policy Period (19Q1–20Q1)	−33.8	0.005	−0.025*	−0.001	0.000
	(31.1)	(0.014)	(0.013)	(0.004)	(0.003)
Anticipation Period (20Q3–20Q4)	−25.8	0.004	−0.015	−0.001	−0.001
	(34.5)	(0.015)	(0.015)	(0.005)	(0.003)
No Price Limit Period (21Q1)	1,293.4***	0.077***	−0.016	0.008*	−0.003
	(45.5)	(0.017)	(0.016)	(0.005)	(0.003)
Cooldown Period (21Q2)	−0.7	−0.083***	−0.056***	−0.029***	0.006*
	(39.2)	(0.017)	(0.017)	(0.005)	(0.004)
Medium Run (21Q3–23Q4)	209.4***	−0.022*	−0.019	−0.009**	−0.001
	(29.3)	(0.013)	(0.013)	(0.004)	(0.003)
Number of Observations	13.9mn	132k	71k	136k	71k
Panel B – Repeat Buyers					
Treated ×					
Pre-Policy Period (19Q1–20Q1)	−50.6*	−0.023*	0.008	−0.003	0.001
	(30.2)	(0.012)	(0.011)	(0.004)	(0.002)
Anticipation Period (20Q3–20Q4)	−28.9	−0.009	0.033**	−0.001	0.000
	(34.7)	(0.013)	(0.013)	(0.004)	(0.002)
No Price Limit Period (21Q1)	1,286.7***	0.041***	0.013	0.008*	0.001
	(48.3)	(0.016)	(0.015)	(0.005)	(0.003)
Cooldown Period (21Q2)	−312.4***	−0.137***	−0.017	−0.023***	0.013***
	(38.6)	(0.017)	(0.016)	(0.005)	(0.004)
Medium Run (21Q3–23Q4)	245.1***	−0.021*	0.041***	−0.003	−0.001
	(29.2)	(0.012)	(0.011)	(0.004)	(0.002)
Number of Observations	17.8mn	105k	45k	108k	45k

Notes: This table summarizes the estimated intensive-margin effect of the transfer tax exemption, separately for first-time (panel A) versus repeat buyers (panel B). Estimated treatment effects $\widehat{\beta}_k$ are based on the difference-in-differences model specified in Equation (1). Column (1) is based on the baseline person-by-quarter panel dataset. Estimates in columns (2) and (4) stem from a transaction-level dataset including all purchases from the baseline panel dataset of individuals 30–34 and those 38–42. Estimates in columns (3) and (5) are based on a subset of transactions for which we have additional broker data from NVM. Treatment is defined either at the individual level (column (1)) or at the buyer level (columns (2)–(5)). Dependent variables are: (1) an individual's monthly home purchase volume in euros, (2) the natural logarithm of a unit's tax-assessed value as of January 2019, (3) the natural logarithm of a unit's floor area, (4) the natural logarithm of median postcode income in 2018, and (5) the log ratio between a unit's final sale price and the initial list price. Standard errors, clustered at the individual/transaction level, are reported in parentheses (significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

