The Effects of Gifts and Inheritances on Consumption, Homeownership, and Wealth

Twisha Asher

Abstract

Americans aged 70 and over own about a third of U.S. wealth, much of which will transfer to younger households over the next decade. However, comprehensive causal evidence explaining how households allocate gifts and inheritances remains absent. In this study, I use longitudinal data from the Panel Study of Income Dynamics (1997–2023) and a modern Difference-in-Differences estimator to assess the effects of such transfers on household finances. I find that households most commonly spend transfers on durable goods expenditures (such as home repairs), which increase by up to $8,200 in the year of receipt. Conservative back-of-the-envelope calculations suggest that an additional $10.6 billion is spent on durable goods due to inheritances alone. Similarly, large inheritances could explain up to 8% of new homeownership. Notably, expenditures on non-durable goods (such as food) only increase for households in the top income or wealth quartiles. These results provide critical insight into the future trajectory of household finances and resolve several contradictions in the literature.

1 Introduction

Households headed by individuals aged 70 and over own about a third of wealth in the United States (Board of Governors of the Federal Reserve System 2025). Given the average life expectancy of about 78 (Arias, Xu, and Kochanek 2025), trillions of dollars will be transferred to younger households over the next decade. To illustrate, the Survey of Consumer Finances finds that about 7.4% of households receive an inheritance over a five-year period, implying a 1.48% annual rate of inheritance (Penn Wharton Budget Model 2021). There are roughly 132 million households in the U.S. as of 2024 (U.S. Census Bureau 2025), suggesting that nearly 2 million households receive an inheritance annually. The figure for both gifts and inheritances is likely to be considerably higher. Understanding how households will respond to these transfers can offer valuable insights into the future trajectory – and perhaps even the current state – of household finances and spending.

However, the research on household apportionment of wealth transfers is highly fragmented. Outcomes have been assessed iteratively, and definitive conclusions are elusive. Straightforwardly, upon the receipt of a wealth transfer, households may increase wealth (by saving, buying a home, *etc.*), reduce labor supply and maintain consumption, or increase consumption. From the literature, it is evident that intergenerational transfers increase homeownership (Wang and Squires 2024), although wealth gains relative to other households are not sustained in the long-run (Elinder, Erixson, and Waldenström 2018; Nekoei and Seim 2023). Moreover, transfers generally have little to no effect on household labor supply (Cox 2014). Given that households do not reduce labor supply or increase wealth in the long-run, consumption expenditures must be examined.

Unlike labor supply, housing, and wealth inequality, consumption has not been studied extensively. Studies that examined the effects of wealth transfers on food expenditures found, at most, a resulting increase of $14 per year (Joulfaian and Wilhelm 1994; Suari-Andreu 2021; Belloc, Molina, and Velilla 2023). However, food consumption is generally considered inelastic, and the conclusions may not be generalizable to other types of consumption. While the effects of lump sum inheritances on durable goods (such as home furnishings or repairs) and non-durable goods (such as food or gas) have recently been studied (Belloc, Molina, and Velilla 2025), the literature currently lacks comprehensive causal evidence on the effects of all gifts and inheritances on overall consumption, as well as the persistence of such effects over time.[1]

This analysis examines a comprehensive set of outcomes – homeownership, non-home wealth, mortgage balances, and expenditures on durable and non-durable goods – to provide a clear picture of how households allocate wealth transfers. Here, I use longitudinal data from the 1997 to 2023 waves of the Panel Study of Income Dynamics (PSID) with a modern Difference-in-Differences estimator (Callaway and Sant'Anna 2021) that accommodates the staggered timing of transfers. I also employ econometric methods proposed by Rambachan and Roth (2023) and Freyaldenhoven, Hansen, and Shapiro (2019) to both minimize and allow for pre-trend violations, thus relaxing the parallel trends assumption. The adjusted estimates are then reported

[1] Belloc, Molina, and Velilla (2025) estimate a first-difference model with household fixed effects using Ordinary Least Squares (OLS) to study effects in the year of inheritance only.

by: (1) type of transfer (gift or inheritance), (2) size of transfer (under or over $50,000), (3) year relative to transfer (up to six years after receipt), and (4) calendar period (all years, 1997 to 2009, and 2011 to 2023). Additional analyses explore responses conditional on pre-existing household finances by dividing households into the top 25% and bottom 75% of the income and wealth distributions in the year before transfer.

Results show that an increase in durable goods expenditures is the most common response to all wealth transfers: recipient households increase expenditures on durable goods by $3,800 to $8,200 relative to comparable non-recipients in the year of transfer, especially in the post-Great Recession period. Assuming 2 million households received inheritances annually over the last decade (U.S. Census Bureau 2025), and focusing just on the effect associated with small inheritances – a $5,300 increase in durable goods expenditures in the year of transfer – implies that inheritances alone spur nearly $10.6 billion in expenditure on durable goods annually. Moreover, most inheritances are large and lead to more substantial increases in such expenditures, which makes this a relatively conservative rough estimate. Meanwhile, non-durable goods expenditures only increased for high-income or high-wealth households and just in the 1997 to 2009 period. No such increase is detected for the overall sample or in the post-2011 period, which may explain why previous studies found no effect on food expenditures.

Other findings add significant nuance to the literature. Homeownership increases exclusively in response to large transfers, and not for all groups: only low-income and low-wealth households in both periods and high-income households in the 2011 to 2023 period saw increases of about 6 and 9 percentage points in the same year as a large gift or inheritance, respectively. To extrapolate, note that about half of all inheritances are large; the figures above thus suggest that about 1 million households receive large inheritances, potentially translating to approximately 90,000 new home-owning households annually. This rough estimate is equivalent to nearly 8% of all new homeowners in 2022 (Joint Center For Housing Studies of Harvard University 2022). Mortgage debt was unaffected, even in the pre-2011 period when the average annual interest rates were higher. Several additional results, sensitivity analyses, and robustness checks are discussed below.

2 PSID, Wealth Transfers, and Other Data

The Institute for Social Research (ISR) at the University of Michigan conducted the PSID annually until 1997, when it was switched to a biennial structure. This study thus uses PSID data from 1997 to 2023 to avoid comparability issues with prior survey years (Social Research Center 2025).

The PSID's main Survey Research Center (SRC) sample was designed to be nationally representative as of 1968, and efforts to reflect the U.S. population are ongoing. Waves of Latino and immigrant families were added between 1990 and 1999 to track demographic trends, and attempts were made to contact participants who stopped responding to the survey.[2] Variables that require explanation or clarification, including outcomes

[2]It is worth noting, however, that the PSID does not capture the top of the wealth distribution, and wealth estimates from the PSID are lower than in estimates from the Survey of Consumer Finances (SCF). For instance, internal SCF data indicate

and transfers are discussed below. Imputed values from the PSID are included where applicable to maximize data availability.[3] [4]

Wealth transfers are reported under two sets of variables in the PSID. The first set captures some lump sum inheritances, while the second (in theory) captures all gifts and inheritances. While the lump sum variable is used more widely in the literature (Belloc, Molina, and Velilla 2025; Lee et al. 2020; Luea 2008), it has significant drawbacks that can lead to a substantial undercount of inheritances, as described below.

Two adjustments are made to the transfer variables: (1) All transfers of less than $18,000 (2023 USD) are coded as zero, as explained below;[5] (2) Gifts and inheritances are attributed to the survey year in which they are reported. This is because any transfer not reported in a survey year was likely received after the survey interview, and is thus unlikely to affect outcomes in that survey year. Therefore, changes probably only appear once a transfer is reported, and in subsequent survey years. While 'year of receipt' and 'year of transfer' are used interchangeably, both refer to the survey year in which the transfer was reported.

Since 1968, the PSID has asked respondents about lump sum payments received by any members of the household in the preceding year, which are reported as income. Starting in 1988, the survey added a follow-up question about how much of the reported sum was from an inheritance. While the survey switched to a biennial structure in 1997, the question continues to ask about payments in the prior year, *i.e.* in the year between surveys. For instance, the 1999 question asked, "Did you (or anyone else in the family there) get any other money in 1998–like a big settlement from an insurance company, or an inheritance?" The follow-up was: "How much of that was an inheritance?" (Institute for Social Research, University of Michigan 2023, 573). The variable, therefore, does not identify lump sum inheritances received in the same years as the survey, and may thus leave out half of these transfers. As a result, the gifts and inheritances variable is essential to capture as many transfers as possible.

General gifts and inheritances are reported as wealth. Respondents are asked about years of receipt and value at the time of receipt for up to three assets or transfer events. Gifts and inheritances valued at over $10,000 (nominal values) received by any member of the household since the last survey are reported in the PSID.[6] As indicated above, any transfers (including lump sum inheritances) of less than $18,000 in 2023 USD ($10,000 in 1997 is roughly $18,000 in 2023) are thus set to zero. Transfers are categorized as small ($18,000

that the average wealth in 2001 was roughly $397,400 and the median was $86,600 (nominal values). By contrast, the 2001 PSID sample had an average and median net wealth of $243,600 and $62,900, respectively (Federal Reserve 2023).

[3]The PSID conducts both individual and family interviews and has had various supplemental surveys over time. The individual-level files are necessary to identify heads of households or 'reference persons' and their characteristics (such as age), but the rest of the data used in this paper are obtained by matching the reference person to their family interviews. This is because all outcome and transfer-related variables used for this study are available at the household level only. Questions regarding gifts and inheritances were added to the main survey in 1999.

[4]Imputed values from the PSID are included when available to maximize data availability. It is worth noting the trade-off for using imputed values: because group averages are maintained with imputations, some values are not plausible. This may be problematic for the analysis, but the imputed values are not used for the transfer-related variables.

[5]$18,000 (2023 USD) is the threshold for reporting gifts and inheritances (in real terms) in the first included year.

[6]The 2021 question was: "During the last two years, have you (or anyone in your family living there) received any large gifts or inheritances of money or property worth $10,000 or more?" (Panel Study of Income Dynamics 2025, 704).

to $50,000 in 2023 USD) or large (over $50,000 in 2023 USD) in the analysis. To distinguish between types of transfers, I use two methods to identify inheritances in this variable. The first is to obtain years of death for parents and grandparents of heads of households and their spouses (when available) using the PSID's Family Identification Mapping System. However, this excludes several inheritances, such as those received by the first waves of immigrant and Latino samples. Therefore, the use of another method is essential.

