Frontloading Wealth: How Parental Transfers Shape Housing and Savings

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Abstract. Parents transfer substantial wealth to their children during their lifetime, yet the timing and consequences of these inter vivos transfers remain poorly understood. I show that most parental transfers occur around first-time home purchases, while transfers are largely unrelated to other life events. Because they are predominantly received by already wealthier children, these transfers increase absolute wealth inequality in the short run. Next, I identify the causal effect of parental home purchase transfers on housing outcomes and subsequent wealth accumulation, exploiting exogenous variation from a Dutch tax-exemption policy. While transfers promote wealth accumulation by enabling homeownership, recipients use them primarily to reduce mortgage debt. As a result, the initial wealth gains dissipate over time, as recipients consume the savings from lower debt service rather than reinvesting them.

JEL Codes: D31, D64, E21, G51, R21

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1. Introduction

Financial transfers from parents and other relatives have long been established as a central component of household wealth (Gale and Scholz, 1994). Their relevance has been growing further in recent years, driven by demographic change and rising asset prices. Across advanced economies, the share of inherited wealth relative to national income is rising rapidly (The Economist, 2025). Yet while the academic and political debate has largely centered on inheritances, parents transfer a sizable share of their total estate while still alive (Yang and Ripoll, 2023). Crucially, *inter vivos* transfers occur earlier in life than inheritances. By affecting individuals at formative stages of the life cycle, they shape future wealth accumulation and inequality. Despite their importance, however, our understanding of parental inter vivos transfers remains limited, largely due to data constraints and a lack of credible identification strategies.

To overcome these challenges, this study leverages detailed administrative data from the Netherlands, paired with exogenous variation from a gift tax reform. The Dutch administrative records offers a number of key advantages over previous studies. They contain family network information that links individuals to their parents and provides disaggregated wealth data for both generations. They also include all reported financial transfers, with details on gift size and the identities of donor and recipient. Moreover, the data record the timing of home purchases and other major life events, enabling me to examine the incidence of parental transfers in an event study framework, rather than relying solely on cross-sectional variation. Finally, the data covers a period during which the incidence of transfers increased exogenously due to a policy change, allowing for a causal analysis of how parental transfers affect subsequent wealth accumulation.

I first document that first-time home purchases are the primary life event associated with parental transfers, whereas few transfers are made around other events. Notably, parents do not make any transfers after an involuntary job loss, suggesting that transfers are not used as a risk-sharing mechanism within extended families. While the direction of causality cannot be established in an event-study framework, the likelihood of receiving a parental transfer increases tenfold in the year of a first-time purchase and remains elevated thereafter. On average, first-time buyers in my sample receive an additional €3,200 in parental support during the year of purchase. Importantly, these are unconditional averages across all homebuyers, including those who do not receive a transfer.

To explore how transfers are distributed across the wealth distribution, I estimate separate event studies by percentiles of parental liquid assets. Buyers below the median receive less than €1,000 on average, whereas those with parents at the 95th and 99th percentiles receive approximately €10,000 and €34,000, respectively. A similar but less pronounced gradient emerges when conditioning on recipients' own assets: transfers predominantly accrue to those with higher initial liquid assets. These patterns suggest that, at least in the short run, parental transfers reinforce rather than mitigate wealth inequality. Turning to other sources of heterogeneity, I find that larger transfers are received female and single buyers.

Next, I investigate the impact of targeted parental transfers for first-time home purchases (*home purchase transfers*) on future wealth accumulation. Because these transfers

occur early in adulthood, they can meaningfully shape life-cycle wealth trajectories. By decomposing *transfer wealth* based on how the funds are invested, I show that the implications for future wealth accumulation are not clear ex ante. On the one hand, transfer wealth may continue to grow over time if recipients invest in additional housing or liquid assets, or use the funds to deleverage costly debt—widening the wealth gap between recipients and non-recipients over time. On the other hand, if recipients use the funds primarily to finance consumption, transfer wealth will dissipate, and the initial gap will narrow in the longer run.

Empirically, testing the effect of home purchase transfers on future wealth accumulation is challenging, since a child's home purchase and the parent's transfer decision are likely jointly determined within families. I address this endogeneity by exploiting a Dutch policy reform that temporarily raised exemption limits specifically for home purchase transfers. The policy triggered a sharp, short-lived surge in such transfers: While the exemption limit was raised, 4.7 % of all first-time homebuyers received a parental transfer, compared to 9.6 % in the year prior. Because the reform primarily affected wealthy parents who stood to benefit from higher exemption limits, I construct an instrument that interacts the reform period with initial parental liquid assets. I use this instrument to estimate the effect of parental home purchase transfers on housing decisions and on subsequent wealth accumulation.

Home purchase transfers can affect wealth accumulation in two ways: at the extensive margin, by enabling children to buy a home at all, and at the intensive margin, by influencing the type of home purchased and the associated borrowing decision. In my setting, the extensive margin response is largely predetermined, since transfers made under the special exemption were explicitly earmarked for housing-related expenditures. Nonetheless, I can use the observed spike in home purchases that is predicted by my instrument to investigate wealth accumulation in subsequent years.

2SLS estimates show that, relative to the full control group—including non-buyers—transfer recipients continue to accumulate more wealth in the years following their purchase. Seven years after the purchase, transfer recipients hold an additional €2.5 per euro of initial transfer. This is exclusively driven by the extensive margin: transfer recipients earn additional housing returns relative to those who remain renters, due to the fact that house prices appreciated considerably in the period under investigation.

However, when compared to other homebuyers without parental support, a different picture emerges. I find that, rather than buying more valuable properties or making alternative investments, recipients use parental transfers entirely to reduce their mortgage balances. As a result, they enter homeownership with lower liabilities—and therefore higher net wealth—than comparable buyers without parental support. Over time, however, this initial wealth gap narrows. After seven years, transfer recipients hold only €0.7 per euro of initial transfer, compared to buyers in the same year without a transfer. Decomposing transfer wealth into its components reveals why: rather than adopting faster amortization schedules or reinvesting savings from reduced debt service, recipients use their higher residual income (net of mortgage payments) to finance greater consumption. Taken together, these findings suggest that home purchase transfers primarily *frontload* wealth, rather than promoting sustained accumulation. In the longer

run, parental support enables young homeowners to smooth consumption rather than widen wealth inequalities.

Contribution to the Literature

This article contributes to several fields of research. First, it contributes to the literature on intergenerational wealth persistence—a topic long of interest to economists (Charles and Hurst, 2003) but receiving renewed attention amid rising wealth inequality across countries. Two recent trends in this field are the increasing use of administrative data (Adermon, Lindahl, and Waldenström, 2018; Boserup, Kopczuk, and Kreiner, 2016; Boserup, Kopczuk, and Kreiner, 2018; Black et al., 2020) and improved identification strategies (Ager, Boustan, and Eriksson, 2021; Fagereng, Mogstad, and Rønning, 2021; Daysal, Lovenheim, and Wasser, 2023). One strand of this literature examines the role of inheritances and transfers in shaping wealth accumulation (Gale and Scholz, 1994; Boserup, Kopczuk, and Kreiner, 2016; Elinder, Erixson, and Waldenström, 2018; Palomino et al., 2022; Nekoei and Seim, 2023; Black et al., 2025). However, whereas these studies typically provide a snapshot by examining cross-sectional wealth at a point in time, I follow transfer recipients over multiple years, allowing for a richer understanding of long-run effects and behavioral responses. In this respect, my work complements Colmsjö (2024), who studies the effects of intra-family home sales—another channel of inter-generational wealth transfer—on long-run wealth accumulation. A key advantage of my data is that I can observe all financial transfers directly and identify their effects through policy-induced variation.

A second, related field of research examines the drivers of parental transfers. Early models of parental altruism view cash transfers primarily as a risk-sharing mechanism to help children smooth consumption (Cox, 1990; Altonji, Hayashi, and Kotlikoff, 1997; McGarry, 1999), the more recent literature emphasizes the importance of tax considerations (Kopczuk, 2007; Sommer, 2017; Escobar, Ohlsson, and Selin, 2023) and life events (Leopold and Schneider, 2011 McGarry, 2016; Andersen, Johannesen, and Sheridan, 2020; Lee et al., 2020; Boileau and Sturrock, 2023; Kvaerner, 2023) in explaining parental transfers. However, most of these studies rely on survey data or focus narrowly on a single life event. For instance, Kvaerner (2023) studies the effect of negative health shocks experienced by parents on financial transfers. The comprehensive administrative data I use allow me to analyze multiple major life events children experience and compare their relative importance in relation to parental transfers.

Third, a growing body of evidence highlights the importance of parental wealth in the housing market. While early studies examine inter vivos transfers based on survey data (Engelhardt and Mayer, 1998; Guiso and Jappelli, 2002), more recent work combines reduced-form estimates from administrative data with life-cycle models to trace the role of parental wealth in housing markets in greater detail (Blickle and Brown, 2019; Brandsaas, 2021; Benetton, Kudlyak, and Mondragon, 2022; Wold et al., 2024; Landen Mammos, 2025). I extend this literature by examining parental wealth at an unprecedented level of detail. Specifically, I group individuals into percentiles based on their parents' wealth to study how parental support in housing transactions varies across

the distribution.

Finally, I add to the empirical literature that examines marginal propensities to consume (MPC) in response to changes in housing wealth (Aladangady, 2017) and, more specifically, to changes in mortgage debt and debt service cost (Di Maggio et al., 2017; Fan and Yavas, 2020; Agarwal et al., 2022). My findings align with Ganong and Noel (2020), who show that changes in debt service costs trigger consumption responses, whereas principal reductions that leave debt service costs unchanged have no effect. Moreover, my finding that transfer recipients forgo their initial wealth advantage by not increasing savings is in line with Bernstein and Koudijs (2024), who show that homeowners do not reduce other forms of savings when facing higher monthly costs from amortization.

The remainder of the paper is structured as follows. Section 2 introduces the institutional context and describes the data. Section 3 examines the incidence of parental transfers around first-time home purchases and contrasts this with other major life events. Section 4 investigates the implications of home purchase transfers for future wealth accumulation. Section 5 concludes.

2. Institutional Setting & Data

In the Netherlands, households typically aspire to enter homeownership at a relatively young age. This is facilitated by a well-developed mortgage market without down payment requirements and further encouraged by generous tax treatment, including substantial mortgage interest deductibility and low transaction costs. It is also partially driven by a limited alternatives in the rental market. While the Dutch social housing sector is the largest in Europe, accounting for around a third of all housing units in the country, waiting times can be up to 10 years in the most crowded urban markets (Van Dijk, 2019). By contrast, supply in the private rental market is relatively scarce.

Rising house prices over the past decade have made access to homeownership increasingly difficult for young households. Although banks offer mortgages covering the full property value, low- and middle-income households are often constrained by tight debt-service-to-income ratios. Prices have risen most in the densely populated Randstad region (Amsterdam, Utrecht, The Hague, Rotterdam), where new construction has not kept pace with demand. As a result, the average age of first-time homebuyers has increased from 34.9 year to 36.7 years between 2012 and 2020. These developments have generated substantial political pressure, prompting the government to introduce policies aimed at improving access to homeownership. Measures include an exemption of transfer taxes for young buyers and an outright ban on buy-to-let transactions, intended to increase the supply of owner-occupied housing (Francke et al., 2023). These policies—along with declining house prices—have lowered the average age of first-time buyers to 34.4 years in 2023. Nevertheless, concerns about housing access and affordability remain central to the political debate.

2.1. Taxation of Parental Transfers in the Netherlands

Against this backdrop, parental wealth has become an increasingly important factor in the Dutch housing market. For children of wealthy parents, financial transfers can bridge the gap between borrowing capacity and the price of the desired home. As in most countries, the Dutch gift tax system combines marginal tax rates that rise with transfer size and a set of exemption limits.[1] Both the kinks in the tax schedule and the exemption limits are adjusted annually, roughly in line with inflation. To maintain clarity, I focus on the key rules and major policy changes, illustrating them with examples from selected years rather than documenting each annual adjustment.

The marginal tax rate for parental transfers is either 10% or 20%, depending on the size of the transfer. In 2011, at the start of the sample, the threshold at which the marginal tax rate increased was €118,708. Three exemptions were in place. First, a regular annual exemption allowed parents to transfer up to €5,030 tax-free. Second, a special one-time exemption applied to parental transfers of up to €24,144, regardless of purpose. These two exemptions remained largely unchanged over time, aside from annual inflation adjustments. By 2023, the regular and special exemption limits had risen to €6,035 and €28,947, respectively.

In addition, a separate one-time exemption exists for parental transfers explicitly intended for housing-related expenditures (such as home purchases or mortgage prepayments) or to finance expensive education. Unlike the other exemptions, this one was changed several times. In 2011, the limit was €50,030 and remained around that level until October 2013, when the government unexpectedly raised it to €100,000—but only for housing-related transfers.[2] In 2015, the limit was lowered again, before being raised for a second time in 2017. It then remained at the higher level until the end of 2022, when the special exemption was abolished altogether. Figure 1 illustrates the timeline of these policy changes, which are discussed in more detail in Section 4.

Although the special exemption limits—particularly, that for housing-related transfers—allow for sizable tax-free transfers, gift tax rules in the Netherlands remain relatively strict compared to other developed economies. In the United States, a lifetime exemption of $14 million makes virtually all parental transfers tax-exempt. In Germany, no annual exemption exists, but parents can transfer up to €400,000 in a tax-free fashion every 10 years. Similarly, France allows tax-free transfers of up to €100,000 every 15 years.

2.2. Data Sources and Sample Construction

To study the effect of parental wealth transfers on life-cycle outcomes of young individuals, I rely on extensive administrative data provided by Statistics Netherlands (CBS). The data cover a wide range of information on every person living in the Netherlands

[1] To avoid confusion with other types of transfer taxes (e.g., property transfer tax), I refer to the tax on financial transfers between two individuals as gift tax, as is common in practice.

[2] The government's main motivation for raising the exemption limit for housing-related transfers was to stimulate the housing market and reduce the prevalence of underwater mortgages following the post–Global Financial Crisis slump.

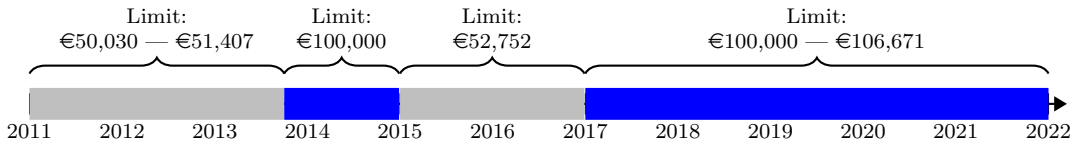


Figure 1: Timeline of the Special Exemption Limit for Housing-Related Transfers

Notes: This figure traces important changes to the special tax treatment of transfers for housing-related expenditures. While housing-related transfers are subject to a more generous tax exemption limit throughout the sample, this special tax exemption treatment is changed at various points. The most high-profile policy change occured in October 2013, when the special tax exemption limit was first raised to €100,000.

from 2011 to 2023, reported annually. A unique feature of the Dutch administrative data is the information on family network ties, which allows me to link individuals to their parents, whose administrative records are fully available, too. These records provide detailed annual household-level balance sheet information, including a breakdown of net wealth into different assets and liabilities. One challenge includes the treatment of parents who live in separate households. To ensure a fair comparison with cohabiting parents, I average all financial variables for parents living in separate households.