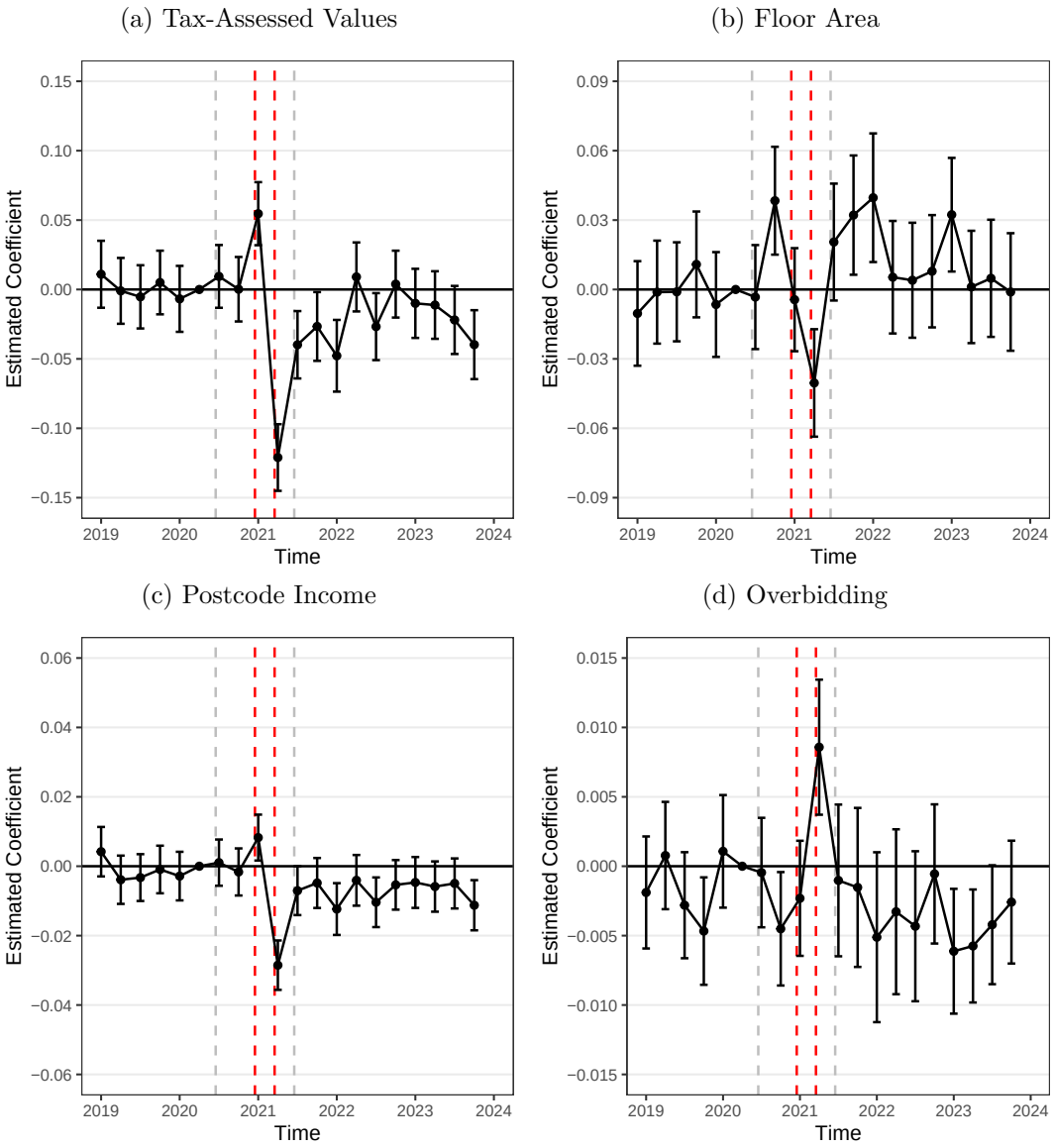


Figure B.12: Intensive-Margin Responses to the Transfer Tax Exemption

Notes: This figure shows the effect of the transfer tax exemption on homebuyers at the intensive margin. All plots are based on Equation (1), estimated on the transaction sample. The dependent variables are: log of 2019 tax-assessed values (panel (a)); log of floor size in m^2 (panel (b)); log of postcode median income (panel (c)); log of sale price minus log of list price (panel (d)). Black vertical bars indicate 95% confidence bounds.