The lump sum inheritance variable is used to identify inheritances in two ways: (1) directly, if valued higher than any other transfers reported in the same survey year,[7] and (2) to identify years of inheritance, so that other transfers reported in the same survey year are categorized as inheritances.[8] Transfers that are not identified as inheritances are considered to be gifts.[9]

The treatment year for each transfer category (small/large, gift/inheritance) is the year in which a household first reports a transfer, if it falls in that category. Households are dropped in and after any year in which they report a second transfer to allow for a clean analysis while preserving as many observations as possible.[10] Moreover, households that report any category of transfer are excluded from the control groups for all other categories of transfers,[11] and control groups are thus limited to households that reported no transfers across all PSID waves, including survey years before 1997.

Turning to outcome variables: while homeownership, home value, mortgage debt,[12] and non-home wealth (non-home assets minus non-mortgage debts), are relatively self-explanatory, consumption expenditures merit further explanation. Most consumption variables are available in the PSID since either 1999 or 2005. They are categorized as durable goods or non-durable goods (see Belloc, Molina, and Velilla 2025), or aggregated for total expenditures. Durable goods include household repairs and furnishings, and vehicle-related expenses (excluding gas, insurance, and parking). Non-durable expenditures include healthcare, food, clothing, utilities, all other transportation costs, childcare and education, vacations and recreation costs, rent, and select housing costs.[13] [14] Therefore, while the rest of the paper refers to expenditures on goods, services are also included.

The PSID's total expenditure variable includes expenditures on durable and non-durable goods, as well

[7] This is because it is not clear how much overlap, if any, exists between the two variables. Very few households that report lump sum inheritances of over $10,000 also report a value for gifts and inheritances in the same year. Lump sum inheritance amounts also often exceed the gift and inheritance amounts. However, the gifts and inheritances question does not explicitly ask that respondents exclude lump sum inheritances from their response. The relationship between the two variables, therefore, remains muddled. As a result, values from only one of the two variables are used in the analysis, not a combination of the two.

[8] Therefore, if a household reports $1,000 in lump sum inheritances as well as $22,000 under gifts and inheritances in 2023, that will be treated as a $22,000 inheritance for the analysis.

[9] A buffer period of two years (the time between consecutive surveys) is used when identifying inheritances to allow for delays due to ownership transfer, litigation, or other administrative issues. However, few transfers are identified in these buffer periods.

[10] As detailed in Section 3, assumptions for the estimator used in the analysis include treatment being an absorbing state. The central results are not sensitive to this restriction, as shown in Appendix Section B.1.1.3.

[11] The main results from the analysis are not sensitive to this restriction, see Appendix Section B.1.1.

[12] Mortgage principal is the total mortgage balance for up to two reported mortgages. These values are adjusted for inflation and the corresponding analysis is limited to households with positive mortgage balances before the receipt of the first transfer, and to households that did not move since the prior survey year. The variable will thus zero in on the debt-related behavior of existing homeowners. The mortgage results do not change if all observations are included. See Appendix Section B.1.1.4.

[13] Unlike Belloc, Molina, and Velilla (2025), rent equivalent for homeowners is not included in the analysis since it is not an actual expenditure. Property taxes and home insurance involve money outlays and are thus included.

[14] A detailed breakdown of the components of each type of expenditure is included in Appendix Table A.1.

as a few other variables. Mortgage-related payments (which are related to wealth) and expenditures on computers and electronics (which are only available since 2017 and are not easily identified as durable or non-durable goods) are included in total expenditures, but not under durable or non-durable goods (Panel Study of Income Dynamics 2025, 1414). As a result of inconsistent data availability over time, the definition of total expenditures in the PSID changes slightly between 1999 and 2005, and 2005 and 2017. Notably, expenditures on home furnishings and repairs, clothing, vacations, and recreation are included in and after 2005. Sub-categories of goods are used to include data between 1999 and 2005.[15]

Many covariates are included in the analysis and are in keeping with the literature (Blickle and Brown 2019; Belloc, Molina, and Velilla 2025), unless infeasible due to estimator assumptions, as discussed in subsequent sections. Variable definitions are straightforward, and complete lists of variables used in each regression are included alongside the results.[16]

Inflation adjustments are made separately for home equity and all other dollar values, given the substantial differences between home price appreciation and general inflation over the analysis period. Home equity is adjusted to (January) 2023 present values based on average annual home price data (from series USSTHPI) from the Federal Housing Finance Agency (U.S. Federal Housing Finance Agency 2025). All other dollar values are adjusted for inflation to 2023 USD using the inflation index for the U.S. from the World Inequality Database (World Inequality Database, n.d.).

Descriptive Statistics

Table 1 provides a summary of the key descriptive statistics for the entire 1997 to 2023 period, grouped by type of transfer received; recipient households received a transfer at some point. The means are calculated using PSID longitudinal weights for the household in each year, which correct for attrition bias. Averages for control variables are available in Appendix Table A.2.

About a fifth of all households receive gifts or inheritances – roughly 8% of all households receive small transfers, while about 10.9% receive transfers in excess of $50,000. Inheritances are, on average, 1.6 times larger than gifts. The averages for small gifts and small inheritances are similar (about $31,000); the average large gift is about $230,000, while the average large inheritance is about $346,000. All average outcome values for recipients are higher than for non-recipients. However, inheritance recipients have higher rates of homeownership and home-related durable goods expenditures than gift recipients, while the latter have higher home values, total expenditures, non-durable goods expenditures, and total incomes. This is consistent with group demographics: the average gift recipient is 3.7 years younger than the average inheritance recipient.

[15]For instance, expenditures on vehicle-related durable goods are available since 1999. Available variables are included as a separate set of durable and non-durable goods for the analysis. See Appendix Table A.1 for details.

[16]Regional indicators are limited to the South and West based on the descriptive statistics to allow for an optimally matched control group. Results do not change if either the Northeast or Midwest indicator is included.

3 Difference-in-Differences Estimators

Given the structure of the data, causality can be pinpointed with correctly implemented Difference-in-Differences (DiD) estimates. Roth et al. (2023) detail recent developments in DiD methods and recommend discussing "comparison group and time frame selection, causal estimands, estimation methods, and robustness checks" (p. 2220). Accordingly, two characteristics of the data used in this study warrant consideration:

1. **Multiple time periods:** the treatment is staggered across two and a half decades, and the effects may vary with time period. This weakens any arguments for the two-way fixed effects estimator; if the effects differ over time, a fixed effects estimator may use newly treated households as controls for households that were treated in prior years, leading to so-called 'forbidden comparisons' that can distort the values, and even reverse the sign.[17] Therefore, an estimator that selects appropriate comparisons and allows for time heterogeneity is required (p. 2219-24).

2. **Parallel trends assumption:** this standard assumption asserts that all outcomes would evolve in parallel without treatment. This may be problematic: most inheritance recipients are older (and generally wealthier), and their behavior may differ from younger, non-recipient households.

    - It is also possible that there is an anticipatory effect for households expecting wealth transfers, which is incompatible with standard DiD approaches.

Given that time heterogeneity is the primary consideration in producing estimates, the Callaway and Sant'Anna (2021) estimator is the most appropriate for staggered treatment timing in this case. Estimates are calculated separately for every cohort (year of treatment), in each year of available data. Estimator assumptions and the manner in which they are met in the analysis are as follows:

1. Treatment is an absorbing state, which means that transfers are a one-time event. To meet this assumption, only the first transfer is included. Households that receive multiple transfers are dropped, starting with the year in which the second transfer is reported.[18]

2. Generalized parallel trends, as described above. Potential bounded violations of this assumption are taken into account in the results, as detailed in the next section.

3. No anticipatory effects, such that a transfer does not affect the outcome before it is actually received.[19] Event graphs with the Callaway and Sant'Anna (2021) estimator using lead treatment dummies do not indicate any anticipatory effects. Individual graphs for each cohort and outcome are available in Appendix C and also do not indicate any anticipatory effects.

[17] Issues may thus arise if, for instance, the effects of wealth transfers on homeownership vary between those who received transfers in 2007 and those who received them in 2011. As another example, the availability of cheaper goods due to expanded global trade can also be problematic, especially if the effects within the U.S. are inconsistent by area.

[18] While this eliminates about a third of all transfers, the results are not sensitive to this choice. See Appendix B.1 for details.

[19] As noted above, transfers are attributed to the year in which they are reported, not the year in which they are received. The year in which a transfer is reported is most likely to be the year in which any effects are first reported, as well.

Before digging into the Callaway and Sant'Anna (2021) estimator further, note that the receipt of a transfer will move a household from the status of untreated to treated. Treatment effects are estimated at time t for a cohort that received the treatment in time g by comparing the average change for the cohort between *g-1* and t to never-treated and not-yet-treated observations. The Stata package *csdid* estimates all possible 2x2 DiD treatment effects for each cohort and year combination using doubly-robust inverse probability weights: The values of covariates in the period before treatment are used for both propensity score matching to identify appropriate control groups for each cohort, and for regressions (OLS) to model conditional expectations of the outcomes for the control group. Dynamic effects are then aggregated by time relative to treatment year across all cohorts; the individual 2x2 effects are derived and then aggregated by event as follows for a cohort that received treatment in year g and is being observed at time t (Callaway and Sant'Anna 2021, 208):

$$\theta_{\text{es}}^{bal}(e;e') = \sum_{g\in\mathcal{G}} \mathbf{1}\{g+e' \leq \mathcal{T}\} ATT(g,g+e) P(G=g \mid G+e' \leq \mathcal{T})$$

where e is the period relative to treatment ($e = t-g$), e' defines the highest period relative to treatment for which the aggregated estimate can be calculated, thus restricting aggregation to only the cohorts that are in the data for at least e' periods after treatment. e and e' are such that $0 \leq e \leq e' \leq T-2$, where T is the last time period in the analysis, and G is a binary variable indicating treatment (transfer) status. $P(G=g \mid G+e' \leq \mathcal{T})$ is the probability of being in the treatment group at time g conditional on $G+e'$ being in the data (*i.e.* that the cohort is observed for at least the time horizon relative to treatment for which the aggregation is being calculated). $ATT(g,g+e)$ is the average treatment effect in period $g+e$ for the cohort treated in period g. The period before treatment is used for baseline values, which is consistent with typical event studies. Longitudinal weights from the PSID are used as probability weights to correct for attrition, and errors are clustered at the household level (see Bertrand, Duflo, and Mullainathan 2002).