For the analysis, I include all individuals in the Dutch population who were born between 1972 and 1993 and had at least one living parent in 2011. I restrict the sample to those who are registered as household heads throughout the observation period. Since income and wealth are measured at the household level, this restriction ensures a clear separation between the resources of parents and children. A drawback of this approach is that the household head designation in different-sex couples typically refers to the male partner, introducing a gender imbalance: 67.4% of individuals in the sample are male. This imbalance represents a limitation, as parental transfer behavior, as shown below, differs by the child's gender.

Since I focus on major life events as potential triggers of parental transfers, I further restrict the sample to individuals who, in 2011, are non-homeowners, unmarried, childless, and formally employed. The resulting cohort of 133,762 individuals is followed in a balanced person-by-year panel from 2011 to 2023. While the child is the unit of observation, I also track all parental wealth variables over time. First-time home purchases are identified from changes in property ownership records, and the characteristics of purchased properties are obtained from *Kadaster*, the Dutch land registry. To contrast these purchases with other major life events, I draw on additional datasets from Statistics Netherlands (CBS). The year in which individuals marry or have their first child is inferred from annual updates to the family network data, while job loss events are identified from records of layoffs due to firm bankruptcy.

Financial transfers are recorded in a separate CBS dataset. In each year, this includes all cash transfers registered with the tax authorities that exceed the regular annual exemption limit. Importantly, transfers made under special exemption policies must also be registered, even if no tax is due. However, very small gifts below the annual exemption limit may not be captured. For each recorded transfer, both donors and recipients are identified, along with the transfer amount and payable tax. This allows

me to capture, for every person and every year, the number and total amount of parental transfers received. In addition, it is recorded whether a transfer was made under one of the two special exemption limits.

2.3. Summary Statistics

Of all 133,762 individuals in the sample, 10,306 (7.8%) receive a parental transfer at some point between 2011 and 2023. In Table 1, key summary statistics are reported, separately for transfer recipients and non-recipients. All demographic and financial variables are measured in 2011, before any transfers are received.

Unsurprisingly, the most striking difference between recipients and non-recipients relates to their parents resources. Recipients' parents own, on average, about €219,000 in liquid assets (bank balances, stocks, and bonds), whereas non-recipients' parents hold only €53,000. Recipients themselves also hold more than twice as much in liquid assets as non-recipients, even before receiving any transfer. Recipients are also positively selected with respect to their income and education: while only around 48% of non-recipients hold at least a college degree, more than 76% of recipients do.

Turning to major life events, the statistics suggest an association with parental transfers. Transfer recipients more often experience "positive" life events (first-time home purchase, marriage and child birth), with the largest gap observed for first-time home purchases. While only around 34% of non-recipients become homeowners throughout the sample period, more than 59% of recipients do. The pattern is reversed for the sole negative life event considered—job loss due to firm bankruptcy—which transfer recipients experience less frequently. Whereas fewer than 20% of transfer recipients experience none of the major life events that I study, more than 42% of non-recipients do. In the following section, this association will be tested in a more formal way.

3. Parental Transfers for First-Time Home Purchases

In the first part of the empirical analysis, I investigate the incidence of parental cash transfers around first-time home purchases, and compare this to other major life events that children experience. A key strength of my data is that it includes information on different life events within the same sample, allowing for a quantitative comparison of their relative importance. Nonetheless, given that first-time home purchases (and other life events) are likely co-determined with parental transfers, I do not attempt to infer the direction of causality. In the first part of the analysis, I show that first-time home purchases are an important—and indeed the most relevant—life event that is associated with parental transfers. I assess to what extend the dominance of home purchases is driven by the special tax treatment. Next, I document substantial inequalities in transfers along the distributions of parent and child assets. Finally, I show additional heterogeneity based on parent and child characteristics.

Table 1: Summary Statistics

	Transfer Recipients			Non-Recipients		
Variable	P25	Mean	P75	P25	Mean	P75
Age	27.00	30.29	33.00	27.00	31.65	36.00
Hours Worked	26.54	31.78	39.08	25.69	31.48	38.10
Household Income (€)	20,599	30,112	36,624	18,608	26,556	31,759
Household Liquid Assets (€)	4,260	29,776	34,299	1,410	13,382	14,472
Household Net Wealth (€)	−593	26,329	32,405	−3,535	6,534	11,804
Parental Household Income (€)	41,404	64,706	81,638	27,388	43,033	53,936
Parental Liquid Assets (€)	46,304	218,567	310,612	6,194	52,647	55,078
Parental Net Wealth (€)	247,755	923,758	857,997	9,866	185,288	248,705
Share Female (%)		32.79			32.54	
Share College Educated (%)		76.46			48.17	
Share Single Household (%)		73.58			77,72	
Share First Home Purchase (%)		59.48			34.12	
Share Job Loss (%)		4.77			6.50	
Share First Child Birth (%)		48.60			36.15	
Share Marriage (%)		33.38			26.12	
Share No Life Event (%)		19.70			42.39	
Individuals		10,427			123,335	

Notes: This table shows summary statistics, separately for individuals who receive a parental transfer between 2011 and 2023 (recipients) and those who do not. All financial and demographic variables are measured in 2011 levels, i.e., before any transfer is received. All prices are adjusted for inflation based on the CPI and expressed in 2015 levels.

3.1. The Dominance of Transfers Around First-Time Home Purchases

At the start of the sample period in 2011, all individuals are renters and have not yet experienced any of the life events analyzed in this study. While a substantial share remains in this initial state throughout the sample, others experience one or more life events at different points in time. I examine how parental cash transfers evolve around the first occurrence of each event.[3]

In the baseline analysis, I consider each life event separately and estimate staggered event studies of the following form:

$$Y_{it}^{(d)} = \alpha_i^{(d)} + \delta_t^{(d)} + \sum_{k \neq -2} \beta_k^{(d)} \cdot D_i^{(d)} \cdot \mathbf{1}\left\{K_{it}^{(d)} = k\right\} + \varepsilon_{it}^{(d)}, \tag{1}$$

where $d \in \{\text{Home Purchase; Job Loss; Marriage; Birth of Child}\}$. The outcome variable in each model $Y_{it}^{(d)}$, denotes one of the measures of parental transfers introduced

[3]Because the data begin in 2011, I cannot rule out that some individuals experienced these life events prior to the sample period (e.g., they may have previously owned a home or been married). However, given that the sample consists of young adults, such cases are likely rare.

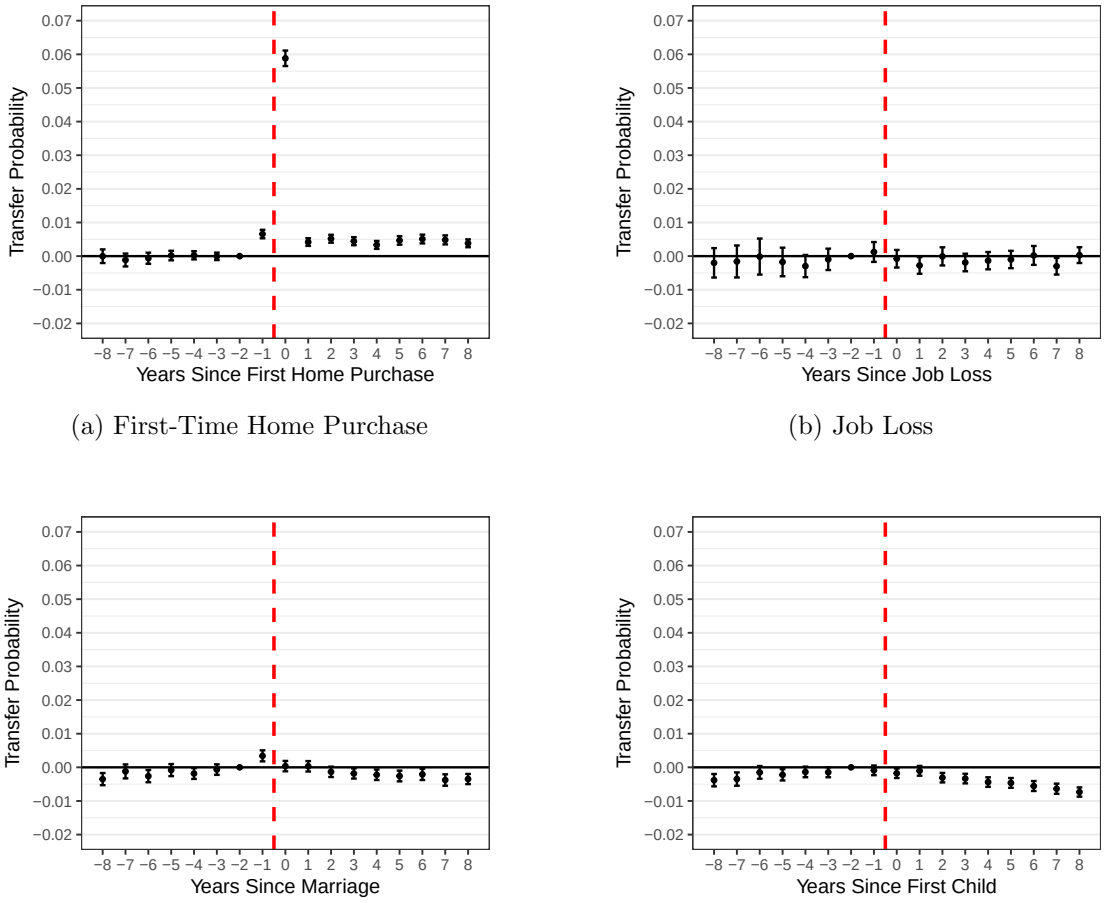
below. $D_i^{(d)}$ indicates whether person i ever experiences event d throughout the sample and $K_{it}^{(d)} = \left(t - T_i^{(d)}\right)$ indexes time relative to $T_i^{(d)}$, the year in which the event takes place. The coefficients $\beta_k^{(d)}$ trace the average difference in transfers between individuals who experience event d and those who never do, k years before or after the event. The coefficient for $k = -2$ is omitted, so that all dynamic effects are expressed relative to the difference two years before the event. Person fixed effects $\alpha_i^{(d)}$ control for time-invariant individual heterogeneity, and year fixed effects $\delta_t^{(d)}$ absorb common shocks. Finally, the event window is capped at eight years around the respective event and observations outside of that window are grouped in the end-cap variable (Miller, 2023).

I measure parental transfers in three ways. First, I construct a binary indicator for whether person i received any parental transfer in year t. The corresponding coefficients are visualized in Figure 2. All four life events are shown in panels (a)–(d), using a common vertical scale. The results are striking: while parental transfers respond strongly to first-time home purchases, they are largely unresponsive to other major life events. As shown in panel (a), pre-trends are flat in the years leading up to a first-time home purchase. In the year before the purchase, the likelihood of receiving a transfer increases by 0.7 percentage points on average, and by 6.0 percentage points in the year of the purchase itself. This represents roughly a tenfold increase relative to the incidence of transfers among individuals who experience no life event during the sample period. In subsequent years, the probability of receiving a transfer remains elevated by about 0.5 percentage points. This suggests that parents not only contribute to the initial home purchase but also continue to assist with subsequent housing-related expenses such as renovations or durable goods.

In contrast, there is no evidence of increased parental transfers following an involuntary job loss. This finding contrasts with Andersen, Johannesen, and Sheridan (2020), who document that parents often provide informal insurance against income shocks. For marriage, I observe a small but statistically significant increase in transfers in the year preceding the event. Finally, there is no evidence that parents make transfers around the birth of their first grandchild. If anything, the incidence of parental transfers declines in the years after their children have children of their own.

Considering net parental transfers (in €) as an alternative outcome, a similar picture emerges. This is illustrated in Figure 2. For first-time home purchases, I find that the average (after-tax) transfer increases by €3,200 in the year of the purchase, and remains around €300-400 higher in the following eight years. Importantly, these are unconditional averages: for individuals who do not receive a transfer, net transfers are coded as 0 rather than NA. Thus, these numbers represent the additional financial support that the average homebuyer can expect from their parents around the time of the first purchase. Regarding other life events, I again find little to no evidence for parental support. The sole exception is marriage, before which parental transfers increase by around €200.

Next, I focus on an alternative outcome that is not based on formal transfers. There are two reasons for this: first, since cash transfers below the annual exemption threshold (around €6,000) do not need to be registered, the administrative data may understate the



(a) First-Time Home Purchase

(b) Job Loss

(c) Marriage

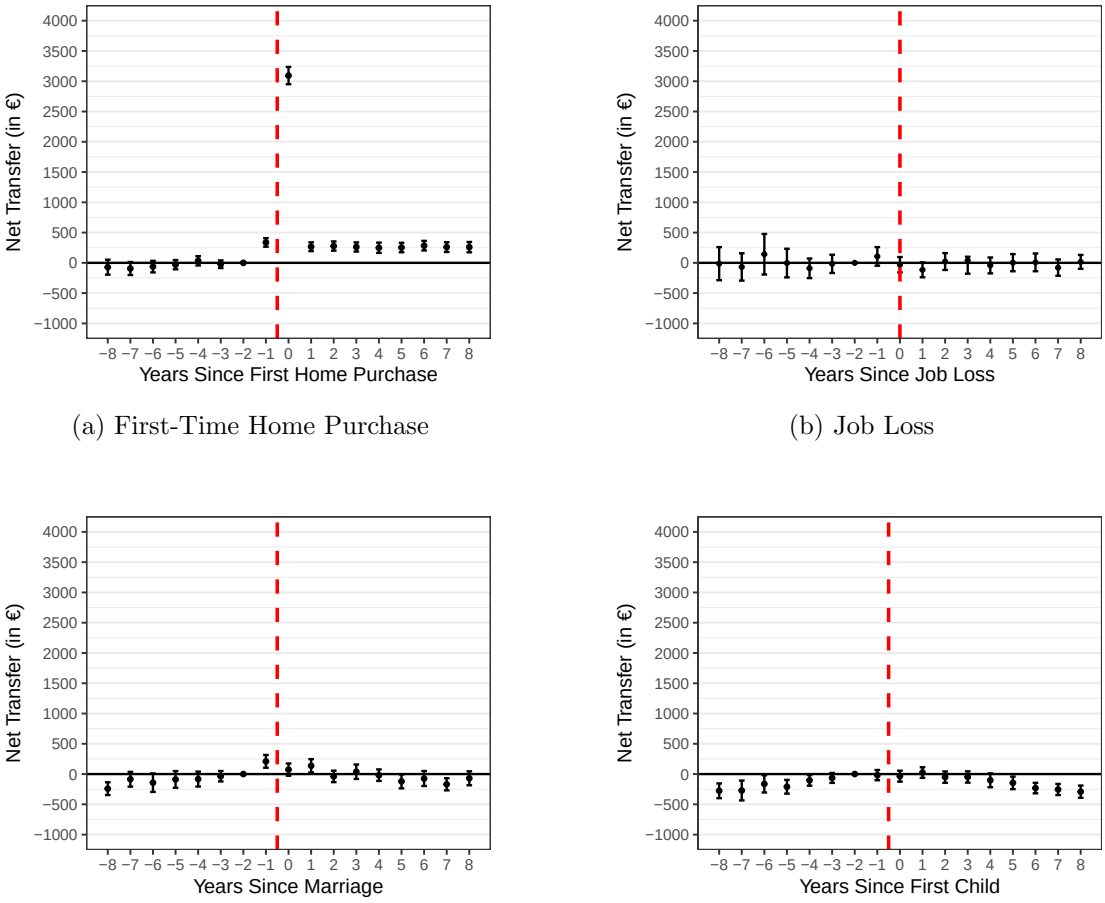
(d) Birth of Child

Figure 2: Parental Transfers Around Major Life Events

Notes: This figure presents event study coefficients estimated from equation (2), where all four major life events are modeled separately. The dependent variable is a binary indicator equal to one if a parental transfer was received in a given year. The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.

incidence of small transfers. Second, instead of making an outright transfer, parents may offer financial support by covering some of their child's expenditures after a life event. For instance, parents may take over (some of) the cost of their child's wedding ceremony directly, rather than making a registered cash transfer. To capture such informal support, I examine changes in parental bank deposits around each of the four life events. While both forms of support—registered transfers and direct payments—should manifest as a drop in parental deposits following the event, I interpret drops in parental deposits that do not coincide with registered transfers as evidence for informal support.