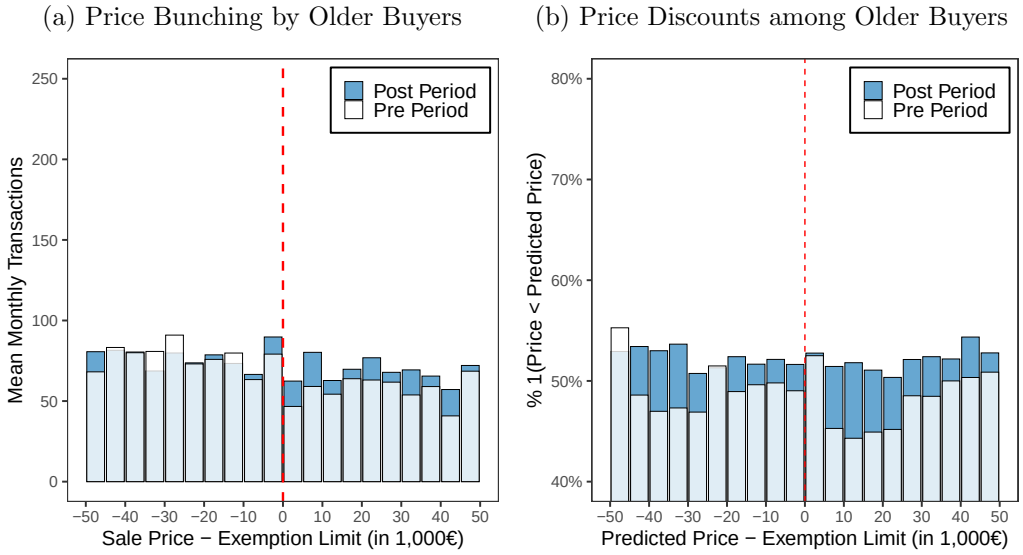


Figure B.13: Older Buyers' Non-Response to the Transfer Tax Notch

Notes: This figure shows the average monthly count of home purchases of buyers aged 35 or older for the periods 2019Q1–2020Q1 (pre) and 2021Q3–2023Q4 (post). Panel (a) groups transactions into €5,000 bins according to their realized sale price. In contrast to the strong bunching response by young buyers documented in Figure 6, there is no evidence for bunching around the notch among older buyers, who are ineligible for the policy. Panel (b) groups transactions into €5,000 bins using a predicted price. We impute these price predictions by running the following regression on the transaction dataset: $\ln P_{it} = \beta \cdot \ln WOZ_{it} + \gamma_{c(i),t} + \varepsilon_{it}$, with WOZ denoting the most recent tax value and postcode-by-quarter fixed effects $\gamma_{c(i),t}$. Transactions with realized prices within −€25,000 and €24,999 of the exemption limit are removed from the estimation. We then forecast prices based on the estimates $\widehat{\beta}$ and $\widehat{\gamma}_{c,t}$. Again, in contrast to transactions involving young buyers, there is no evidence for systematic price discounts in transactions involving older buyers.

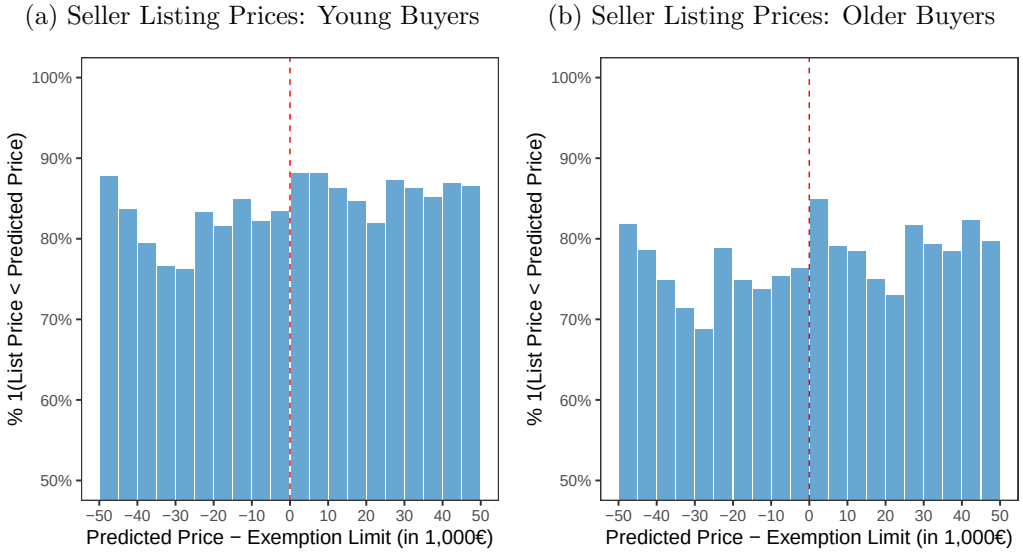


Figure B.14: The Role of Sell-Side Brokers Around the Transfer Tax Notch

Notes: This figure investigates whether the underpayment of young buyers documented in panel (b) of Figure 6 reflects list price setting by the sellers. Both panels group transactions from the period 2021Q3–2023Q4 (post) into €5,000 bins using a predicted price. Price predictions are explained in Figure B.13. On the y-axis, we depict the share of transactions with a list price below that predicted price. However, we find no evidence for systematically low list prices in transactions with a young buyer, suggesting that the observed pattern in Figure 6 reflects direct negotiation outcomes between (young) buyers and sellers.