Next, the parallel trends assumption must be addressed. Typically, testing this assumption involves checking for placebo effects or ensuring that pre-treatment differences in outcomes between treatment and control groups are close to zero. However, as argued in Roth et al. (2023), the assumption is essential for causality, and these approaches are thus inadequate. Rambachan and Roth (2023) propose a robust approach that allows for differences in outcomes in the post-treatment period based on the magnitude of pre-trend violations. The estimate for the effect of the treatment, β, can be decomposed into the actual causal effect, τ, and pre- and post- treatment bias δ_{pre} and δ_{post} (Rambachan and Roth 2023, 2):

$$\beta = \underbrace{\begin{pmatrix} 0 \\ \tau_{\text{post}} \end{pmatrix}}_{=:\tau} + \underbrace{\begin{pmatrix} \delta_{\text{pre}} \\ \delta_{\text{post}} \end{pmatrix}}_{=:\delta}$$

The effects are then constrained by Δ, the permissible maximum violation of parallel trends after treatment, as a factor of $\bar{M}$, so that $\Delta = \bar{M} \times$ *maximum departure from parallel trends in the period before treatment*

(p. 3). The reported results will include the estimates with $\bar{M} = 1$.

Furthermore, lagged outcome variables are used as covariates to minimize pre-trend violations, in keeping with Freyaldenhoven, Hansen, and Shapiro (2019). This approach helps select control groups that are closer to the treated cohorts based on prior trends in the outcome variable. Extended models will also include a lagged first difference for the outcome variable, as well as lagged first differences for several additional variables including total income and home value, effectively accounting for values in two prior survey years (four and six years before treatment) when selecting the control group and estimating the effects rather than just using baseline values (two years before treatment). Including these variables invalidates the significance of some estimates – most of the results for small inheritances, for instance – and lends credence to the results that remain significant.

4 Analysis

The theoretical approach expands on Blickle and Brown (2019), who lay out a model for the effects of transfers on home ownership and home value for households in a two-period lifecycle model (p. 543-9). The resulting hypotheses for non-housing consumption, homeownership, and non-home wealth are explained below.

For non-housing consumption: (1) if households do not face credit constraints and transfers are higher or lower than expected, households may increase or decrease (respectively) their utility maximizing choice of non-housing consumption; (2) if households face credit constraints, then non-housing consumption will increase after the receipt of the transfer, regardless of the size.

The implications for homeownership and home value are two-fold (see Blickle and Brown 2019, 543–49 for details): (1) For households that are inclined to purchase a home, receiving wealth transfers may ease down payment constraints, thus leading to a higher likelihood of homeownership (Blickle and Brown 2019, 548). (2) For households that have already purchased a home that is smaller (or in a worse neighborhood) than their utility maximizing choice of housing, the receipt of a wealth transfer can ease the down payment constraints – or otherwise bolster financial resources – so that they are more likely to move to a larger (or better) home. For households that receive transfers that are higher or lower than expected, the optimal choice of housing may be revised so that they are more likely to move to larger or smaller homes, respectively (Blickle and Brown 2019, 549).

Extending this model slightly, non-home wealth is only relevant insofar as it affects the household's budget constraint. Therefore, while there may be an increase in non-home wealth after the receipt of a transfer, this will lead to an increase in non-housing consumption or homeownership in the long run.

Quantitative Analysis

Results are grouped by type and size of transfer, and time period.[20] Estimates are for event time (year relative to treatment),[21] and are adjusted in accordance with Roth et al. (2023).[22] This captures effects that can only be identified after some time – for instance, in the case of large purchases such as homes. Detailed results for individual cohorts are in Appendix C, and tables with aggregated estimates are in Appendix Section A.1. Two models are included in each result, and the second controls for a large set of variables to assess the possibility of an omitted variable bias. Only effects significant in both models are discussed.[23]

Figure 2 shows the results for small gifts (of over $18,000 and under $50,000) across all years of data. The results for the full model (Figure 2b) indicate that expenditures on durable home goods are $2,570 higher for recipients than comparable non-recipients in the year of the transfer. Expenditures on vehicle-based durable goods are $7,500 higher for recipients than comparable non-recipients four years after transfer.

The period before the financial crisis does not show any statistically significant effects (Figure 3a). The results for the period after the financial crisis (Figure 3b) suggest that the effects detected in the full time period are driven, at least in part, by the effects of transfers in the 2011 to 2023 period. Recipients spent $3,780 more on durable goods in the year of transfer in this period.

Results for large gifts of over $50,000 (Figure 4) are more varied. Recipients are about 6 percentage points more likely to own homes than comparable non-recipients in both the year of and two years after the transfer. Expenditures on durable home goods and all durable goods are also $4,710 and $7,150 higher for recipients than non-recipients, respectively, in the year of the transfer.

As with small gifts, no results for large gifts are statistically significant in the period before the financial crisis (Figure 5a). The results for 2011 to 2023 (Figure 5b) indicate one statistically significant result: Recipients of large gifts spent $5,330 more on durable home goods in the year of the transfer as compared to non-recipients in this period.

The only statistically significant effect of small inheritances (Figure 6) is that recipients spend $5,330 more

[20] In the 1997 to 2009 period, the treated cohorts reported the transfer between 2001 and 2009. The 2011 to 2023 period includes cohorts that reported the transfer between 2015 and 2023. Other cohorts do not include sufficient years of data to allow for an accurate quantification of effects within the event time window. Period-specific estimates exclude all observations treated before the specified time frame, and not-yet-treated observations are re-coded as untreated for the estimation.

[21] The estimates for the full time period include cohorts that received transfers between 2003 and 2017 for the longest time period (six years before and after the transfer), and between 2001 and 2023 for the shortest time period (four years before and year of transfer). The decade-specific analysis is for four years before, and either the year of or two years after the transfer.

[22] The maximum pre-trend violation is subtracted from and added to the lower and upper bounds of the interval, respectively.

[23] The first model controls for age, head's marital status, head's sex (indicator for female), total family income, number of children, and the lagged outcome variable. The full model additionally controls for race (white or black only), and years of education of the head of household, region of residence (indicators for South and West only), the largest differences between the region of residence for recipients and non-recipients are in these two regions. Homeownership status (not included when homeownership is the outcome variable), and size of household, as well as the first differences for: total family income, head's marital status, number of children, lagged dependent variable, and home value (not included when home value is the outcome variable). This covers nearly all the variables in Belloc, Molina, and Velilla (2025) excluding, for instance, employment status, which should be sufficiently reflected in the total income variable. The breadth of variables suggests that the results that hold across all specifications may be free from omitted variable bias.

on durable home goods in the year of the transfer as compared to non-recipients.

The period before the financial crisis does not show any statistically significant effects (Figure 7a), but the period after the crisis (Figure 7b) shows that durable home goods expenditures increase by $3,420.

Results for large inheritances of over $50,000 (Figure 8) show several significant effects. Recipients are 9, 11, 13, and 15 percentage points more likely to own homes in the year of, and two, four, and six years after the transfer, respectively. They also have homes that are valued 7% and 16% higher in the year of, and four years after the transfer, respectively, as compared to non-recipients. The non-home wealth for recipients is also 136%, 107%, and 101% higher in the year of, and two and four years after the transfer. Moreover, large inheritance recipients also spend $8,190 more on all durable goods in the year of the transfer as compared to non-recipients. Recipients further spend $8,120 more on vehicle-related durable goods in the year of the transfer than non-recipients. Curiously, recipients appear to spend $6,690 less than non-recipients on select non-durable goods (excluding clothing, recreation, and vacations) six years after transfer.[24] [25]

In the period before the financial crisis (Figure 9a), homeownership was 9 percentage points higher for large inheritance recipients than non-recipients in the year of the transfer. Moreover, non-home wealth was 116% and 75% higher for recipients than non-recipients in the year of and two years after the transfer, respectively. Finally, recipients also spent $11,000 more on durable vehicle-related expenditures in the year of the transfer than non-recipients. After the financial crisis (Figure 9b), non-home wealth is once again 188% and 206% higher for recipients than non-recipients in the year of and two years after the transfer, respectively. Recipients spend $9,040 more on durable goods in the year of the transfer than non-recipients. The increase in homeownership is just about statistically significant two years after the transfer, suggesting that recipients are 12 percentage points more likely to own homes than non-recipients two years after the transfer.

Pre-Existing Income and Wealth

Outcomes are now split by high and low pre-transfer (survey year before the transfer) income and wealth. The proportions are chosen to allow for the estimation of all outcomes, including mortgage balances,[26] leading to a split of the sample into the bottom 75% and top 25% of pre-transfer income and wealth (referred to as lower income/wealth and higher-income/wealth, respectively), calculated across recipient and non-recipient PSID households. Coefficients do not change substantially between the basic and full model specifications, and only results for the full model are included below.

Examining effects based on pre-transfer income distributions and time periods reveals substantial heterogeneity across both dimensions. Lower income recipients prioritize homeownership in the year of transfer between

[24] The sample size does fall by two-thirds by this period, and while it is not small, the result should be interpreted with caution. Since the effects of homeownership persist over time, it is possible that the reduction in select non-durable goods expenditures is due to households reducing payments on rent.

[25] Note that the average large inheritance is $116,000 larger than the average large gift. It is quite possible that the results are, therefore, driven by the size of the transfer.

[26] As noted above, mortgage balances for households that moved since the last survey are excluded to zero in on whether households use transfers to pay down existing debt, especially by time period.

1997 and 2009 (Figure 10a): 9 and 8 percentage points more of these households owned homes relative to comparable non-recipient households in the year of the transfer. Lower income households also had 66% higher non-home wealth than comparable non-recipients in the year of transfer. Meanwhile, higher-income recipients increased vehicle-based durable goods expenditures by $6,500 more than comparable non-recipients in this period. Moreover, two years after the transfer, higher-income recipients spent $13,900 more on non-durable goods (excluding clothing, vacations, and recreation) than comparable non-recipients.

In the 2011 to 2023 period (Figure 10b), households across the income distribution shift toward durable goods and, to some extent, homeownership. Lower income recipients spend $5,340 and $1,550 more on all durable goods and durable home goods (respectively) in the year of the transfer, and $1,370 more on durable home goods two years after the transfer, than comparable non-recipients. Meanwhile, higher-income recipients spend $6,810 and $5,360 more on all and home-based durable goods in the year of transfer than comparable non-recipients. All recipient households increase homeownership by 8 and 6 percentage points for lower and higher-income recipients (respectively) in the year of transfer; the effect persists for lower-income recipients, with 8 percentage points more homeownership by two years after transfer compared to non-recipients. Additionally, lower-income recipients also had $5,940 higher total expenditures and 53% higher non-home wealth than comparable non-recipients in the year of transfer.