For first-time home purchases, this pattern is evident. After similar pre-trends, parental deposits fall by about 4 percent in the year of purchase and continue to decline



(a) First-Time Home Purchase

(b) Job Loss

(c) Marriage

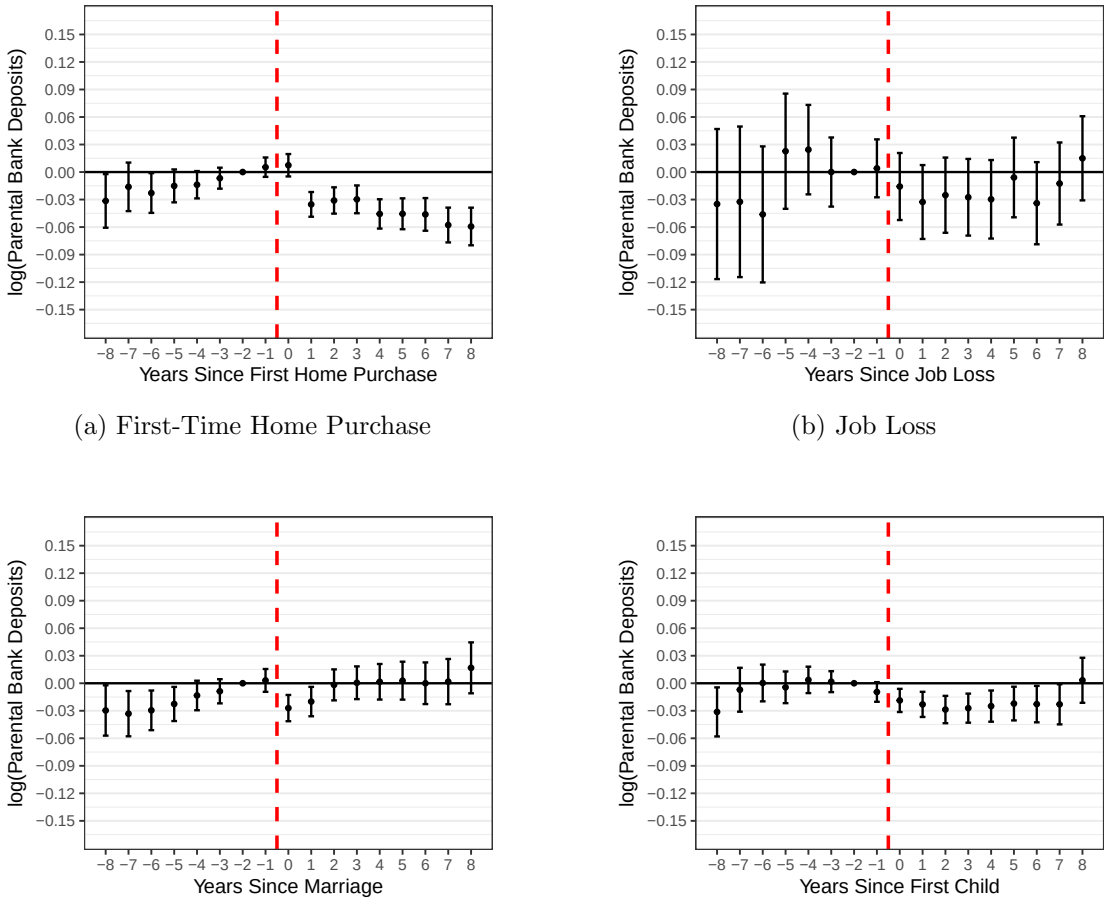
(d) Birth of Child

Figure 3: Net Parental Transfers Around Major Life Events

Notes: This figure presents event study coefficients estimated from equation (2), where all four major life events are modeled separately. The dependent variable is the net transfer (in €) that a person has received in a given year. Years without transfer are coded as 0, rather than NA. The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.

in subsequent years, consistent with the sustained elevation in parental transfers documented earlier. For involuntary job loss, estimates are imprecise given the small number of such events, but there is no visible break in trend, suggesting that parents do not provide insurance even informally by covering expenses.

In contrast, parental deposits decline by nearly 3 percent around the time of marriage, consistent with parents helping to finance marriage-related expenses such as wedding ceremonies. Finally, around the birth of a first child, there is again evidence of informal support: although no registered transfers are observed, parental deposits decline steadily after the event. This suggests that, in addition to the inter-generational time transfers, which are established in the literature (Eibich and Siedler, 2020), grandparents also



(a) First-Time Home Purchase

(b) Job Loss

(c) Marriage

(d) Birth of Child

Figure 4: Parental Bank Deposits Around Major Life Events

Notes: This figure presents event study coefficients estimated from equation (2), where all four major life events are modeled separately. The dependent variable is the natural logarithm of parental bank deposits in a given year. The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.

provide monetary assistance by covering some of their children's expenses.

3.2. Robustness Analysis

One key issue with the baseline event study specification is that it ignores correlations between different life events. As a result, it may fail to correctly attribute the role of each individual event. For example, if marriage typically occurs shortly before or after the first-time purchase, home purchase transfers may be mistakenly attributed to marriage. Therefore, I implement a combined model, in which all life events are considered jointly:

$$Y_{it} = \alpha_i + \delta_t + \sum_d \sum_{k \neq -2} \beta_k^{(d)} \cdot D_i^{(d)} \cdot \mathbf{1}\left\{K_{it}^{(d)} = k\right\} + \varepsilon_{it}. \tag{2}$$

In contrast to (1), this model includes a full set of event time dummies for all four life events. Therefore, it identifies the association with parental transfers for each event, after partialling out the effect of all other events. The omitted group comprises those who do not experience any of the four events during the sample, accounting for 40.6% of all individuals. The resulting coefficients, reported in Figure A.1, align very closely with the baseline model, suggesting that correlations in the timing of events is not a first-order concern. To investigate this further I remove, for each individual who experiences more than one life event, observations after that first event occurs. This further reduces potential confounding, as the post-event coefficients are purged of any influence from additional events. The results, shown in Figure A.3, again confirm that formal parental transfers are primarily directed toward supporting children's first-time home purchases, while other events do not elicit a response.

One reason for modest parental support around life events other than first-time purchases is that they may not be important enough, in financial terms. To test this, I estimate how an individual's own bank balance evolves around each life event. The resulting coefficients are shown in A.2. Notably, for all events except involuntary job loss, parallel trends are clearly violated. This is unsurprising, given that first-time home purchases, marriage and child birth are endogenous from the perspective of the individual. Nevertheless, there is a discernible change in bank deposits around the time of each event. For instance, people who have their first child accumulate bank deposits at an increased rate in the years leading up to the event, before reducing their balance quickly afterwards. This suggests that the small response of parental transfers to life events other than first-time home purchases is not due to these events being financially insignificant for the child.

Finally, I address the special tax treatment of housing-related transfers in driving the baseline results. A legitimate concern is that the sharp increase in parental transfers around first-time home purchases may partly reflect the more generous exemption threshold for housing-related gifts, as discussed in the previous section. I devise two additional tests to investigate this concern. First, I examine how transfers for first-time home purchases vary over time, related to changes in the special exemption threshold. To that end, I estimate the difference in parental transfers between those who make a first-time purchase and those who do not, separately for each year in the sample.[4] The resulting estimates are shown in Figure A.4, with years in which the special exemption threshold was raised highlighted in blue. The pattern is clear: home-purchase-related transfers respond strongly to the tax treatment. In years when the special exemption was raised to €100,000 or more, first-time homebuyers were substantially more likely to receive parental transfers than in other years. This response is most pronounced in

[4]Concretely, I estimate the following model: $Y_{it} = \delta_t + \sum_t \beta_t \cdot D_{it} + \varepsilon_{it}$, where D_{it} indicates that person i has made a first-time home purchase in year t.

2014—the year of the first major policy change—which I analyze in more detail in the following section. However, even in years when the exemption threshold was around €50,000, first-time homebuyers remained 3–4 percentage points more likely to receive a transfer compared to non-buyers.

Nevertheless, because housing-related transfers were always subject to some form of special exemption during the sample period—only the size of this exemption varied over time—this analysis cannot fully rule out tax-based explanations. Therefore, I take one step further and identify, for each transfer in the data, whether it was made under a special exemption or not. In a final robustness test, I retain only regular (i.e., non–tax-exempt) transfers and repeat the event-study analysis. This yields the most conservative estimate of the increase in parental transfers around home purchases.[5]

The resulting estimates are shown in Figure A.5. The implications are twofold. First, the special tax treatment of housing explains a substantial share of the quantitative magnitude of the observed spike in transfers. Focusing only on regular transfers, the increase in parental transfers in the purchase year is about 0.6 percentage points, compared to roughly 6 percentage points for all transfers. Second, in qualitative terms, the dominance of housing-related transfers is not driven by tax policy. Even when focusing only on non–tax-exempt transfers, first-time home purchases remain the only life event associated with a significant increase in parental support.

3.3. Transfers Across the Wealth Distribution

Ultimately, the main determinant of a financial transfer are the parents' available resources. Transfers can only be made by the parents who have the means to do so. The administrative data allow me to investigate in detail how transfers vary with parental wealth. To that end, I rank each person in the sample according to their parents' total liquid assets (bank deposits plus financial securities), measured in 2011. In Figure A.6, the baseline event study is estimated separately in each quartile of parental liquid assets. It suggests that, unsurprisingly, parental home purchase transfers depend strongly on the amount of liquid assets that parents have access to. Importantly, it also suggests that the parallel trends assumption continues to hold if the sample is split by parental assets.

Building on this, I then group people into percentiles of parental liquid assets and repeat the baseline analysis within each percentile. There are vast differences in parental resources across the sample. At the median, parents' total liquid assets are equal to €17,000. By contrast, parents in the 10th percentile of the liquid asset distribution hold only €800, while those in the 90th percentile hold more than €130,000. Given these stark inequalities, financial transfers are expected to rise nonlinearly across the distribution. Given a sample size of at least 1,000 individuals in each of the 100 percentiles, there is sufficient statistical power to conduct 100 separate event studies, in order to investigate the transfer patterns across the distribution in details. Since pre-trends are virtually

[5] Because a special exemption for housing-related transfers existed throughout the sample, this restriction retains only parents who were unable or unwilling to file for an exemption—for example, because a large transfer had already been made previously.

flat in the baseline analysis, I focus here on estimating a single treatment effect for each percentile. Specifically, I restrict the sample to observations from the pre-period (up to two years before the first-time home purchase) and the purchase year. I then collapse the dynamic model from (1) to a static specification, comparing transfers made in the purchase year with those made during the entire pre-period. In other words, I estimate only the size of the spike in transfers immediately surrounding the purchase, neglecting transfers made shortly before or in the subsequent years.

The estimates, shown in Figure 5, reveal pronounced inequalities in parental support across first-time homebuyers. Up to the 60th percentile, the average first-time buyer receives less than €2,000 in parental support, while those at the 80th percentile receive approximately €3,000 on average. At the 90th and 95th percentile, buyers receive approximately €4,500 and €10,000, respectively. Finally, children with the wealthiest parents, at the top of the liquid asset distribution, receive more than of €30,000 upon entering homeownership. To reiterate, these are unconditional averages calculated over all people within each percentile group, including those who do not receive a transfer. This suggests that children from affluent families do not need to wait for an inheritance to access parental resources. Instead, parents with sufficient means use their child's first-time home purchase as an opportunity to pass on wealth proactively.

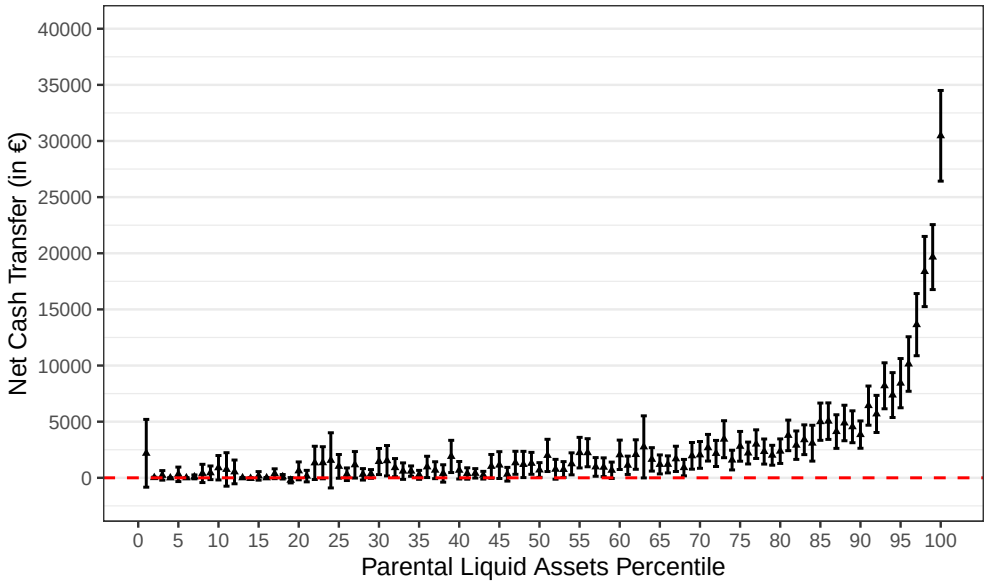
A similar, though less pronounced, pattern emerges when considering heterogeneity by child (i.e., recipient) assets. Although the gradient is less steep, panel (b) indicates that children with higher liquid assets in 2011 receive more transfers in the year of their first-time home purchase. This pattern largely reflects the positive inter-generational correlation in wealth. On average, children at the median of the liquid assets distribution receive around €2,000 in parental support, while those at the 90th and 95th percentile receive around €5,000 and €7,500, respectively. The fact that transfers are predominantly received by wealthier children suggests that, at least in the short run, parental transfers increase absolute wealth inequality.