B.6. Auxiliary Results for the Price Analysis

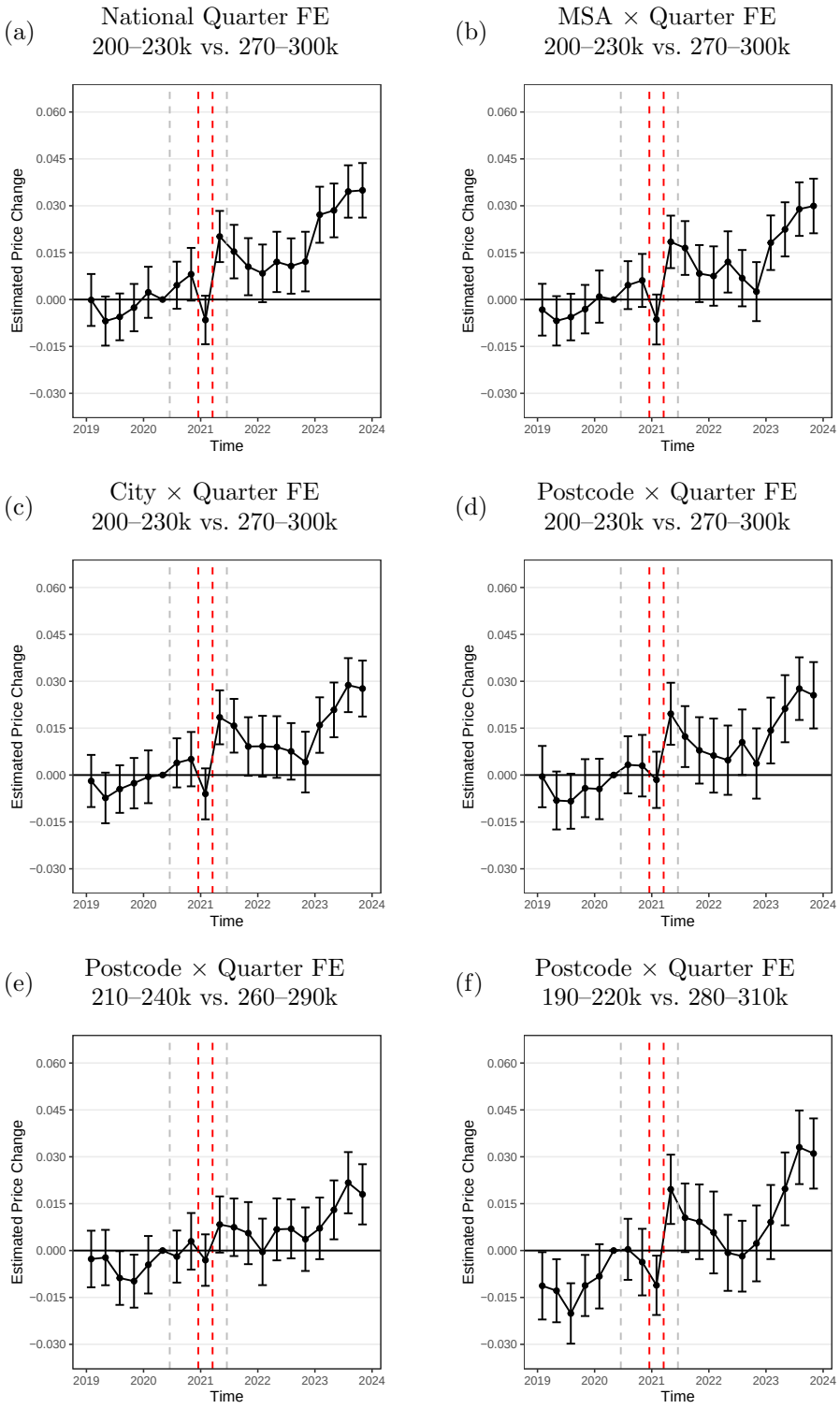


Figure B.15: The Effect of the Transfer Tax Exemption on House Prices

Notes: This figure shows the estimated effect of the transfer tax exemption on the price of tax-exempt properties, based on Equation (4). 2020Q2 is the omitted quarter. The different panels correspond to columns (1)–(6) in Table 5. Black vertical bars correspond to 95% confidence bounds.