As with lower-income recipients, lower-wealth recipients prioritize homeownership between 1997 and 2009 (Figures 11a-i) with an increase of 9 percentage points relative to comparable non-recipients in the year of transfer. Moreover, similar to higher-wealth recipients, higher-wealth recipients in this period (Figures 11a-ii) increase non-durable goods (excluding clothing, vacations, and recreation) expenditures by $11,600 relative to comparable non-recipients two years after transfer.

Between 2011 and 2023, lower-wealth recipients continue to prioritize homeownership (Figures 11b-i), which increases by 16 percentage points two years after transfer. Expenditures on durable home goods increase by $6,080 for higher-wealth recipients and by $2,030 for lower-wealth recipients in the year of transfer. The effects persist for lower-wealth recipients, who increase durable home goods expenditures by $1,990 two years after transfer. Durable goods expenditures and total expenditures also increase for lower-wealth households in the year of transfer, by $4,320 and $5,750 relative to non-recipients, respectively. The effect persists for lower-wealth recipients: durable goods expenditures are $4,240 higher two years after transfer.

Appendix Figures C.1 through C.4 show results split further by size of transfer. No effects are observed in response to small transfers in the 1997 to 2009 period, while large transfers in the same period increase homeownership for lower-income, lower-wealth, and higher-wealth recipients. Large transfers in this period also increase vehicle-based durable goods expenditures for higher-income recipients and select non-durable goods expenditures and home value for higher-wealth recipients. In the 2011 to 2023 period, small transfers increase durable goods expenditures and vehicle-based durable goods expenditures for lower-wealth recipients, and durable home goods expenditures for higher-income recipients. The effect on higher-wealth recipients is

also nearly statistically significant. Large transfers in this period increase homeownership for lower-wealth, lower-income, and higher-income recipients. Large transfers in this period also increase all or home-based durable goods expenditures for lower-wealth and higher-income recipients, and the effect is nearly statistically significant for higher-wealth and lower-income recipients, as well.

5 Conclusions

This paper provides a comprehensive, causal analysis of the effects of wealth transfers on typical U.S. households. The analysis spans multiple subcategories of expenditure and wealth, and estimates are separated by type and size of transfer, time relative to receipt, time period, and pre-transfer income and wealth status. I use a modern event-study difference-in-differences design with adjustments for pre-existing trends, allowing these estimates to be interpreted as causal effects. By combining contemporary identification techniques with PSID data covering both the pre- and post-Great Recession periods, the paper updates and significantly adds to the evidence on intergenerational transfers in the U.S. context.

The results show that, regardless of the type or size of transfer, recipient households increase expenditures on durable goods relative to comparable non-recipients in the year of transfer. In the year of receipt, households spend about $3,200 (small gifts) to $8,200 (large inheritances) more on durable goods, especially in the post-2008 period. Increases in (some or all) durable goods expenditures in this period were significant for all income and wealth groups. By sharp contrast, expenditures on non-durable goods only increased for households in the top wealth and income quartiles that received large transfers in the 1997 to 2009 period. This explains why no significant results related to food expenditures were found in previous studies.

All effects on homeownership occurred in response to large transfers only and are highly dependent on pre-existing income and wealth, which adds nuance to prior work that did not separate transfers by both size and pre-existing wealth and income (Wang and Squires 2024). A large inheritance increases the probability of owning a home by roughly 9 to 15 percentage points between the year of and six years after transfer, while recipients of large gifts are 6 percentage points more likely to be homeowners in the year of and two years after receipt. Low-income, low-wealth and high-wealth households increased homeownership in response to transfers in the 1997 to 2009 period, while high-income and high-wealth households also increased home value in the same period. Meanwhile, low-income, low-wealth and high-income households increase homeownership in the 2011 to 2023 period. Based on the theoretical hypotheses, these results are consistent with households facing credit constraints, suggesting they are unable to borrow against future income or wealth.

External validation in other datasets and settings is needed to clarify generalizability. Further stratifications by transfer size (for instance, over $100,000) and pre-transfer wealth levels may reveal additional heterogeneity. Future work can also build on these insights to explore the mechanisms behind the results.

The conditional nature of the results above could help resolve the contradictory literature regarding home-

ownership and long-term wealth accumulation, since the increases in homeownership and non-home wealth are not large or ubiquitous, while recipient households across the board purchase durable goods related to homes or vehicles, which are both depreciable assets.

6 Figures and Tables

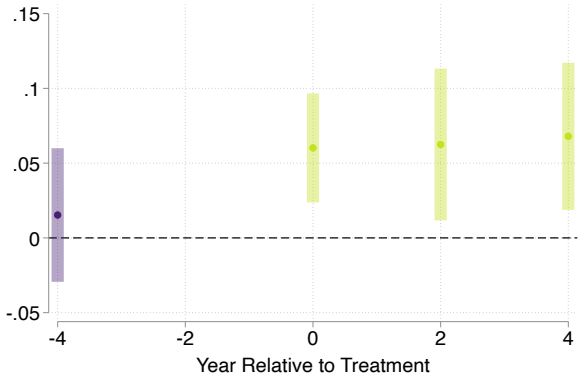
Table 1: Weighted Averages for 1997 to 2023 Grouped by Type of Transfer

Variable	Gifts	Inh	Any Transfer	No Transfers	All
Gift/Inh (Y/N)	1	1	1	0	0.19
	(0)	(0)	(0)	(0)	(0.39)
Gift/Inh (USD)	141,792	224,867	184,741	0	184,741
	(286,605)	(1,217,572)	(898,112)	(0)	(898,112)
Small Gift/Inh (Y/N)	0.44	0.38	0.41	0	0.08
	(0.50)	(0.49)	(0.49)	(0)	(0.27)
Small Gift/Inh (USD)	30,980	30,652	30,839	0	30,839
	(8,380)	(8,651)	(8,507)	(0)	(8,507)
Large Gift/Inh (Y/N)	0.56	0.62	0.59	0	0.11
	(0.50)	(0.49)	(0.49)	(0)	(0.31)
Large Gift/Inh (USD)	230,095	346,355	292,969	0	292,969
	(360,517)	(1,540,010)	(1,159,947)	(0)	(1,159,947)
Home Owner	0.78	0.80	0.79	0.60	0.63
	(0.41)	(0.40)	(0.41)	(0.49)	(0.48)
Home Value	531,646	471,947	500,613	404,729	427,899
	(542,070)	(380,249)	(465,883)	(467,984)	(469,272)
Mortgage Principal	180,548.80	168,147.60	174,256	163,788.20	166,527.50
	(153,026.20)	(149,922)	(151,598.80)	(155,340.30)	(154,435.80)
Non Home Wealth	454,429	580,796	519,783	236,044	288,741
	(1,417,616)	(3,399,375)	(2,632,996)	(1,317,945)	(1,647,434)
Total Expenditures	65,921	60,712	63,254	50,028	52,485
	(45,596)	(43,809)	(44,767)	(42,479)	(43,220)
Durable Goods	13,008	12,899	12,952	8,966	9,695
	(24,348)	(24,861)	(24,615)	(21,041)	(21,793)
Durable Goods Home	4,986	5,222	5,109	3,248	3,589
	(16,002)	(16,535)	(16,283)	(13,661)	(14,195)
Durable Goods Vehicle	8,621	8,383	8,497	6,074	6,524
	(17,779)	(18,255)	(18,025)	(15,003)	(15,637)
Non Durable Goods	43,464	39,669	41,522	34,410	35,731
	(29,600)	(26,807)	(28,263)	(26,006)	(26,584)
Non Durable Goods (Sel)	36,391	33,592	34,953	29,618	30,617
	(23,894)	(21,563)	(22,765)	(20,175)	(20,789)
Total Income	126,919	118,828	122,778	86,857	93,522
	(136,522)	(146,342)	(141,757)	(138,546)	(139,846)
Obsv	8,111	7,621	15,722	96,668	112,390

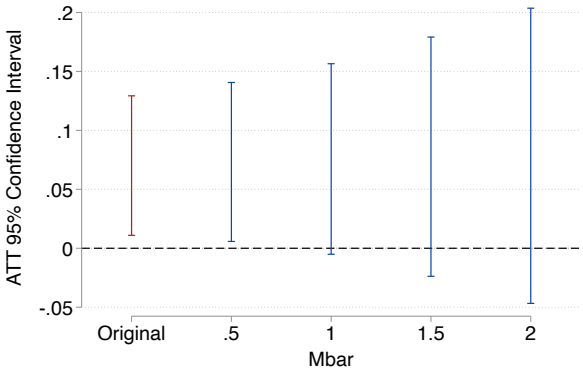
Households	1,713	1,480	3,191	23,448	26,639

Means for the dollar value of transfers, home value, non-home wealth, and mortgage principal are calculated over non-zero values only. Recipient households reported either a gift or inheritance ('Inh') valued at over $18,000 (2023 USD) in any year between 1997 and 2023. Large transfers are valued over $50,000. Households are dropped in and after any year in which a second transfer is reported. Averages are calculated by group for all years with longitudinal weights. Non-Durable Goods (Sel.) includes all non-durable expenditures other than clothing, vacations and recreation.