3.4. Other Sources of Heterogeneity

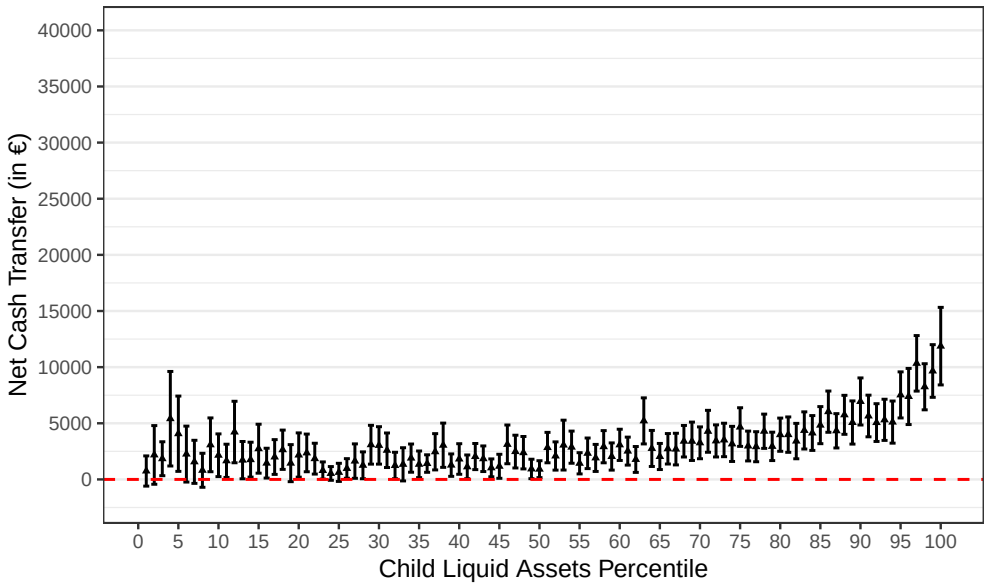
The richness of the administrative data allows me to examine several additional sources of heterogeneity, which are presented in turn. To test these, I extend the baseline dynamic event study as follows:

$$Y_{it} = \alpha_i + \delta_t + \sum_{k \neq -2} \beta_k \cdot D_i \cdot \mathbf{1}\{K_{it} = k\} + \sum_{k \neq -2} \gamma_k \cdot D_i \cdot \mathbf{1}\{K_{it} = k\} \cdot Int_i + \varepsilon_{it}, \quad (3)$$

The additional term, Int_i, represents different binary interaction terms. The new dynamic treatment coefficients, γ_k, indicate how patterns of parental transfers differ between the two groups being compared. Figure 6 presents dynamic treatment effects for the omitted baseline group (β_k) in blue, alongside the total interacted effect ($\beta_k + \gamma_k$) in green. Since the latter represents the sum of two estimated coefficients, standard errors are calculated manually, accounting for the covariance between them.



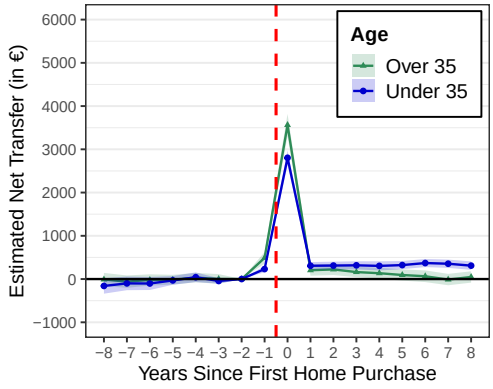
(a) Net Transfer for First-Time Home Purchase: Split by Parental Assets



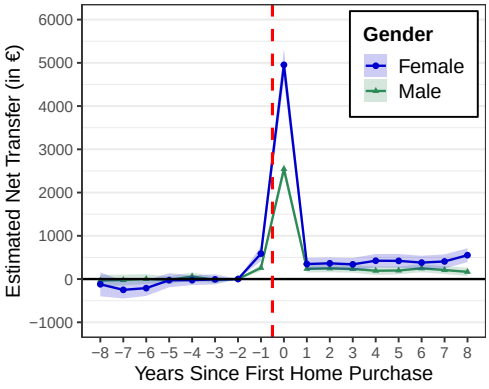
(b) Net Transfer for First-Time Home Purchase: Split by Child Assets

Figure 5: Transfers For Home Purchases Along The Distribution of Liquid Assets

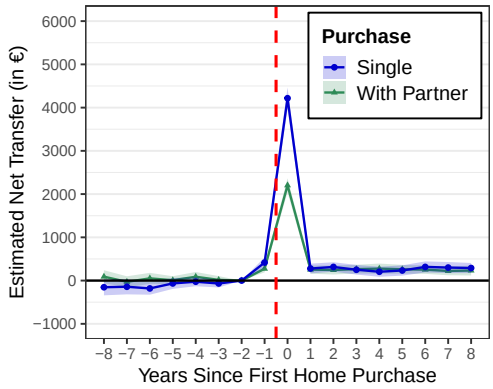
Notes: This figure presents event study coefficients from equation (1), estimated separately within each percentile of the distributions of parent household assets in 2011 (panel (a)) or household assets in 2011 (panel (b)). The life event that is studied is a first-time home purchase. Rather than estimating a full dynamic model, all pre-event years are pooled together, and the plotted treatment effect reports only the spike in transfers in the purchase year ($t = 0$). Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.



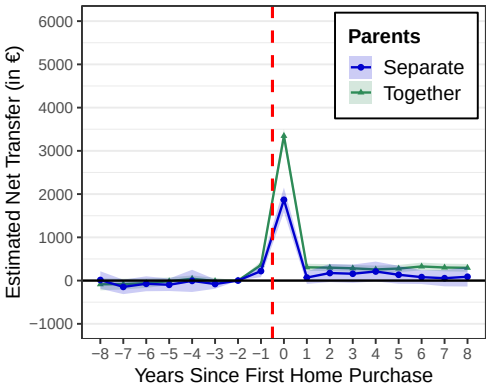
(a) Younger vs. Older Buyers



(b) Male vs. Female Buyers



(c) Single Buyers vs. Couples



(d) Parents Together vs. Separated

Figure 6: Heterogeneity in Net Transfers Around First-Time Home Purchases

Notes: This figure presents event study coefficients estimated from equation (3), with first-time home purchases as life event. The dependent variable is the net transfer (in €) that a person has received in a given year. Years without transfer are coded as 0, rather than NA. The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bands represent 95% confidence intervals.

First, first-time buyers over 35 receive slightly larger transfers than those under 35. Panel (a) of Figure 6 illustrates this pattern. In the transaction year, younger buyers receive an average net transfer of approximately €2,800, compared to €3,500 for older buyers. Most likely, this reflects a tendency of older parents to make more transfers, compared to younger ones. However, older buyers receive no further transfers after the purchase, whereas homeowners under 35 continue to receive approximately €300-400 per year in the subsequent years. A more pronounced discrepancy emerges when comparing buyers by gender. While male household heads receive an average transfer of €2,500 at the time of purchase, female household heads receive twice that amount, approximately €5,000. They continue to receive slightly higher levels of support in the years after the purchase, although these estimated differences are no longer statistically significant. Third, buyers receive substantially larger transfers if they purchase on their own (more than €4,000), compared to purchases that are made with a partner (around €2,000). Finally, parents who do not live together make smaller transfers (around €2,000) than those who cohabit (around €3,500), potentially due to coordination challenges in providing financial support.

4. Home Purchase Transfers Future Wealth Accumulation

So far, I have documented that parents transfer a substantial amount of wealth around their child's first-time home purchase and that transfer recipients tend to be wealthier to begin with. However, given that transfers for first-time home purchases are received at a relatively young age, it is important to understand what their effect is on future life cycle outcomes. Finding that transfer recipients also accumulate more wealth in the future would suggest that the effect of transfers on wealth inequality is even stronger in the longer run. If instead, transfer recipients consume most of what they are given, the longer-run effect of transfers on inequality would be attenuated. Which of these two patterns is found depends crucially on how transfer recipients decide to allocate their funds. This will be investigated in this section.

I will proceed as follows: first, I explain theoretically how *transfer wealth*—the additional net wealth in year t per Euro of parental transfer received in year 0—evolves over time, based on the behavior of the recipient. Then, I discuss the empirical challenges that are involved in estimating the causal effect of parental transfers and introduce my strategy for overcoming these challenges, based on quasi-experimental variation from policy-induced changes to the special tax exemption limit. Finally, I present the empirical results from estimating the future path of transfer wealth. These are based on a comparison of transfer recipients with two different counterfactual groups: the full sample, including individuals who remain renters; and a buyer-only sample, where recipients are compared to other homebuyers who do not receive parental support.

4.1. Decomposing Wealth From Home Purchase Transfers

Consider an individual who receives a parental transfer of size T to support her first-time home purchase. Of that amount, T_h is used to purchase a more valuable home, T_s is kept

in liquid savings and T_m is used to lower the initial mortgage principal. The residual, T_c is immediately consumed. Let Ω_t denote transfer wealth, that is, the additional net wealth at time t that recipients derive for every Euro of parental transfer received at $t = 0$, when a home purchase is made. Transfer wealth at $t = 0$ is equal to:

$$\Omega_0 = T_h + T_s + T_m. \tag{4}$$

Every Euro that is not immediately used for consumption raises net wealth one-to-one, either by increasing assets or lowering mortgage debt. Over time, funds that are initially invested into a more valuable home (T_h) or liquid savings (T_s) yields a constant return of r_h and r_s, respectively.[6] In contrast, how the initial reduction in mortgage debt (T_m) shapes future wealth depends on two additional decisions that recipients must make. First, the decision of whether the smaller principal should be amortized over a shorter period. This in turn determines the reduction in recurring debt service cost, compared to a counterfactual mortgage without reduced principal. Second, the decision of whether reductions in debt service cost are offset by investments into liquid savings, or whether they finance higher consumption.

Formally, let D_0 denote the initial principal on a fully amortizing mortgage absent a transfer, and ${D_0}' = D_0 - T_m$ denote the reduced principal for transfer recipients. To simplify the exposition, mortgage interest rates (r_m) are assumed constant and therefore unaffected by the reduction in principal.[7] The initial term of the mortgage is n, but transfer recipients may choose for a faster amortization schedule, such that $n' \leq n$. Crucially, in choosing n', transfer recipients effectively also pin down their debt service payments $P' \leq P$.[8] Debt service cost and outstanding mortgage debt at time t are given by the standard annuity formulas:

$$P = \frac{r_m D_0}{1 - (1 + r_m)^{-n}} \qquad D_t = (1 + r_m)^t D_0 - P \, \frac{(1 + r_m)^t - 1}{r_m} \tag{5}$$

The first way in which mortgage reductions shape future wealth is determined by how the outstanding mortgage debt of transfer recipient evolves mechanically, relative to the counterfactual debt level without a transfer:

$$\Delta D_t(\Delta n') = D_t - {D_t}' = (1 + r_m)^t \, T_m - \Delta P \, \frac{(1 + r_m)^t - 1}{r_m}, \tag{6}$$

[6] To simplify the exposition, I abstract from uncertainty in future returns.

[7] In practice, reducing the principal will lower mortgage interest rates. Incorporating this would imply that—for a constant term n—ΔP would increase, as debt service cost decrease even more. Therefore, in reality, the reductions in debt service cost that transfer recipients enjoy are even larger than the ones derived here.

[8] If transfer recipients decide to keep debt service payments unchanged ($P' = P$), they will amortize more quickly ($n' < n$). Concretely, the implied term is given by $n' = -\ln(1 - \frac{r_m {D_0}'}{P'})/\ln(1 + r_m)$. In contrast, if they decide to keep the same term ($n' = n$), their debt service cost will be lower ($\Delta P > 0 \iff P' < P$).

where $\Delta D_t = D_t - D'_t$, $\Delta P = P - P'$ and $\Delta n = n - n'$. The first term captures the future value of the initial principal reduction, and the second term captures the future value of reduced debt service cost. Note that $\Delta D_0(n') = T_m$, i.e., the initial gap in mortgage debt is equal to the transfer amount allocated to debt reduction.

Additionally, transfer wealth from mortgage reductions is shaped by the savings decisions that recipients make in every period. Concretely, if recipients pay lower debt service cost, they can use the periodic savings in two ways: to invest in liquid assets, or to finance additional consumption. Assuming that of each Euro that is saved in debt service, a constant fraction $\varphi \in [0, 1]$ is invested every period, additional liquid savings at time t will amount to:

$$\varphi \, \Delta P \, \frac{(1 + r_s)^t - 1}{r_s}. \tag{7}$$

Combining all terms, total transfer wealth at time t equals:

$$\begin{aligned} \Omega_t = {} & (1 + r_h)^t \, T_h + (1 + r_s)^t \, T_s + (1 + r_m)^t \, T_m \\ & - \Delta P \left[\frac{(1 + r_m)^t - 1}{r_m} - \varphi \, \frac{(1 + r_s)^t - 1}{r_s} \right]. \end{aligned} \tag{8}$$

As this decomposition shows, how transfer wealth continues to grow depends on the initial allocation of funds, as well as on the behavioral response of the recipient. The first three terms highlight how parental transfers mechanically promote further wealth accumulation, regardless of how they are used: additional real estate or liquid assets yield increased capital gains, and deleveraging is profitable if mortgage debt is costly. However, the fourth term captures the decline in transfer wealth if recipients lower their monthly debt service cost. Effectively, if first-time buyers without parental support make higher periodic mortgage payments, they close the wealth gap over time by accumulating home equity more quickly. This does not occur in a linear way, but slow at first and faster later on, in line with the way mortgage amortization schedules are structured. Transfer recipients can prevent this in two ways: either by not reducing their debt service cost too much and opting for faster amortization, or by offsetting their lower debt service with liquid savings. If $r_s > r_m$ and φ is sufficiently large, transfer wealth may actually increase, rather than decrease, over time, if recipients lower their mortgage payments.

To highlight the different possibilities, I simulate the mortgage-related components of transfer wealth based on the standard product in the Dutch market: a 30-year, fully amortizing mortgage with monthly payments. Consider two extreme scenarios: on the one hand, transfer recipients may leave the term of their mortgage unchanged—thereby lowering their monthly cost—and fully consume their monthly savings. On the other hand, they may lower the term of their mortgage to the extent that their monthly debt service cost stay unchanged ($\Delta P = 0$). While the mortgage gap is eventually closed in both scenarios, they imply very different wealth accumulation patterns, as visualized in Figure A.7. In the same term scenario without offsetting investments, transfer wealth declines steadily over the course of the mortgage. In the fast amortization scenario,

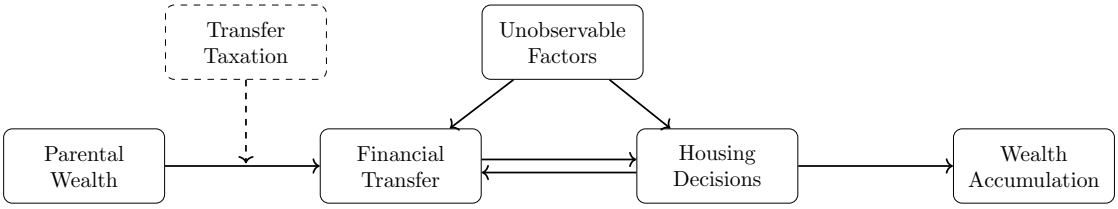


Figure 7: The Causal Relationships Around Transfers, Housing, and Wealth

Notes: This figure visualizes the causal relationships between parental wealth, parental transfers, housing outcomes, and wealth accumulation in a directed acyclic graph (DAG). Arrows indicate the direction of causality. Transfer taxation, visualized in a dashed box, is a moderating factor for the relationship between parental wealth and transfers.

transfer wealth continues to grow, as transfer recipients build up equity at a faster rate. Only after recipients have fully amortized (dashed line) can non-recipients begin to catch up, as they continue to pay off their debt.

4.2. Identifying the Effect of Home Purchase Transfers

In Figure 7, I present a simple directed acyclic graph (DAG) illustrating the causal relationships between parental transfers, housing market decisions, and future wealth accumulation—the main focus of this section. Identifying the effect of parental transfers on housing decisions and subsequent wealth accumulation is challenging for at least two reasons. First, within the extended family, transfer and home purchase decisions are likely to be jointly determined, making it difficult to disentangle the direction of causality. While parents control the supply of financial transfers, children determine their demand for them based on their housing preferences. For example, children who identify a dream home that exceeds their budget may request a (larger) transfer from their parents. Second, even after conditioning on observable characteristics, transfer recipients may differ from other buyers in unobserved ways that also influence their housing market behavior and economic outcomes. For instance, those receiving transfers might be better informed about the housing market, leading them to make systematically different purchase decisions. For these reasons, simple regressions of housing market outcomes on received transfer amounts are unlikely to capture the underlying causal mechanisms.