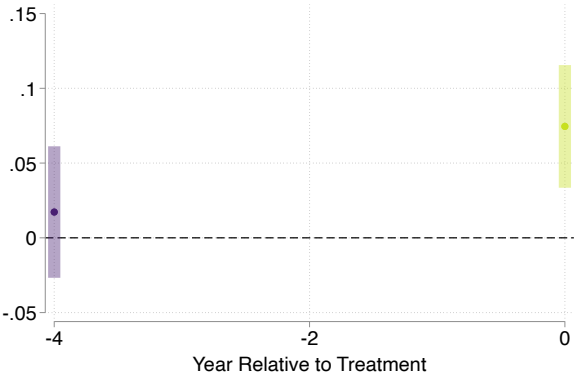
Figure 1: Example to Explain the Structure of the Results: Aggregated DiD Estimates for the Effects of Small Gifts Between 1997 and 2023 on Homeownership with Pre-Trend Violations



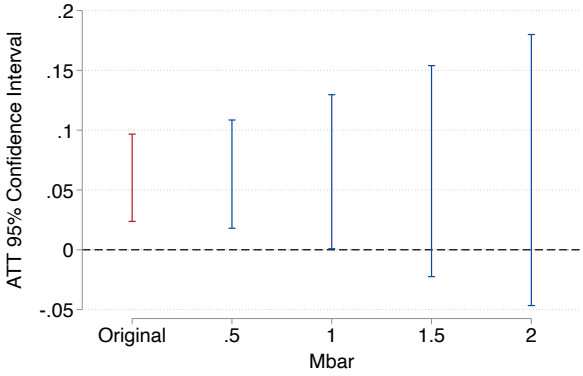
(a) First Gift In 1997-2023



(b) Effect (4 Years After Transfer) w/Pre-Trend (4 Yrs Prior)



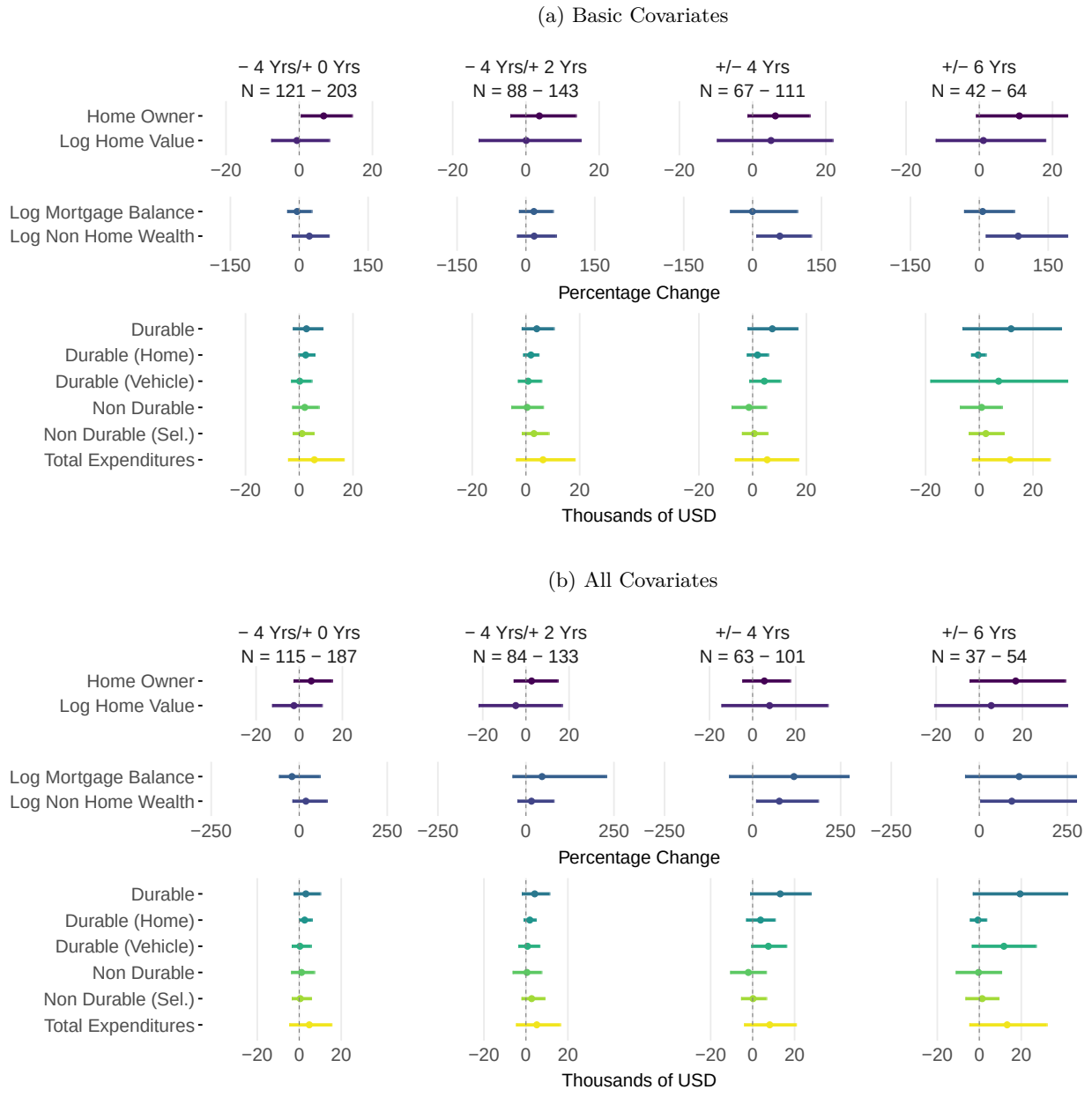
(c) First Gift In 1997-2023



(d) Effect (Year of Reporting) w/Pre-Trend (4 Yrs Prior)

Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates with a 95% confidence interval using 1997-2023 PSID data. Only includes the first transfer if it is not identified as an inheritance and is valued between $18,000 and $50,000 (2023 USD). Households that report inheritances in any year are dropped. Households are dropped in and after any year in which a second transfer is reported. Regression controls for age, marital status, income, number of children, and lagged outcome variable.

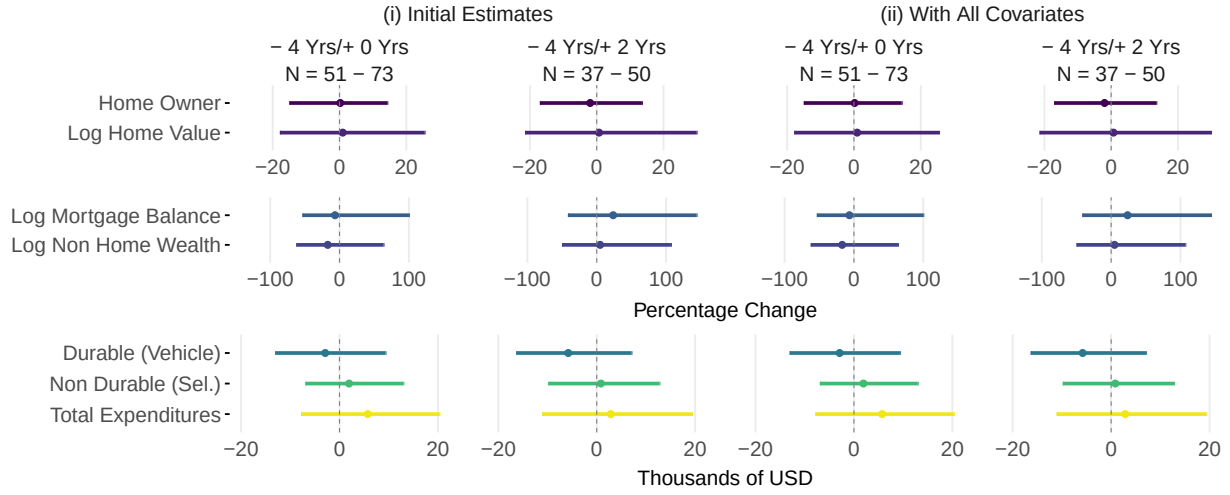
Figure 2: Difference-in-Differences Estimates for the Effects of Small Gifts



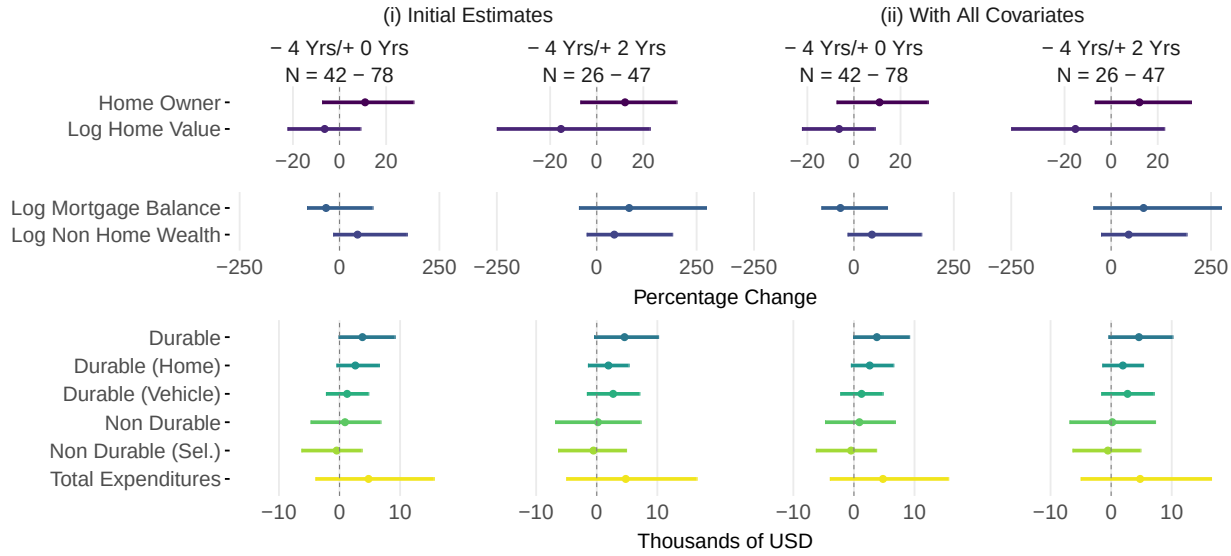
Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0, 2, 4, and 6 years after the transfer) with 1997-2023 PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Only includes the first transfer if it is worth between $18,000 and $50,000 (2023 USD) and is not identified as an inheritance. Households are dropped in and after any year in which a second gift is reported, or if an inheritance is identified in any PSID year. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions in panel (a) control for age, marital status, income, number of children, and lagged outcome variable. Panel (b) additionally controls for race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

Figure 3: Difference-in-Differences for the Effects of Small Gifts Estimates by Period