The supply of financial transfers is ultimately driven by the availability of parental assets that can be converted into cash. Isolating exogenous variation in parental resources would therefore help identify the causal effect of a financial transfer. For instance, Daysal, Lovenheim, and Wasser (2023) study variation in parental wealth due to differences in house price appreciation using a long adminstrative panel. However, this approach is less suitable for the setting I study. In particular, there is likely insufficient variation in parental housing returns to generate meaningful changes in transfer behavior. Moreover, converting housing wealth into financial transfers requires extracting home equity, which is less common in the Netherlands than in other countries (Benetton, Kudlyak, and Mondragon, 2022).

In contrast, the Dutch setting offers variation not in parental resources per se, but in how financial transfers from parents to children are taxed. As shown in Figure 7, the taxation of transfers moderates the relationship between parental wealth and financial transfers. In late 2013, the Dutch government implemented a policy reform that altered the taxation of transfers for housing-related expenditures (e.g., home purchases, mortgage prepayments, and renovations). Prior to the reform, the special exemption limit for housing-related transfers was set at €51,407. In October 2013, as part of a broader effort to stimulate the housing market, the government temporarily increased this one-time exemption limit to €100,000 for transfers earmarked specifically for housing. In practical terms, this implied potential tax savings of approximately €5,000 for parents who fully utilized the exemption, relative to the pre-reform regime. From 2015 onward, the special exemption limit was reduced to its pre-reform level.

During the initial phase-out of the increased exemption, the measure received wide media attention and triggered a surge in tax-free gifts, with around 150,000 recorded transfers. While many of these transfers were used to finance first-time home purchases—the focus of my analysis—the primary policy goal was to help existing homeowners with underwater mortgages following the prolonged housing market downturn after the Global Financial Crisis. Between 2017 and 2023, the policy was reintroduced in a slightly modified form. However, according to Statistics Netherlands, take-up rates during this second policy window remained well below those observed in the initial rollout phase (Statistics Netherlands, 2021).

To study the effect of the initial policy rollout on financial transfers—and, in turn, on housing market outcomes—I make several adjustments to the data set. First, I restrict the sample to the years 2011–2016, surrounding the policy reform. Next, I collapse the original person-by-year panel into a repeated cross-section of potential first-time buyers, retaining one observation per individual. Individuals who make a first-time home purchase between 2011 and 2016 are assigned to the year of their purchase. For all others—those who remain renters throughout this period—I randomly assign a year between 2011 and 2016. This procedure allows me to compare, within each year, housing market outcomes of transfer recipients and non-recipients. Crucially, because this data set originates from the underlying panel data, I can include information on outcomes measured up to 7 years after the observation year. For example, for an individual observed in 2016, I track household wealth information from 2016 (year t) through 2023 (year $t + 7$) in separate columns. Similarly, for a person observed in 2014, the corresponding entries for year t and $t+7$ track her wealth in 2014 and 2021, respectively.

Empirically, I attempt to investigate the causal effect of parental transfers targeted for first-time home purchases—which I will refer to as *home purchase transfers* for brevity—on housing decisions and future wealth accumulation.[9] A straightforward way to estimate the effect of home purchase transfers on housing and wealth outcomes would be

[9] I focus on targeted transfers, since the policy change related specifically to transfers made for housing-related expenditures. Conceptually, this is distinct from studying the effect of untargeted transfers, i.e., pure cash gifts. In a targeted transfer, the causal link between the transfer and the decision to make a home purchase is established mechanically. Therefore, this setting is not suitable to estimate how the probability of making a purchase is increased upon receiving a transfer.

the following:

$$Y_{it}^{t+k} = \delta_t + \beta \cdot Transfer_{it} + \mathbf{X}_{it} + e_{it}, \tag{9}$$

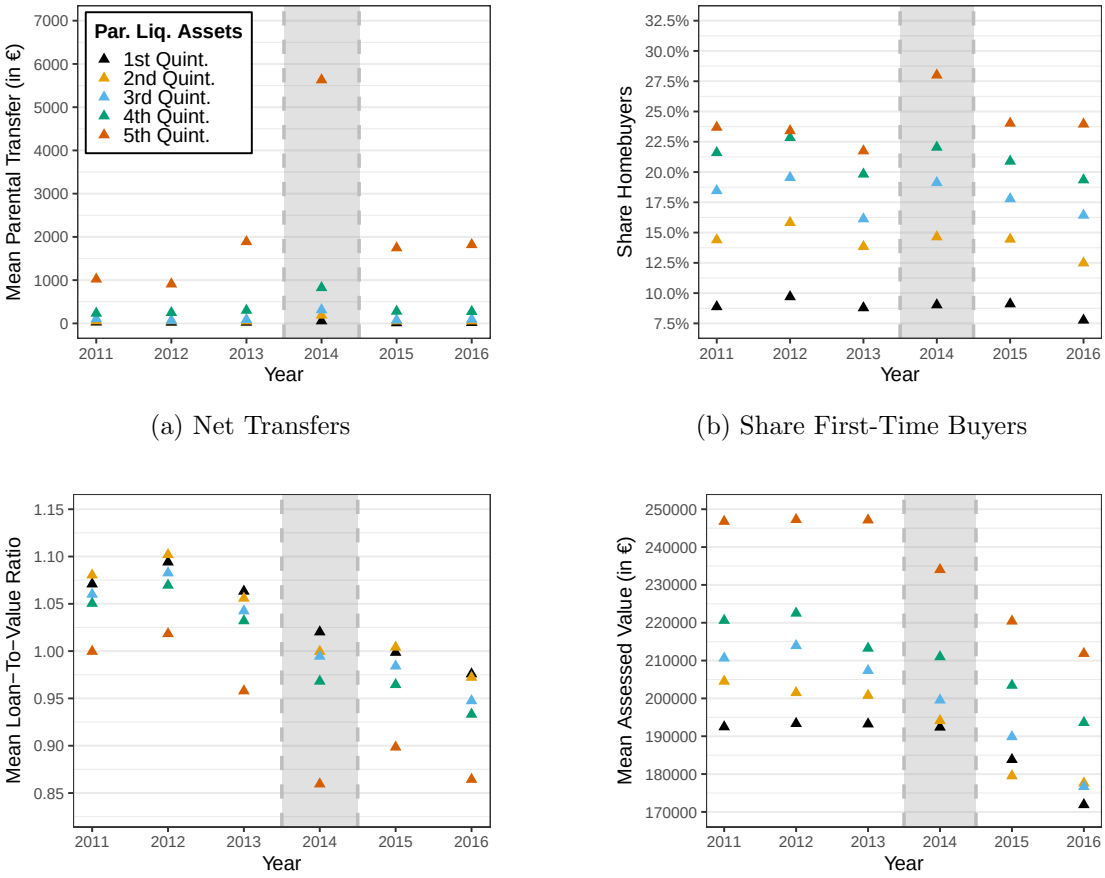
where $Transfer_{it}$ indicates the amount of parental transfer that person i has received in year t, Y_{it}^{t+k} denotes a housing or wealth outcome from year $t+k$, $\mathbf{X}_{it}$ contains a vector of control variables and δ_t is a time fixed effect. However, for the reasons outlined above, this approach suffers from endogeneity and cannot reveal a causal effect. Therefore, my empirical strategy for identifying the effect of transfers on housing market outcomes is based on the relationship between parental wealth and the taxation of parental transfers. The policy change announced in late 2013 creates exogenous variation in this relationship, which I will capture in a two-stage least squares (2SLS) framework. Before introducing the empirical strategy formally, I visualize the underlying intuition in Figure 8.

In 2014, when the exemption limit is raised from around €50,000 to €100,000, there is a substantial increase in parental transfers. However, that is the case only for people with wealthy parents (measured via parental liquid assets in 2011). For people with parents in the top quintile of liquid assets (orange triangles), the unconditional net transfer increases to €6,000 in that year. In 2015, when the exemption limit was reduced again, this number drops back to less than €2,000. In contrast, for those with less affluent parents, transfers respond substantially less to the special tax-exemption policy, and for those in the bottom quintile, they remain virtually flat around zero. This unequal increase in transfers in 2014 captures the first stage in the 2SLS framework. Similarly, the share of individuals making a first-time purchase increases in 2014, relative to other years, but this increase is driven by those with affluent parents.

The last two panels are based on the buyer-only sample and depict the intensive-margin response to the policy. Panel (c) suggests that, among first-time buyers, the policy led to a significant increase in down payments: buyers with wealthy parents enter the housing market with lower loan-to-value (LTV) ratios in 2014, compared to other years.[10] In panel (d), average property values across all first-time buyers are shown. Here, no visible pattern stands out with respect to the impact of the policy. At first glance, this suggests that, at the intensive margin, parental transfers mainly lead to a deleveraging of mortgage debt.

Of course, these visual patterns merely represent unconditional averages and should not be interpreted as causal effects per se. To formally identify the effect of parental transfers on housing outcomes, I estimate a two-stage least squares (2SLS) model. In this framework, the receipt of a parental home purchase transfer is instrumented using an interaction between parental liquid assets (measured in 2011) and an indicator for the year 2014, when the exemption policy was in effect. This interaction captures plausibly

[10] Strikingly, in the early years of the sample, LTV ratios reach values of up to 110%. To some extent, this reflects imprecise measurement, as I calculate LTV ratios based on tax assessed values, rather than the unobserved market values that mortgage lenders use. However, the statutory LTV limit in 2012 was as high as 106%. In the following years until 2018, that limit was reduced by 1% per year.



(a) Net Transfers

(b) Share First-Time Buyers

(c) LTV Ratio At Purchase

(d) Property Value At Purchase

Figure 8: Housing Market Outcomes Around The Policy Reform

Notes: This figure shows transfers and housing outcomes between 2011 and 2016, depending on parental liquid assets (measured in 2011). Panels (a) and (b) are based on the full sample, panels (c) and (d) consider first-time homebuyers only. Non-buyers are randomly assigned to a year, based on a uniform draw between 2011 and 2016. The year 2014, highlighted in gray, highlights the period in which the special tax exemption for housing-related transfers was raised to €100,000.

exogenous variation in transfer receipts stemming from the policy's differential impact across the parental wealth distribution. I estimate the following model:

$$Transfer_{it} = \delta_t + \pi \cdot \Big(LiquidAssets^{Par}_{i,2011} \times \mathbf{1}\{t = 2014\} \Big) + \mathbf{X}_{it} + u_{it} \tag{10}$$

$$Y^{t+k}_{it} = \delta_t + \beta \cdot \widehat{Transfer}_{it} + \mathbf{X}_{it} + \varepsilon_{it}, \tag{11}$$

where I instrument for the net financial transfer that person i receives in year t using an interaction between her parents' liquid assets, measured in 2011, and an indicator for the year 2014. In the second stage, predicted transfers are linked to various housing decisions and, in the next step, to downstream wealth outcomes. In both models, I include for

granular set of controls and fixed effects, all measured in 2011: year-by-residential-location (NUTS3 region) fixed effects; a single-household dummy; gender; highest degree; birth year. Moreover I control for the following wealth and income variables, measured in 2011 and captured in percentile bins: household income, household liquid assets, parental household liquid assets and household net wealth. Since I control for parent household liquid assets, the variation is driven by how people with wealthy parents behave differently during the policy period, relative to other years.

The relevance of the constructed instrument is supported by the visual evidence in Figure 8 and will be formally confirmed in the first-stage regressions. In contrast, the exclusion restriction cannot be tested directly. In this context, it requires that the housing decisions—and subsequent wealth outcomes—of children with wealthy parents observed in 2014 differ from those of others only because they received larger parental transfers. Given the salience of the policy change at the time, it is plausible to assume that any increase in parental support is captured by a higher propensity to make cash transfers, rather than by other forms of assistance such as co-signing mortgage applications.

Nonetheless, the exclusion restriction could be violated if other concurrent policy changes or market trends affected wealthy parents' children differently from others observed in the same or nearby years. For example, in 2013, the Dutch mortgage interest deduction rules were reformed (Bernstein and Koudijs, 2024), and throughout the sample period, lending limits were gradually tightened (Rouwendal and Petrat, 2022). However, there is little reason to believe that these changes uniquely affected children of wealthy parents in 2014. A key strength of the design is that the tax exemption reform was temporary and centered in the middle of the sample period. As a result, only those with wealthy parents in 2014 are "treated," while those with equally wealthy parents observed before or after serve as a control group. This feature mitigates concerns about linear time trends that might otherwise affect the relative outcomes of children from wealthier versus less wealthy families.

However, a remaining concern is that due to the policy, buyers may select into the year 2014, in order to make use of the higher exemption limit. This may introduce selection bias for the intensive margin analysis, as it involves conditioning on a variable (making a home purchase) that is itself affected by the treatment (receiving a parental transfer). Concretely, selection bias arises if the marginal homebuyer that is induced to buy during the policy period differs from other homebuyers in ways that are related to the outcomes of interest. In that case, the estimated effects do not only capture the local average treatment effect (LATE) of parental transfers on the behavior of homebuyers, but also reflect compositional changes in the sample of buyers. To address this, I implement inverse probability weighting, whereby the probability of making a purchase during the policy period (rather than in another year) is estimated on all observable characteristics (Wooldridge, 2002; Carry et al., 2021).[11]

[11] Concretely, I estimate probabilities $\hat{p}$ from a logistic regression of the probability of making a purchase in 2014, based on all covariates and fixed effects listed above, excluding the year FE. Next, weights are obtained by calculating $w = Pr(year = 2014)/\hat{p}$ for all those who buy in 2014 and $w = Pr(year \neq 2014)/(1 - \hat{p})$ for those who buy in another year, whereby $Pr(year = 2014)$ is the actual share of buyers making their purchase in 2014.

Table 2: Transfers and Housing Outcomes

	Ext. Margin	*Int. Margin*			*Mortgage Outcomes*	
	(1) Purchase (in %)	(2) Home Value	(3) Mortgage Size	(4) Δ Liq. Assets	(5) Interest Rate	(6) Repay. Rate
Panel A: OLS						
Net Transfer (€)	0.0008***	0.050	−0.700***	0.035***		
	(0.0001)	(0.040)	(0.039)	(0.002)		
Transfer (Y/N)					0.000	0.005**
					(0.000)	(0.003)
Observations	288,084	26,504	28,087	28,087	27,265	27,658
Adjusted R^2	0.200	0.467	0.444	0.350	0.376	0.082
Panel B: IV						
Net Transfer (€)	0.0009***	−0.100	−1.004***	−0.044		
	(0.0001)	(0.194)	(0.172)	(0.070)		
Transfer (Y/N)					−0.006**	0.031
					(0.003)	(0.025)
Observations	288,084	26,504	28,087	28,087	27,265	27,658
Adjusted R^2	0.200	0.467	0.442	0.349	0.367	0.076
First Stage F-Stat	6,804	1,339	1,393	1,393	345	344

Notes: This table shows the OLS and IV estimates of the effect of receiving a parental transfer on various housing market outcomes. IV estimates are based on the 2SLS model specified in (11). Model (1) is estimated on the full sample, including non-buyers. Models (2) - (6) are estimated on the buyer-only sample. All models include the following controls as categorical variables: residential location × transaction year; gender; single household dummy; birth year; highest degree; household income percentile; household liquid assets percentile; household net wealth and parental household liquid assets percentile (all measured in 2011). Heteroskedasticity-robust standard errors are reported in parenthesis. Stars indicate the 10% (*), 5% (**) and 1% (***) significance level, respectively.

4.3. Transfers and Housing Market Outcomes

To examine how parental home purchase transfers are used, I estimate the 2SLS model from equation (11). The resulting coefficients are reported in Table 2. These estimates are compared with results from an OLS model including similar control variables, in which each outcome variable is regressed on the net parental transfer received by the individual in a given year.