(a) 1997-2009

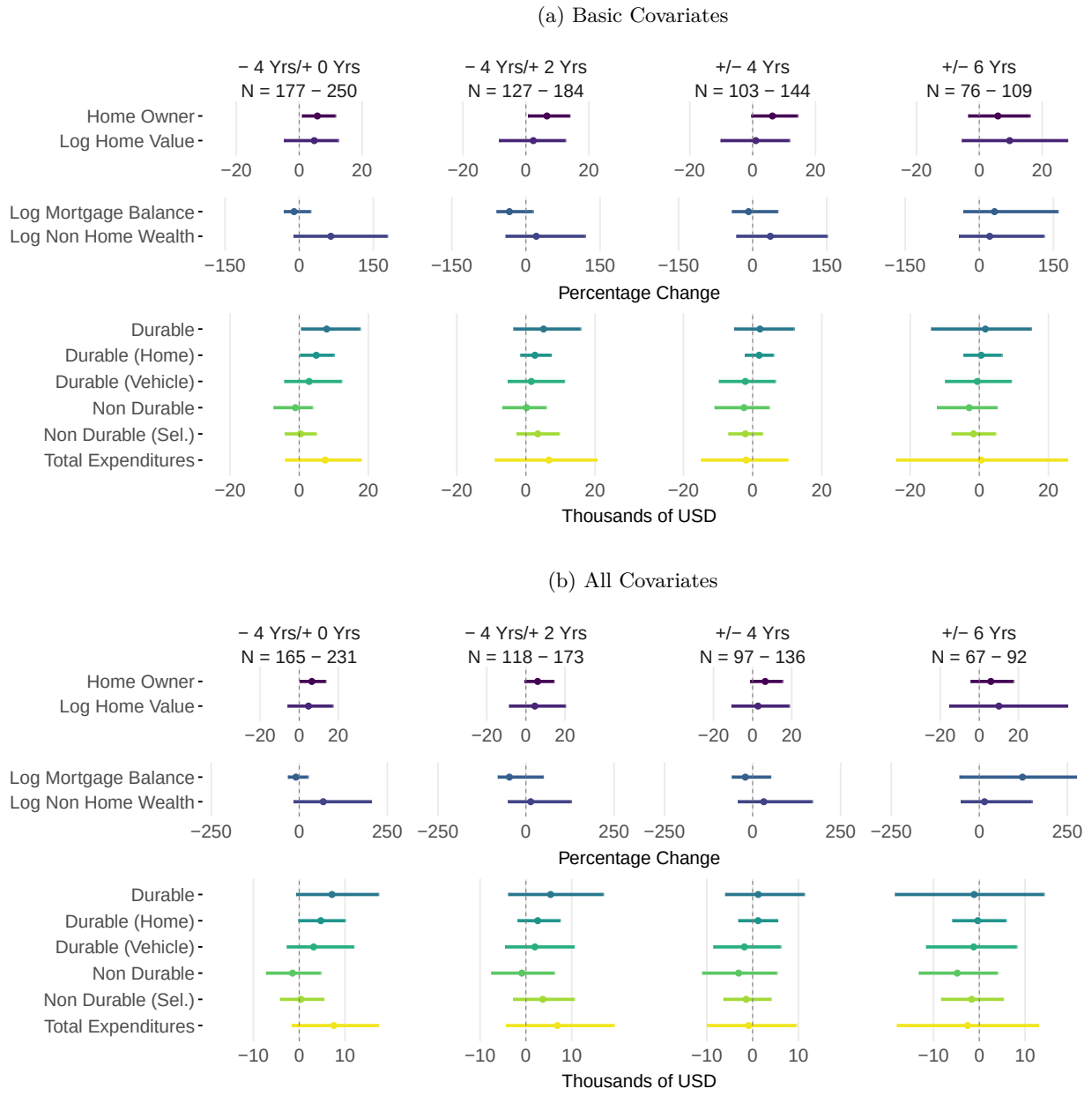


(b) 2011-2023



Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0 and 2 years after the transfer) with PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Only includes the first transfer if it is worth between $18,000 and $50,000 (2023 USD) and is not identified as an inheritance. Households are dropped in and after any year in which a second gift is reported, or if an inheritance is identified in any year. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions in panel (a) control for age, marital status, income, number of children, and lagged outcome variable. Panel (b) additionally controls for race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

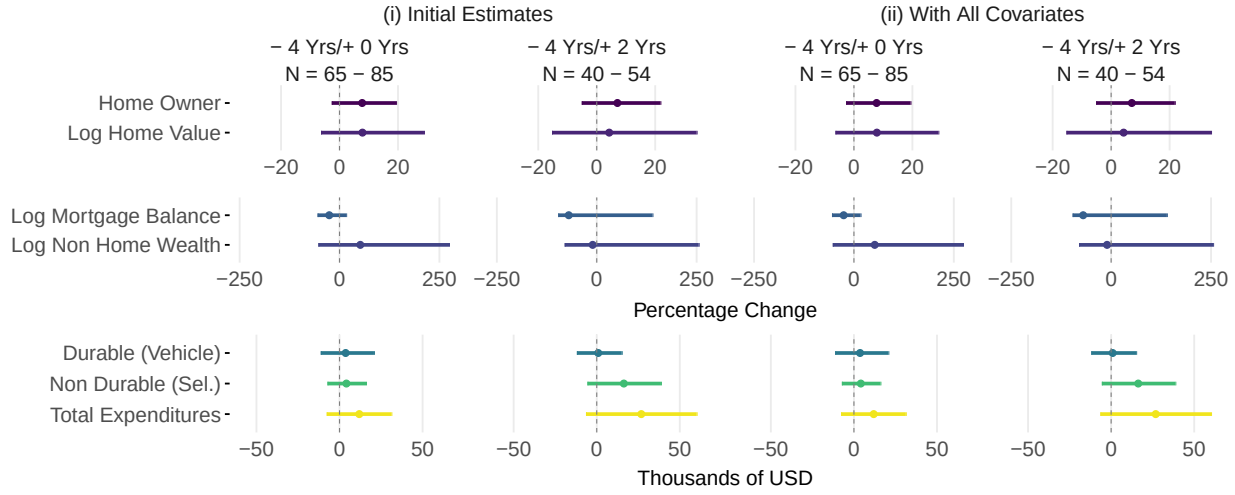
Figure 4: Difference-in-Differences Estimates for the Effects of Large Gifts



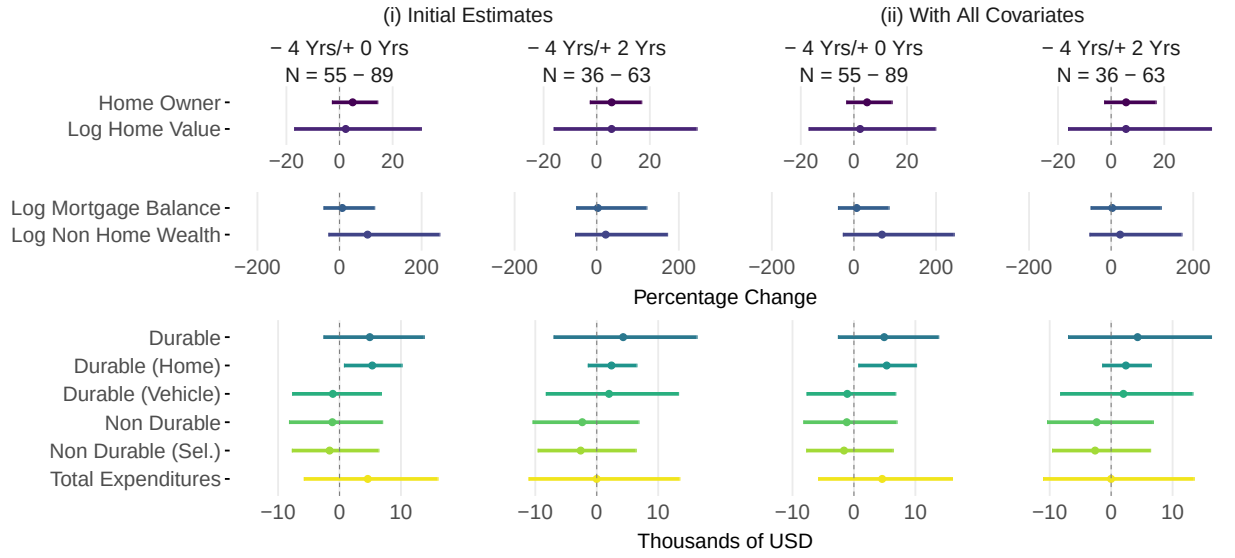
Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0, 2, 4, and 6 years after the transfer) with 1997-2023 PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Only includes the first transfer if it is worth over $50,000 (2023 USD) and is not identified as inheritance. Households are dropped in and after any year in which a second gift is reported, or if an inheritance is identified in any year. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions in panel (a) control for age, marital status, income, number of children, and lagged outcome variable. Panel (b) additionally controls for race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