First, I estimate the extensive-margin effect of parental transfers by examining how the probability of making a first-time home purchase changes with each additional euro of transfer received. This estimation is based on the full sample, including individuals who do not make a purchase between 2011 and 2016. The dependent variable is an indicator equal to one if a first-time purchase occurs in a given year. Among non-recipients, the mean annual probability of making a first-time purchase is 16.6%.

According to the IV estimates, this probability increases by about 0.9 percentage points for every €1,000 in parental transfers received. However, as discussed above, this relationship is mechanical, as the policy supported only transfers that were earmarket for housing. The extensive-margin effect is therefore reported primarily for completeness and to illustrate the empirical relevance of the policy change.

Next, I turn to the intensive-margin response to a transfer. All outcome variables are measured in euros, allowing a straightforward interpretation of treatment effects: *for each euro received in transfers, how many cents are allocated to purchasing a more valuable home, reducing the mortgage balance, and increasing liquid savings?* Thus, the estimated effects can be directly mapped to the channels introduced in the previous section. Investments in additional real estate (T_h) are captured by the tax assessed value of a purchased property, observed in the year prior to the transaction. Investments in mortgage reduction (T_m) are measured at the household level as the outstanding balance at the beginning of the following year. Finally, liquid savings (T_s) are measured as the change in liquid assets (bank deposits plus stocks and bonds) from the purchase year to the subsequent year. The resulting coefficients are reported in columns (2) to (4) of Table 2.

The results indicate that the intensive-margin response to parental transfers is driven entirely by the mortgage channel. The IV estimates suggest that every Euro of parental transfer leads to a one-for-one reduction in mortgage principal. This compares to a reduction of 70 cents per euro based on the OLS model. In other words, transfer recipients appear to use the entirety of the funds to deleverage. In contrast, I find no evidence that transfer receipts are spent on buying more valuable homes or increasing liquid savings: the effect of transfers on assessed values is insignificant in both models. For liquid assets, only the OLS model suggests a significant but small increase of 3.5 cents per euro or transfer.

In columns (5) and (6), I report two additional outcomes that provide further evidence on the mortgage channel. In both cases, the parental transfer is measured as an indicator variable rather than by amount, to ease interpretation. First, I examine mortgage interest rates. Since rates are not directly observed, I impute them by dividing total mortgage interest payments in year $t+1$ by the mortgage principal outstanding at the beginning of that year. While the OLS estimates show no difference, the IV results indicate that transfer recipients pay approximately 0.6 percentage points lower interest rates on their mortgages. Hence, their debt service costs are reduced not only because they take on less debt, but also because the cost of that debt is lower.

Next, I study the amortization schedule chosen by transfer recipients. Because the data are right-censored, I cannot observe the full mortgage term. I therefore proxy for the repayment rate as the reduction in mortgage principal during the first year after purchase divided by the initial principal.[12] The OLS estimates suggest higher repayment rates among transfer recipients, but the 2SLS estimates are statistically insignificant. Thus,

[12] Most Dutch mortgages during this period were either fully amortizing or included an interest-only component, whereas linear mortgages were uncommon. Consequently, repayment rates are not constant over the life of the mortgage.

it cannot be rejected that recipients retain similar mortgage terms rather than opting for faster amortization.

4.4. Transfers and Future Wealth Accumulation

I now examine how this allocation of funds translates into subsequent patterns of wealth accumulation. To this end, I re-estimate the 2SLS model from (11), using household net wealth in year $t + k$ after the purchase as the outcome variable, where k ranges from 0 to 7. This allows me to track the evolution of transfer wealth over the seven years following the purchase. I also estimate future levels of real estate assets, liquid assets, and mortgage debt. This allows for an empirical decomposition of wealth changes into its different components. Since the (instrumented) treatment variable in each model is the size of the parental home purchase transfer, the coefficients are readily interpretable as the change in wealth in year $t + k$ per Euro of transfer received in year t.

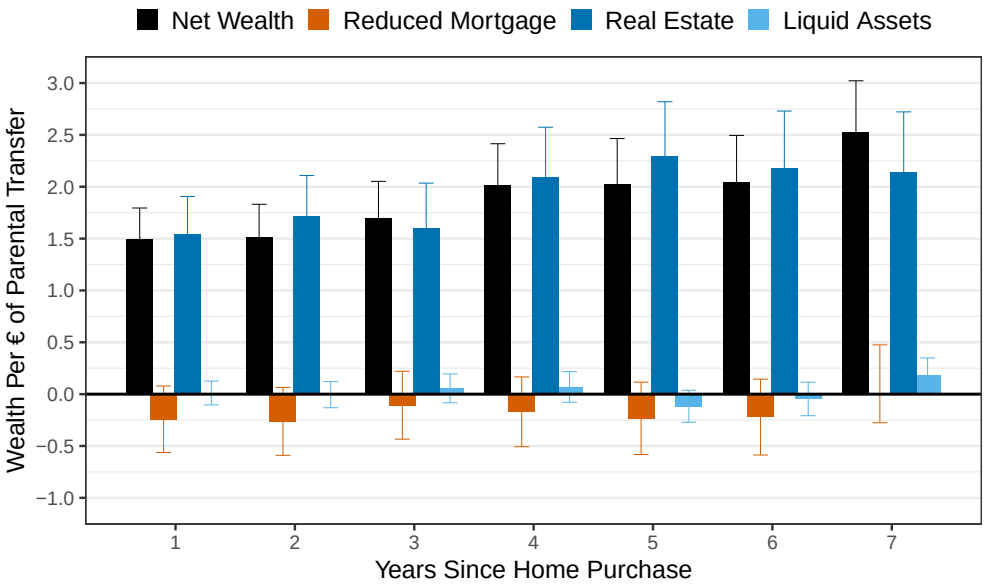
The analysis is done separately for two different counterfactual groups. On the one hand, the full sample of not-yet-homeowners observed between 2011 and 2016, including those who remain renters. On the other hand, the smaller sample of first-time buyers only. Making this distinction is important because, due to the nature of quasi-experiment that I examine, one important counterfactual outcome is unobserved: whether of not transfer recipients would have bought a home absent the transfer or not.[13] If they would have bought a home anyway, the relevant counterfactual group consists of other first-time buyers in the same year. Only if they would have remained renters otherwise is the relevant comparison that with the full sample including never-buyers. I therefore present the empirical results for both counterfactual groups, before discussing which one is likely more appropriate.

Figure 9 presents the second-stage results, using various forward-looking wealth measures as dependent variables. As expected, relative to the full sample that includes non-buyers, recipients of a home purchase transfer exhibit higher net wealth in the following year, driven entirely by an increase in real estate assets. Over time, this wealth advantage continues to grow as the gap in real estate holdings widens. By year seven, the estimated wealth difference between transfer recipients and non-recipients reaches approximately €2.50 for every euro of initial parental transfer.

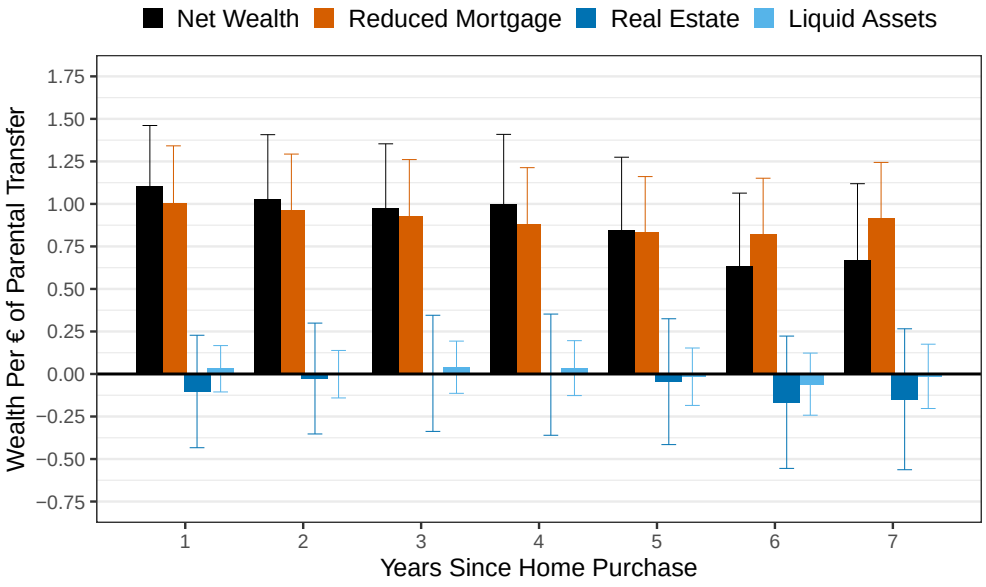
While this represents a substantial increase, it should be noted that during the sample period the Dutch housing market recovered from its 2013 low to a record high in 2022. Consequently, it is unsurprising that, relative to a sample largely composed of non-buyers, individuals who purchase a home with parental support experience substantial housing gains. In contrast, there are no differences in mortgage debt, consistent with the earlier finding that transfers lead to a one-for-one reduction in mortgage principal.

Comparing these results to a simple OLS model, shown in Figure A.8, highlights a key limitation of OLS in this context: because transfer recipients mechanically have higher mortgage debt than non-buyers, OLS would misleadingly suggest that transfers causally increase mortgage borrowing, when in reality they do not. Finally, estimated differences

[13] This is due to the fact that the policy change incentivized parents to make targeted home purchase transfers, rather than unconditional cash gifts.



(a) Full Sample



(b) Buyer-Only Sample

Figure 9: Transfers And Wealth Accumulation (IV Estimates)

Notes: This figure shows the estimated evolution of *transfer wealth*, i.e., the gain in net wealth per Euro of parental home purchase transfer, in the first seven years after the purchase. Total transfer wealth is decomposed into three components: reduced mortgage debt, increased real estate assets and increased liquid savings. Panel (a) is based on the full sample including non-buyers, whereas panel (b) is estimated on a buyer-only sample. All coefficients are based on 2SLS estimates from equation (11). Error bars represent 95% confidence intervals, based on heteroskedasticity-robust standard errors.

in liquid assets remain flat, indicating that transfer wealth is fully invested in real estate, with no evidence of subsequent investment in other assets.

Turning to the buyer-only analysis, shown in panel (b), a different picture emerges. In the first year after the purchase, the estimated wealth differences mirror the intensive-margin results from Table 2: compared to other homebuyers, parental transfers increase recipients' net wealth one-for-one, and this is exclusively driven by a reduction in mortgage borrowing. However, in the following years, the initial gap in net wealth narrows gradually. By year seven, the estimated gap in mortgage debt per Euro of transfer received falls to approximately €0.85. This suggests that homebuyers without parental support close the initial mortgage gap by amortizing faster. In Figure A.9, I illustrate this by estimating the reduction in total debt service cost (interest expenses plus principal repayments) per Euro of transfer received, using the 2SLS model. Transfer recipients enjoy lower debt service cost, both due to lower interest expenses and smaller principal repayments.[14] In other words, by making larger principal repayments, first-time buyers without parental support slowly close the gap in outstanding mortgage debt, relative to transfer recipients.

Crucially, I find no evidence that the reduction in debt service costs is used to accumulate other savings. Differences in real estate assets remain near zero across all years, indicating that transfer recipients are not more likely to make a secondary purchase or upgrade to a more valuable home over time. Likewise, differences in liquid assets remain virtually flat, suggesting that the reduced debt service is not used to invest in securities or increase bank balances. In terms of the transfer wealth decomposition introduced above, this implies that φ is close to zero. Conversely, the marginal propensity to consume (MPC) out of reductions in debt service costs is close to one. This finding aligns with Ganong and Noel (2020), who argue that a reduction in mortgage debt only affects consumption if it is accompanied by a reduction in monthly debt service payments. I therefore conclude that recipients use their increased residual income—net of debt service costs—to finance higher levels of consumption.

For comparison, results from the OLS model are reported in panel (b) of Figure A.8. Unsurprisingly, the OLS estimates show a more muted pattern, reflecting the model's smaller predicted effect on initial mortgage balances. Qualitatively, however, the OLS results similarly suggest that the initial mortgage debt gap closes over time—albeit much more slowly—while no compensating investment in liquid savings occurs.

Taken together, the findings therefore imply that, when compared with other first-time buyers, parental transfers primarily frontload wealth. Initially, recipients use the additional funds to reduce their liabilities rather than to make additional investments or finance consumption. Over time, however, consumption increases as recipients draw on the additional disposable income made available through lower debt service costs.

[14] While interest expenses are reported directly from the income data, principal repayments are imputed based on changes in mortgage debt between year t and $t+1$. Therefore, the estimates are less precise

4.5. Discussion of the Findings

A primary goal of this empirical exercise was to better understand how parental transfers contribute to wealth inequality in the longer run. If transfer wealth decreases over time, this would imply that the contribution of transfers to longer-run wealth inequality may be lower than previously anticipated. If, instead, transfer wealth increases because recipients accumulate additional assets in the future, transfers may amplify wealth inequality even more in the longer run.

As the empirical analysis has shown, which of these scenarios applies comes down to the question of what counterfactual is the relevant one. This in turn depends on when—if at all—transfer recipients would have bought their first home in the absence of parental support. Consider both extremes: if recipients would have otherwise never made a home purchase, the extensive margin effect of a transfer is dominant and recipients should be compared to a full sample including non-buyers. If, instead, transfers do not affect the timing of the purchase, the extensive margin effect is negligible. The truth likely lies between these extremes. It seems plausible that most aspiring homeowners eventually purchase a home, particularly those with wealthy parents. More concretely, it is unlikely that parental transfers lead individuals to become homeowners many years earlier than they would have otherwise. For instance, Engelhardt and Mayer (1998) find that recipients of parental transfers purchase their first home only nine months earlier than comparable individuals without financial support. Consequently, I view the intensive-margin effect, reported in Figure 9, as the main empirical result of this analysis. However, in countries where starter homes are particularly unaffordable or loan-to-value limits are stricter, the extensive-margin effect may be relatively more important.

This implies that, at least in practice, cash transfers for home purchases are ineffective as a vehicle for dynastic wealth building. Instead, their main effect seems to be to frontload wealth, thereby allowing higher consumption earlier in the life cycle. These findings contrast with those by Wold et al. (2024), who argue that housing is a key channel for intergenerational wealth persistence. The key difference is related to leverage: Wold et al. (2024) find that homebuyers with wealthier parents take on more leverage, likely due to the fact that additional collateral or parental guarantees relax borrowing constraints. In contrast, I show that the direct effect of parental cash transfers is to reduce leverage.

One remaining question, beyond the scope of this empirical analysis, is whether the observed pattern of wealth frontloading is intentional. On the one hand, the decision to make a transfer and allocate it toward mortgage principal reduction may be part of a strategy to smooth consumption over the child's life cycle. By reducing debt service costs, investing a transfer in this way increases residual monthly income available for consumption at a time when liquidity constraints likely still bind for many young homeowners.

On the other hand, the observed wealth accumulation patterns could reflect the fact that most people accumulate wealth passively, with mortgage amortization serving as a "default" mechanism (Thaler and Sunstein, 2021). Bernstein and Koudijs (2024) document this mechanism explicitly in the Dutch housing market context: as mortgages

amortize faster, individuals accumulate more wealth because the additional monthly investment into home equity is not offset by reductions in other forms of savings. The narrowing of wealth differences between transfer recipients and other homebuyers observed here may reflect the same phenomenon. In this case, transfer recipients unconsciously adapt their spending patterns to the lower monthly debt service costs, while non-recipients reduce spending to meet their higher amortization targets.

5. Conclusion

Using rich administrative data from the Netherlands, I study parental inter vivos transfers from two perspectives, examining both their drivers and their consequences. Compared with other major life events, first-time home purchases clearly stand out as the dominant driver of parental cash transfers. In the year of the first-time purchase, the mean likelihood of receiving a registered parental transfer increases by six percentage points—a tenfold increase relative to individuals who experience no life event. On average, first-time homebuyers receive €3,200 in additional parental transfers in the year of the purchase, followed by annual transfers of around €300–€400 over the subsequent eight years.

Transfers are strongly concentrated among parents with high levels of liquid assets. While first-time buyers receive around €1,000 in parental support at the median, those with parents at the 95th and 99th percentiles of liquid assets receive €10,000 and €30,000, respectively. Similarly, children who are wealthier to begin with receive higher levels of parental transfers, suggesting that these transfers increase absolute wealth inequality in the short run. In contrast, parents do not make transfers following an involuntary job loss, although some transfers occur around marriage. Regarding the birth of the first grandchild, there is evidence that parents provide informal monetary support by covering some of their child's expenditures.

Motivated by the dominance of transfers around first-time home purchases, I next examine how receiving a parental home purchase transfer shapes housing market decisions and, in turn, downstream wealth accumulation. To do so, I leverage quasi-random variation from a Dutch policy that exogenously increased the tax exemption limit for housing-related transfers. Using an IV design based on parental liquid wealth prior to the policy change, I find that, compared with other homebuyers without parental support, recipients use the windfall from a parental transfer in only one way: to reduce their initial mortgage principal.

Transfer recipients do not appear to purchase more valuable properties or increase their liquid savings. This generates wealth frontloading: in the years following the purchase, the net wealth gap between transfer recipients and other homebuyers narrows. This occurs because transfer recipients, benefiting from lower debt service costs due to their reduced mortgage principal, do not invest more in liquid assets. In other words, while transfer recipients enter homeownership with lower liabilities, non-recipients gradually close the gap by amortizing their mortgages at a faster rate.

These findings have several important implications. First, the spike in parental trans-

fers around first-time home purchases underscores the need to better understand their underlying drivers. Rather than providing insurance against negative income shocks, parents appear primarily motivated to help their children overcome liquidity constraints when investing in homeownership. At the same time, the fact that transfers are used entirely to reduce mortgage debt suggests that parents may aim to facilitate consumption smoothing, rather than dynastic wealth accumulation. An alternative explanation is that, due to bounded rationality, parents (and their children) fail to fully account for mortgage amortization as a wealth-building mechanism.

Overall, the findings of this study add nuance to the discussion of parental transfers and wealth inequality. On the one hand, because transfers are received primarily by children who are already relatively well-off, they clearly increase short-run inequality. On the other hand, wealth frontloading suggests that the long-run impact of inter vivos transfers on inequality within the recipient generation may be more muted.

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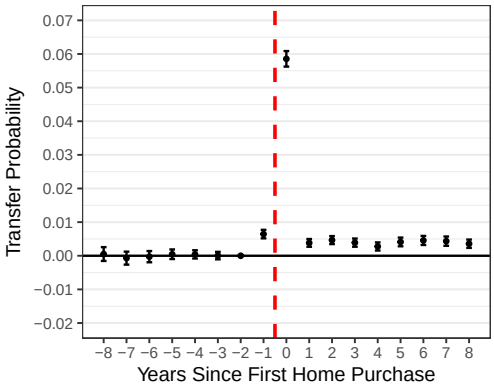
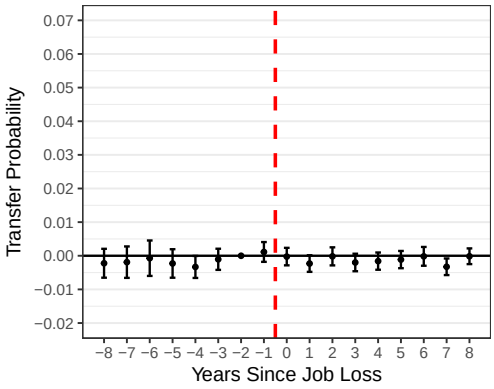
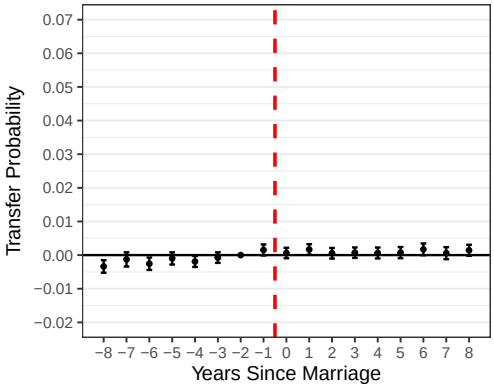
A. Additional Figures



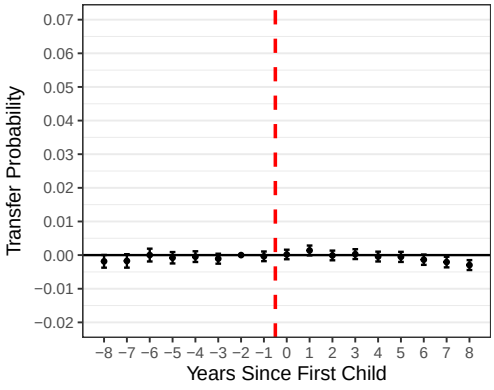
(a) First-Time Home Purchase



(b) Job Loss



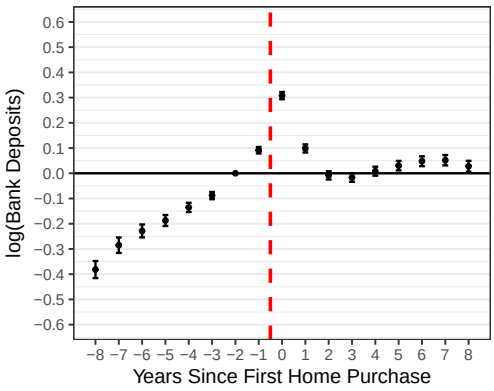
(c) Marriage



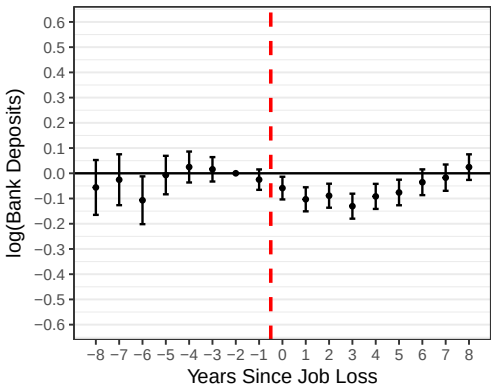
(d) Birth of Child

Figure A.1: Parental Transfers Around Major Life Events (Combined Model)

Notes: This figure presents event study coefficients estimated from equation (2), where all four major life events are included in the same model. The dependent variable is a binary indicator equal to one if a parental transfer was received in a given year. The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.



(a) First-Time Home Purchase



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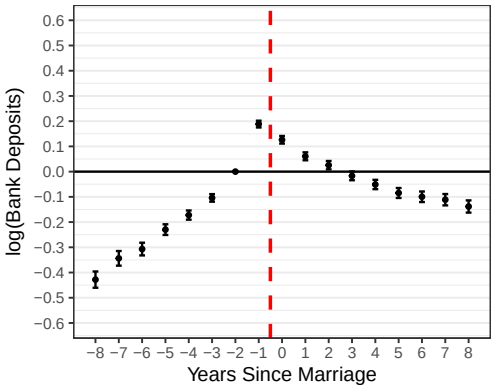

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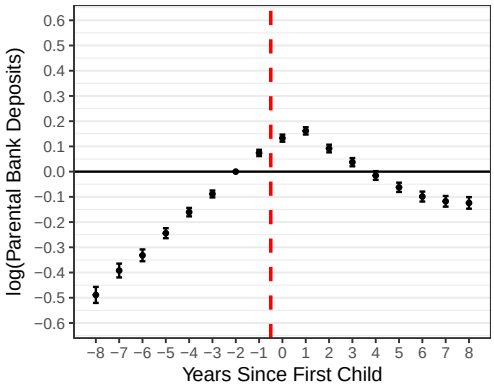
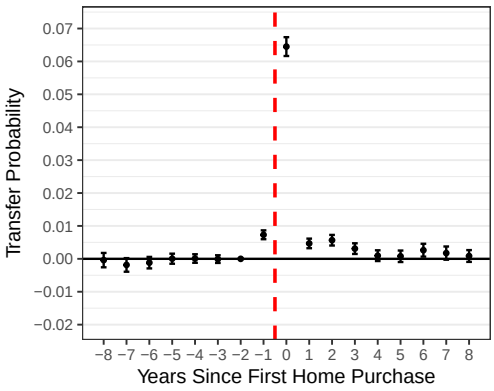


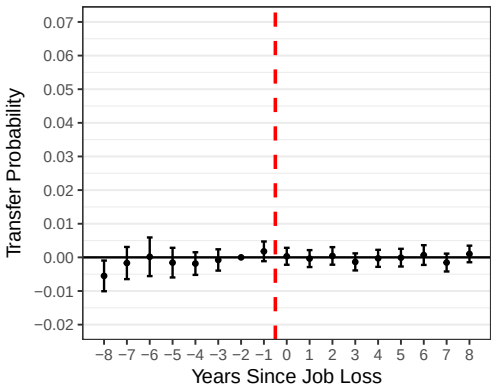
(d) Birth of Child

Figure A.2: Household Bank Deposits Around Major Life Events

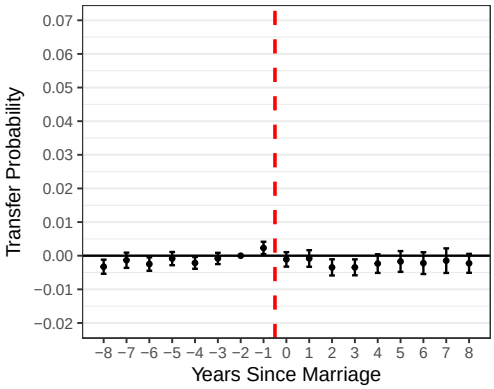
Notes: This figure presents event study coefficients estimated from equation (2), where all four major life events are modeled separately. The dependent variable is the natural logarithm of household bank deposits in a given year. The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.



(a) First-Time Home Purchase



(b) Job Loss



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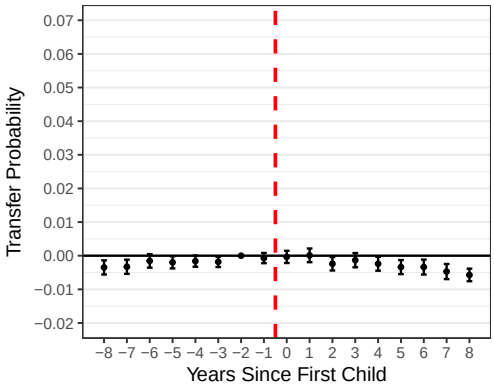


(d) Birth of Child

Figure A.3: Parental Transfers Around Major Life Events: Event Studies Removing Observations After Second Event

Notes: This figure presents event study coefficients, where all four major life events are included in a single model. Here, in comparison to equation (2), observations are removed as soon as a second event occurs. This aims at isolating the effect of the first life event only. The dependent variable is a binary indicator equal to one if a parental transfer was received in a given year. The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.

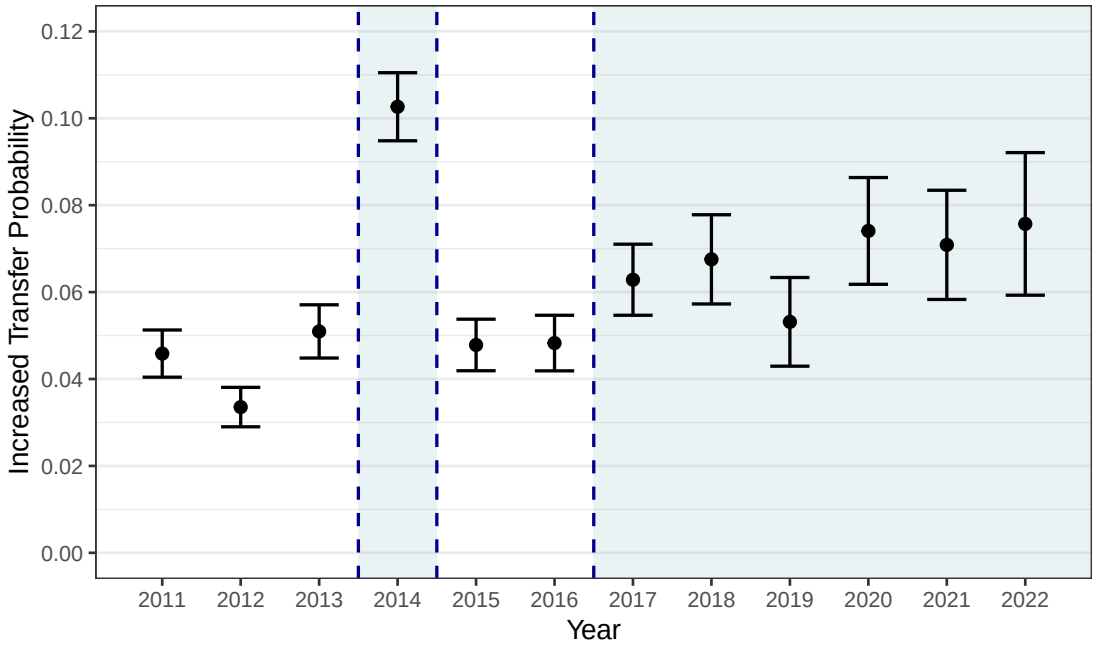
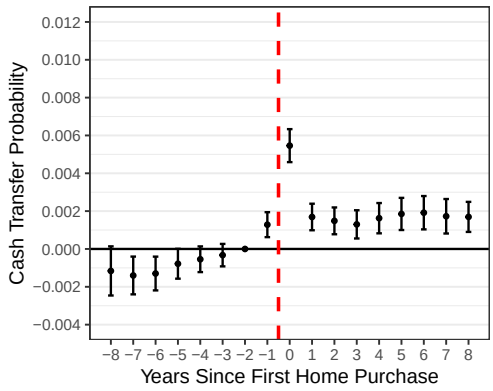
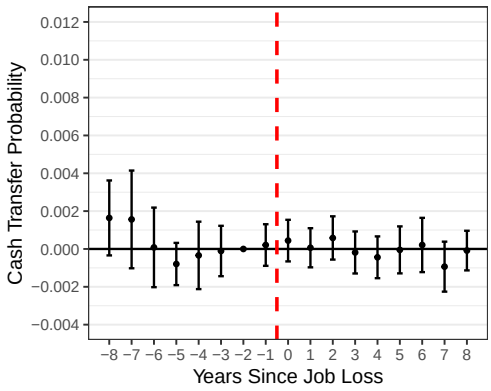


Figure A.4: Home Purchase Transfers Across Years

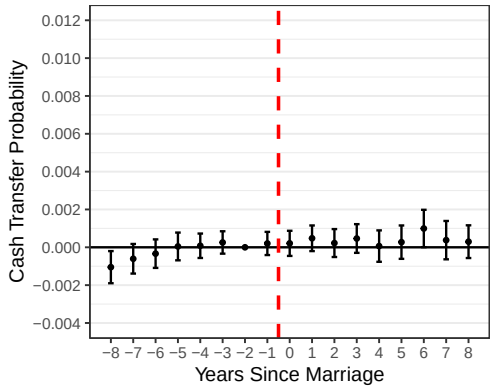
Notes: This figure presents estimates from a model in which differences in parental transfers between homebuyers and others are compared across years. Years in which the special tax exemption threshold for housing-related transfers was raised are shaded in blue. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.