Figure 5: Difference-in-Differences for the Effects of Large Gifts By Period

(a) 1997-2009



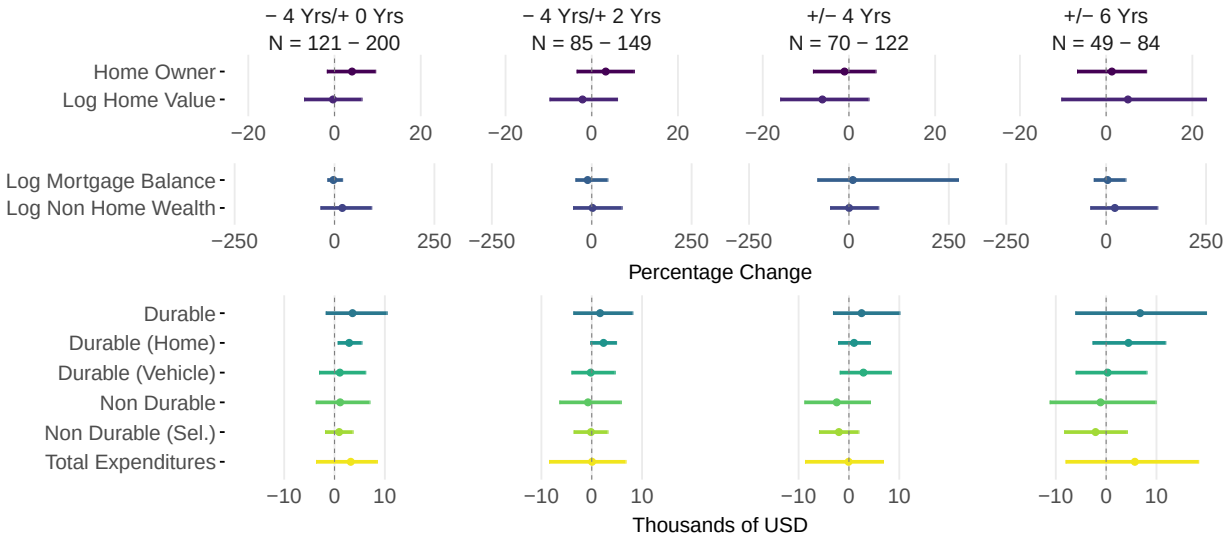
(b) 2011-2023



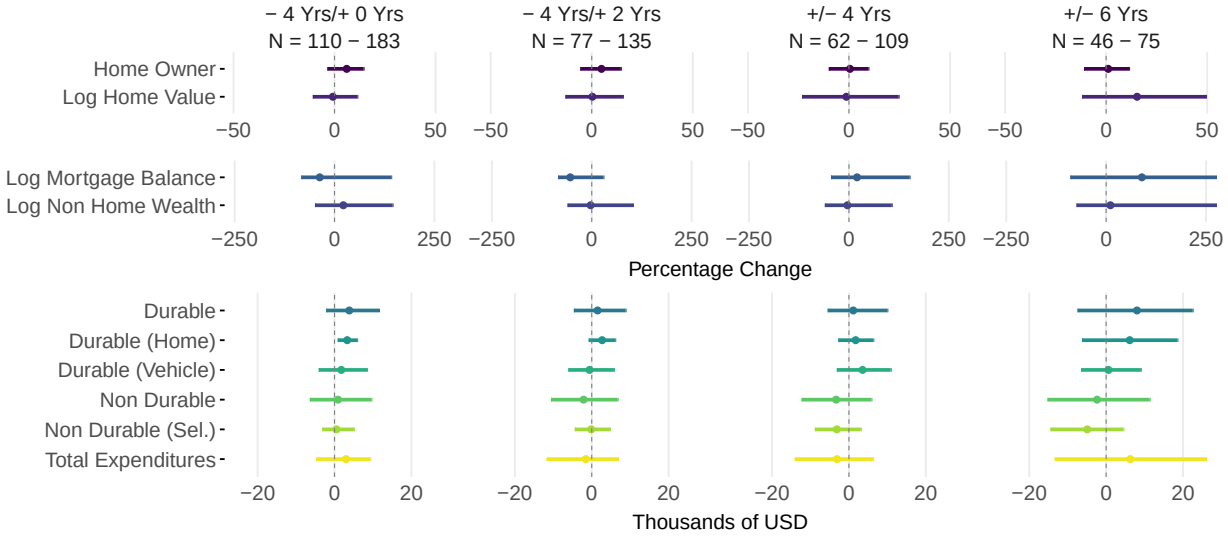
Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0 and 2 years after the transfer) with PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Only includes the first transfer if it is worth over $50,000 (2023 USD) and is not identified as inheritance. Households are dropped in and after any year in which a second gift is reported, or if an inheritance is identified in any year. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions in panel (a) control for age, marital status, income, number of children, and lagged outcome variable. Panel (b) additionally controls for race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

Figure 6: Difference-in-Differences for the Effects of Small Inheritances

(a) Basic Covariates



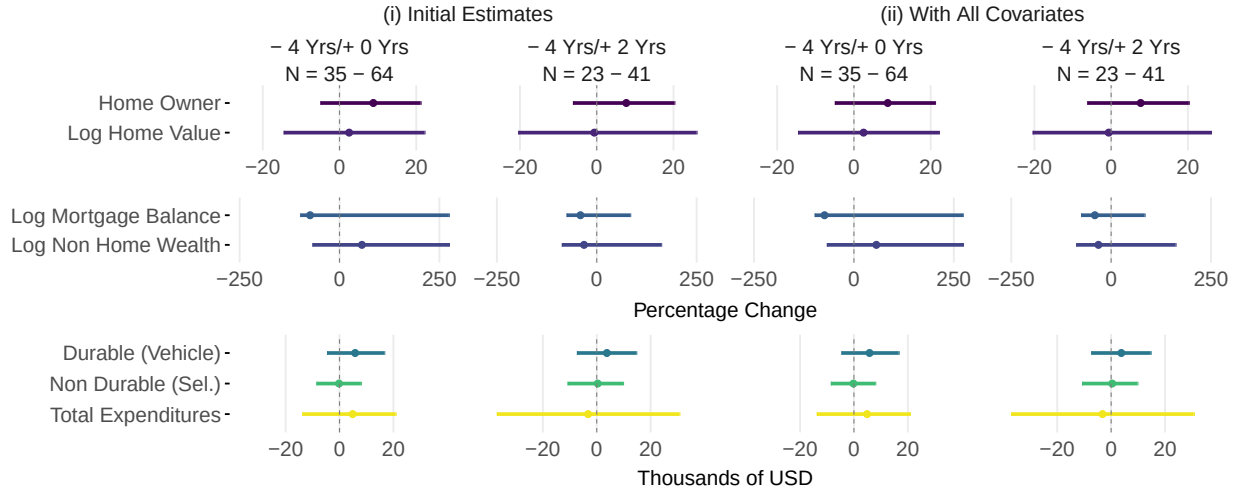
(b) All Covariates



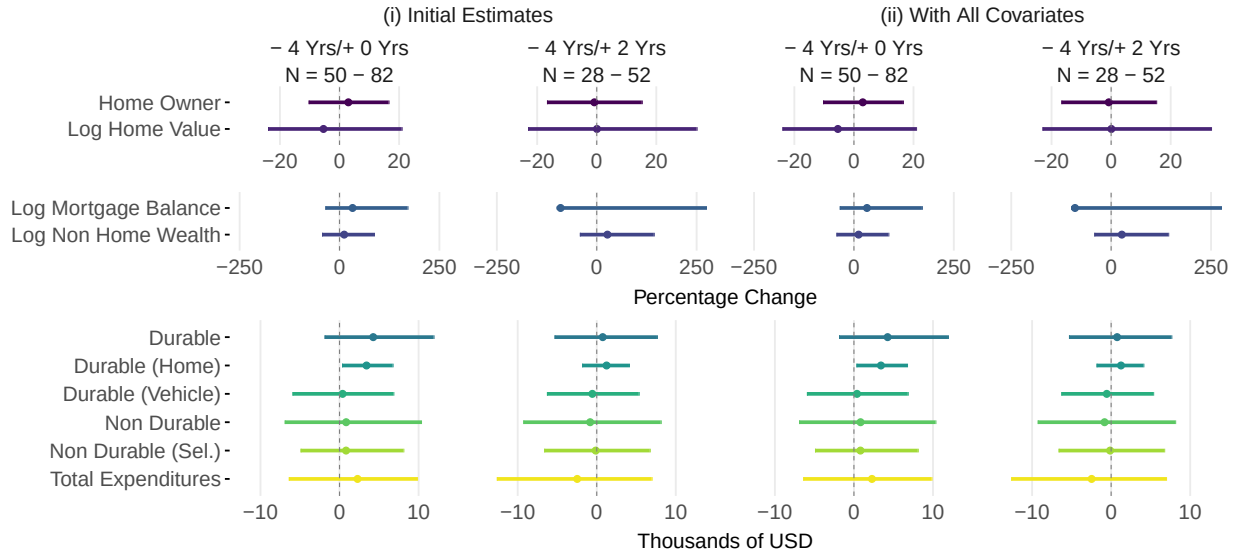
Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0, 2, 4, and 6 years after the transfer) with 1997-2023 PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Only first transfers that are identified as inheritances and are worth between $18,000 and $50,000 (2023 USD) are included. Households are dropped in and after any year in which a second transfer is reported, or if a gift is reported in any year. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions in panel (a) control for age, marital status, income, number of children, and lagged outcome variable. Panel (b) additionally controls for race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

Figure 7: Difference-in-Differences for the Effects of Small Inheritances By Period

(a) 1997-2009

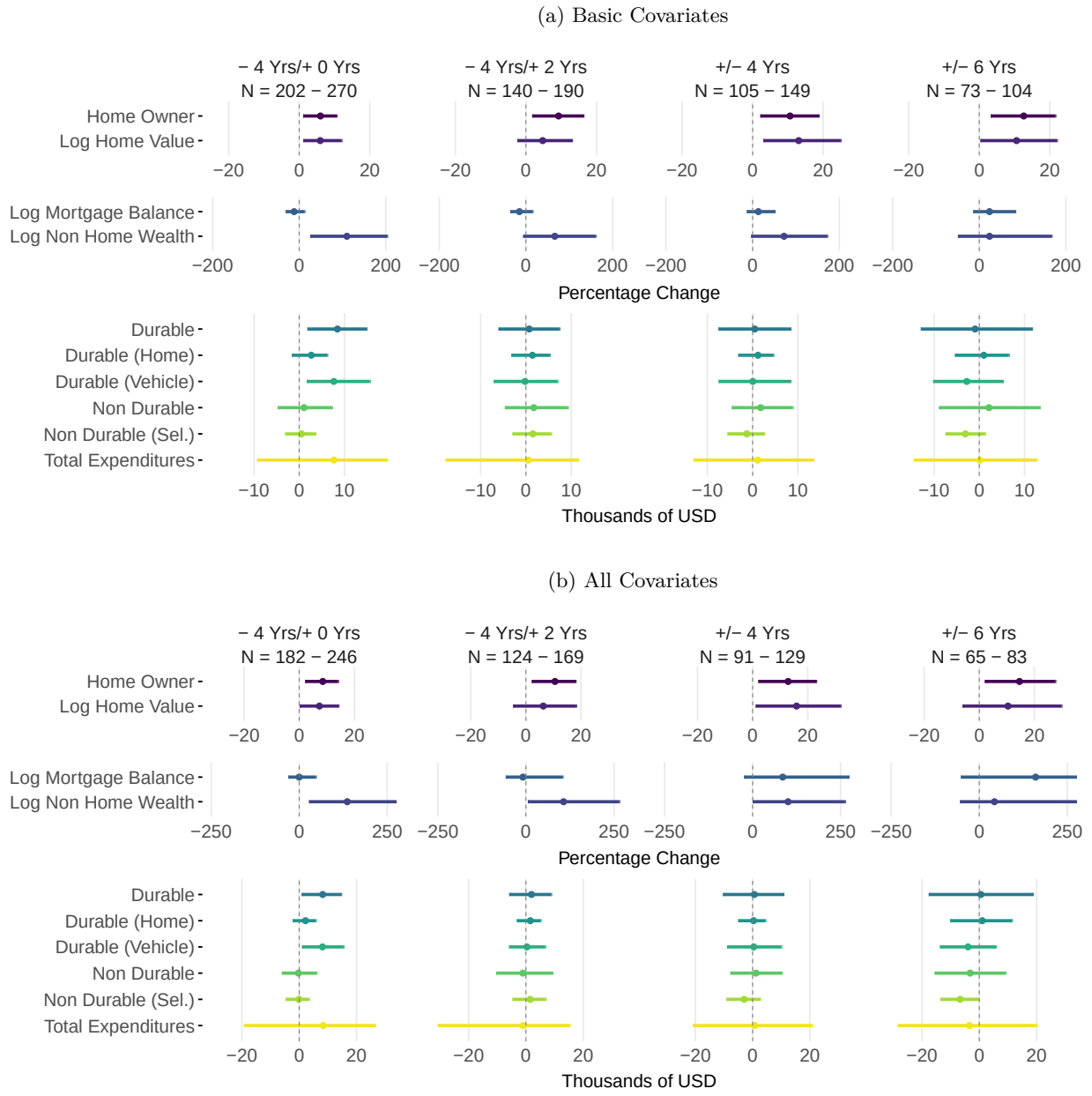


(b) 2011-2023



Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0 and 2 years after the transfer) with PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Only first transfers that are identified as inheritances and worth between $18,000 and $50,000 (2023 USD) are included. Households are dropped in and after any year in which a second transfer is reported, or if a gift is reported in any year. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions in panel (a) control for age, marital status, income, number of children, and lagged outcome variable. Panel (b) additionally controls for race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