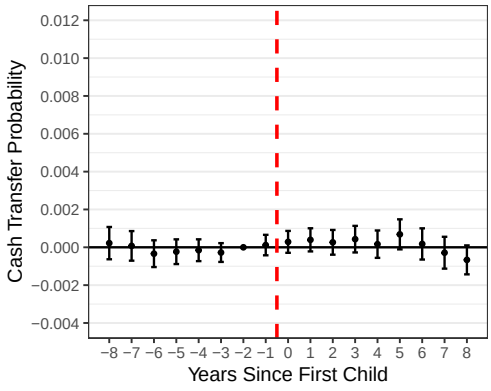
(a) First-Time Home Purchase



(b) Job Loss



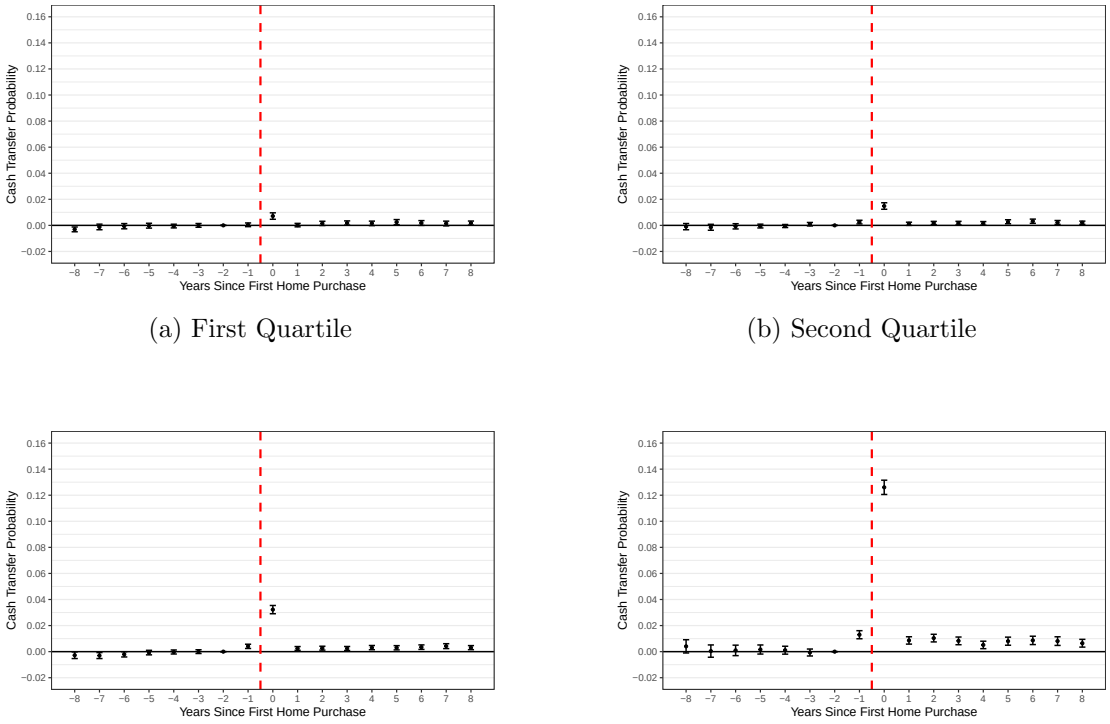
(c) Marriage



(d) Birth of Child

Figure A.5: Regular (Non-Tax-Exempt) Transfers Around Major Life Events

Notes: This figure presents event study coefficients estimated from equation (2), where all four major life events are included in the same model. The dependent variable is a binary indicator equal to one if a regular (taxed) parental transfer was received in a given year. The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.



(a) First Quartile

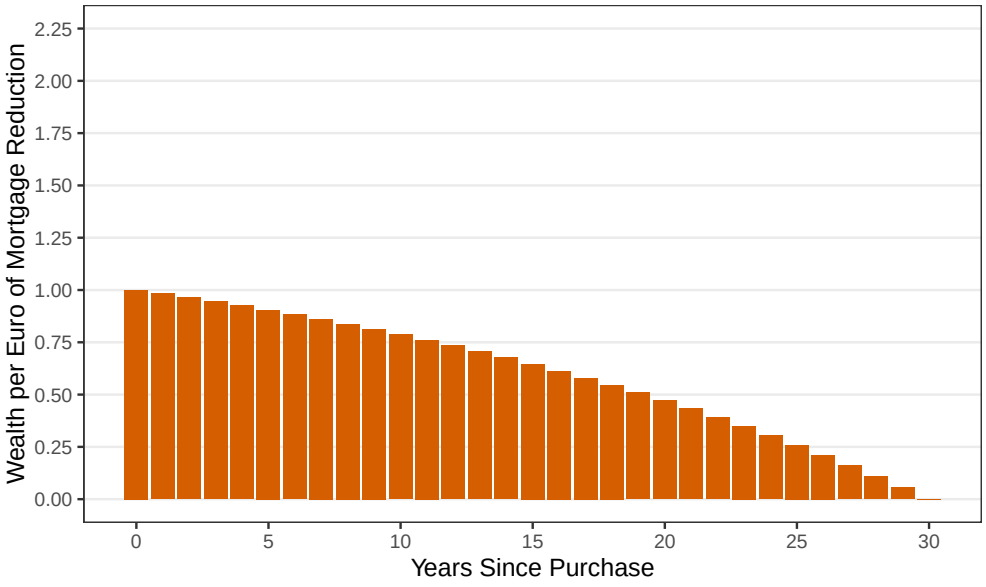
(b) Second Quartile

(c) Third Quartile

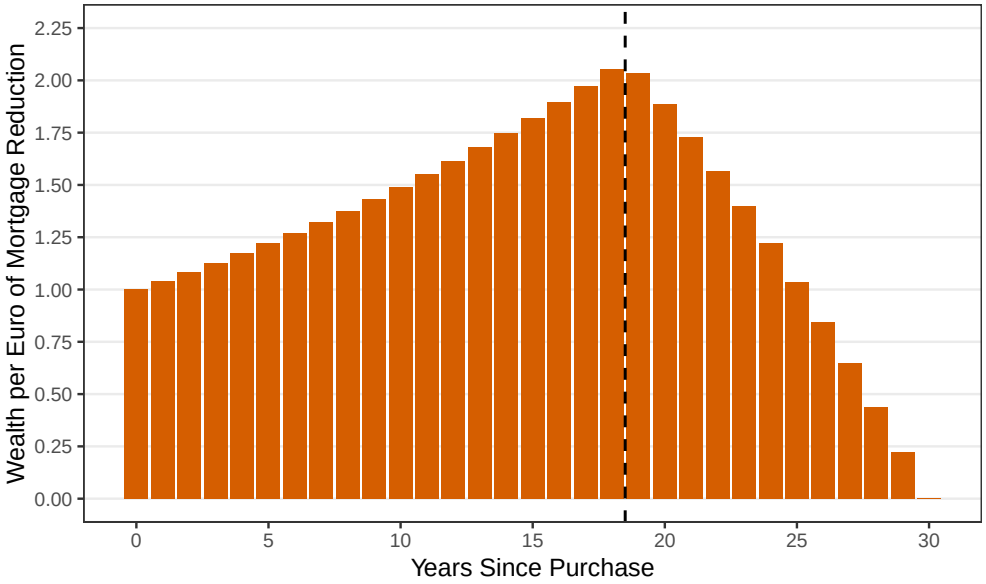
(d) Fourth Quartile

Figure A.6: Transfers Around First-Time Home Purchase (by Parental Assets Quartile)

Notes: This figure presents event study coefficients estimated from equation (1), using first-time home purchases as life event. The dependent variable is a binary indicator equal to one if a regular (taxed) parental transfer was received in a given year. In the four different panels, the model is estimated separately in different quartiles of the distribution of parental liquid assets (measured in 2011). The omitted period is $t-2$, corresponding to two years before the event occurs. Standard errors are clustered at the individual level, and the error bars represent 95% confidence intervals.



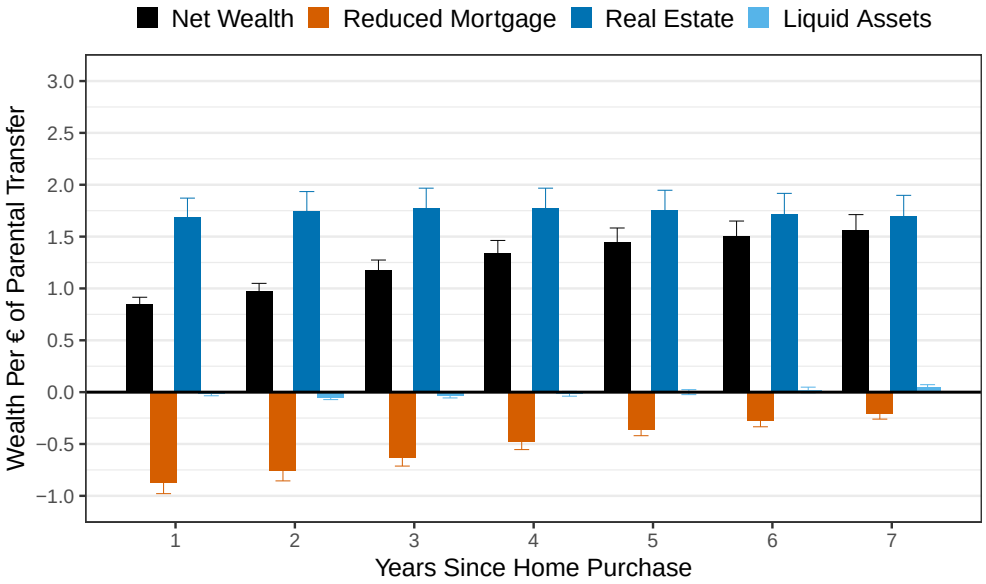
(a) Same-Term Scenario



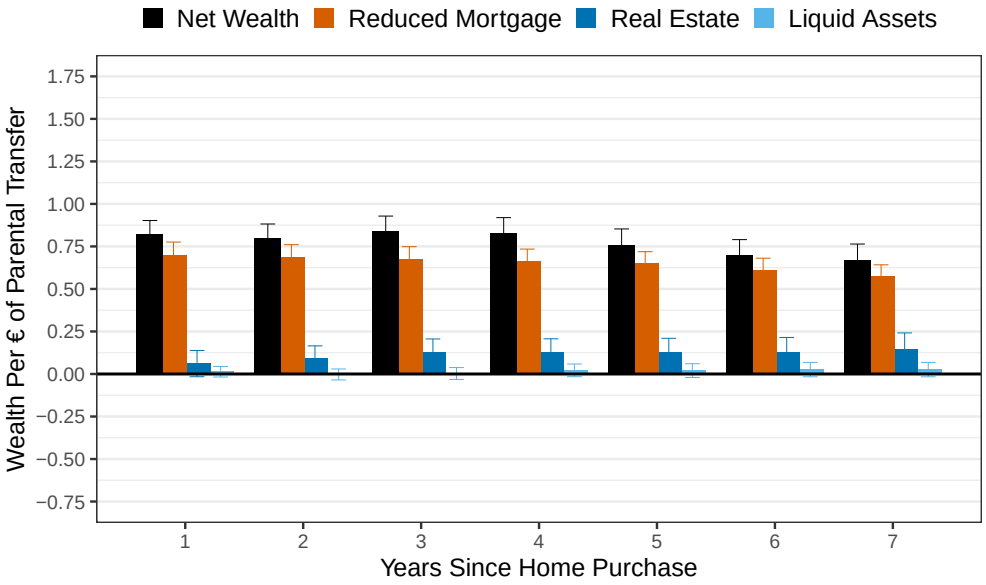
(b) Fast Amortization Scenario

Figure A.7: Simulated Transfer Wealth From Reduced Mortgage

Notes: This figure shows a simulation of the mortgage component of transfer wealth, based on two hypothetical scenarios: First, a same term scenario, whereby transfer recipients do not reduce the term of their mortgage and do not reinvest reduced mortgage payments. Second, a fast amortization scenario, whereby transfer recipients reduce the term of their mortgage to the extent that their debt service cost remain unchanged.



(a) Full Sample



(b) Buyer-Only Sample

Figure A.8: Transfers And Wealth Accumulation (OLS Estimates)

Notes: This figure shows the estimated evolution of *transfer wealth*, i.e., the gain in net wealth per Euro of parental home purchase transfer, in the first seven years after the purchase. Total transfer wealth is decomposed into three components: reduced mortgage debt, increased real estate assets and increased liquid savings. Panel (a) is based on the full sample including non-buyers, whereas panel (b) is estimated on a buyer-only sample. All coefficients are based on OLS estimates from equation (9). Error bars represent 95% confidence intervals, based on heteroskedasticity-robust standard errors.

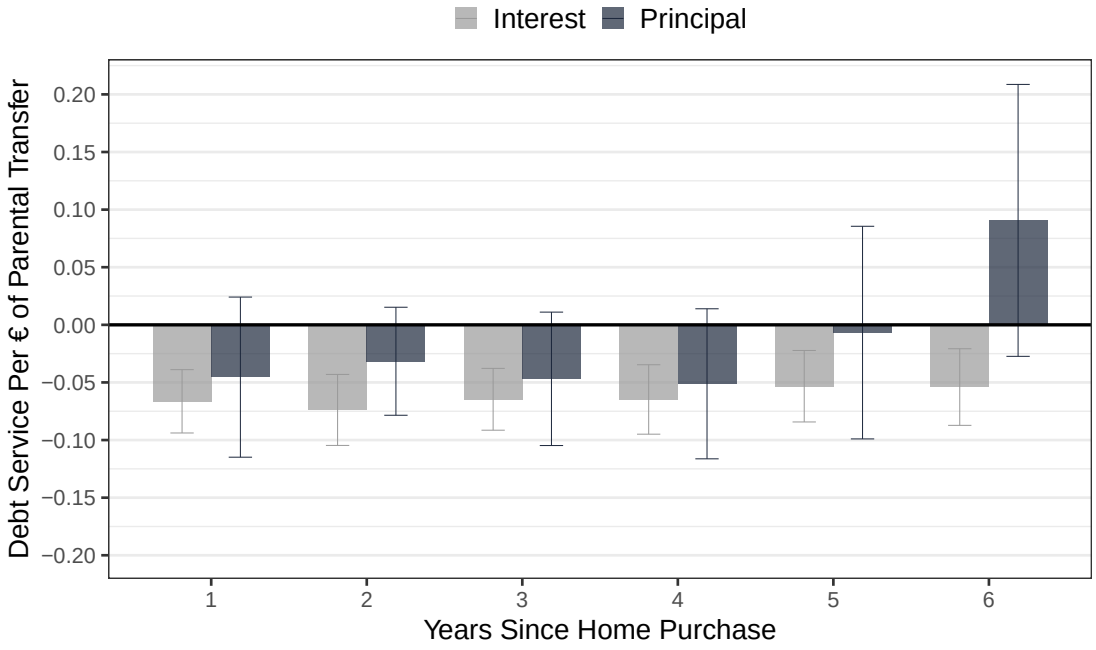


Figure A.9: Debt Service Cost Over Time: Interest Expenses and Principal Repayment

Notes: This figure shows estimated differences in debt service cost (interest expenses and principal repayment) per Euro of parental home purchase transfer, in the first seven years after the purchase. While interet expenses are directly observed, principal repayments are imputed by taking the difference in outstanding mortgage debt between year t and $t+1$. All coefficients are based on 2SLS estimates from equation (11). Error bars represent 95% confidence intervals, based on heteroskedasticity-robust standard errors.