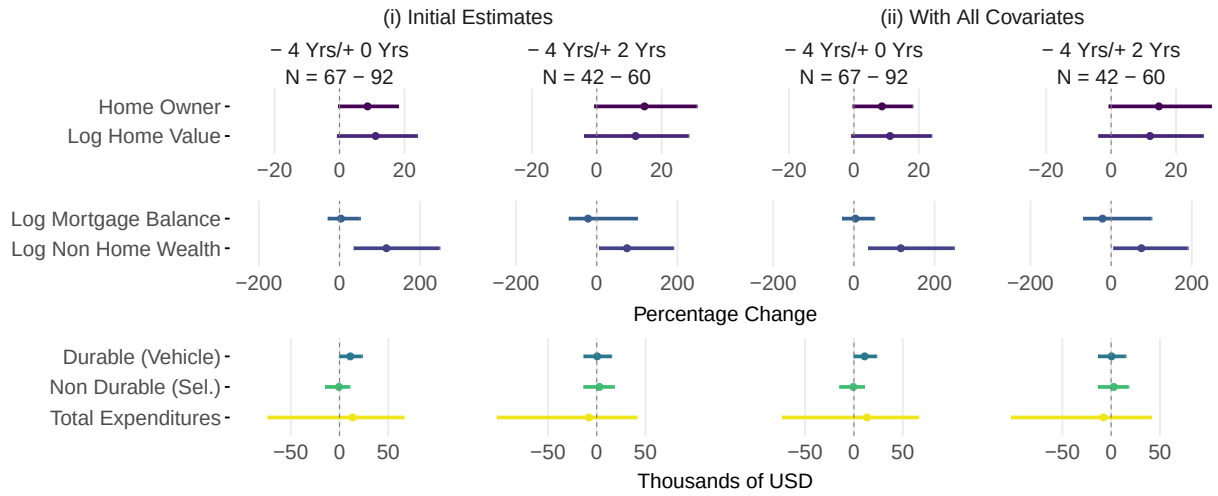
Figure 8: Difference-in-Differences for the Effects of Large Inheritances



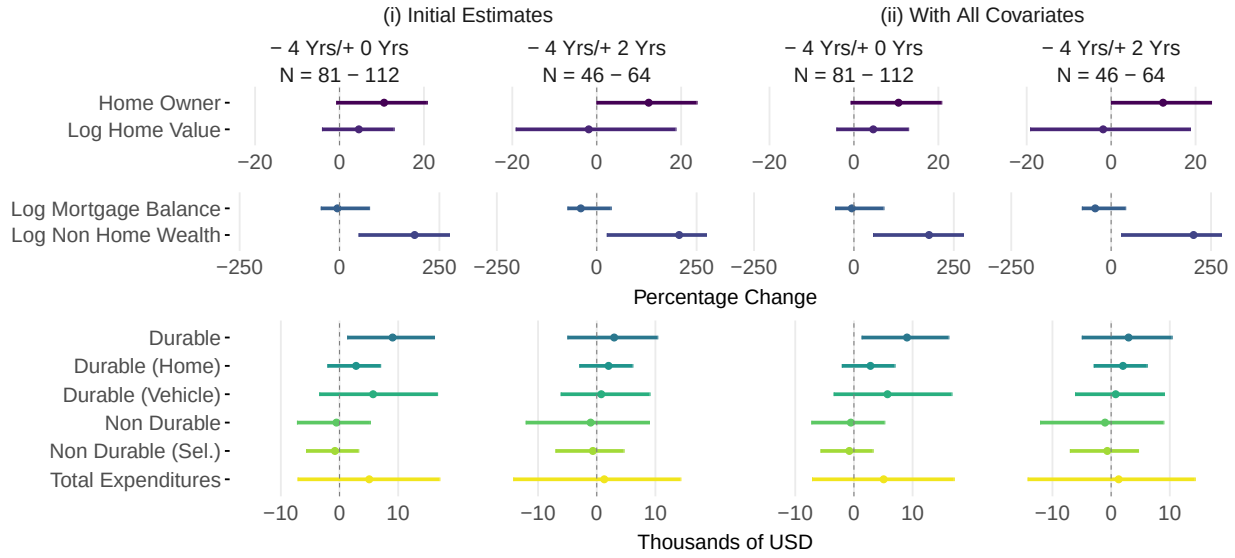
Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0, 2, 4, and 6 years after the transfer) with 1997-2023 PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Only first transfers that are identified as inheritances and are valued at over $50,000 (2023 USD) are included. Households are dropped in and after any year in which a second transfer is reported, or if a gift is reported in any year. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions in panel (a) control for age, marital status, income, number of children, and lagged outcome variable. Panel (b) additionally controls for race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

Figure 9: Difference-in-Differences for the Effects of Large Inheritances By Period

(a) 1997-2009



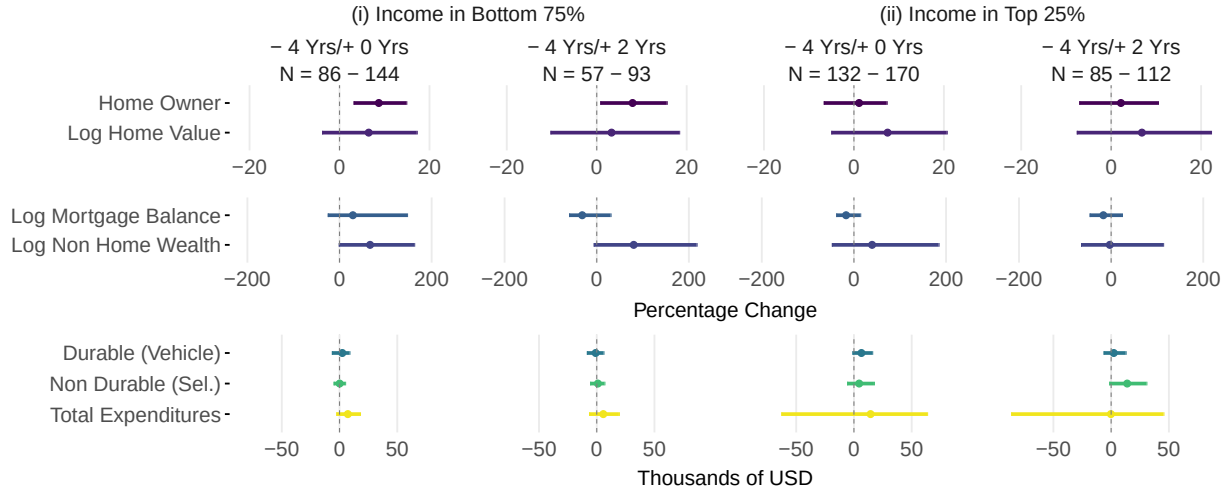
(b) 2011-2023



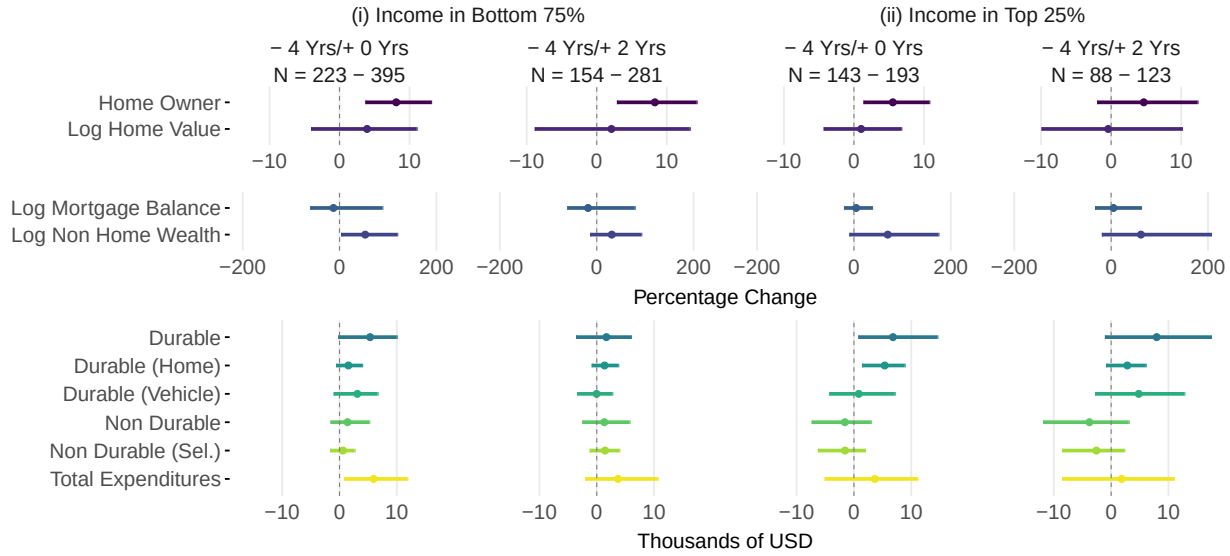
Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0 and 2 years after the transfer) with PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Only first transfers that are identified as inheritances and valued at over $50,000 (2023 USD) are included. Households are dropped in and after any year in which a second transfer is reported, or if a gift is reported in any year. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions in panel (a) control for age, marital status, income, number of children, and lagged outcome variable. Panel (b) additionally controls for race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations; data for these and durable home goods are not available before 2005. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

Figure 10: Difference-in-Differences for the Effects of Transfers By Pre-Transfer Income and Period

(a) 1997-2009



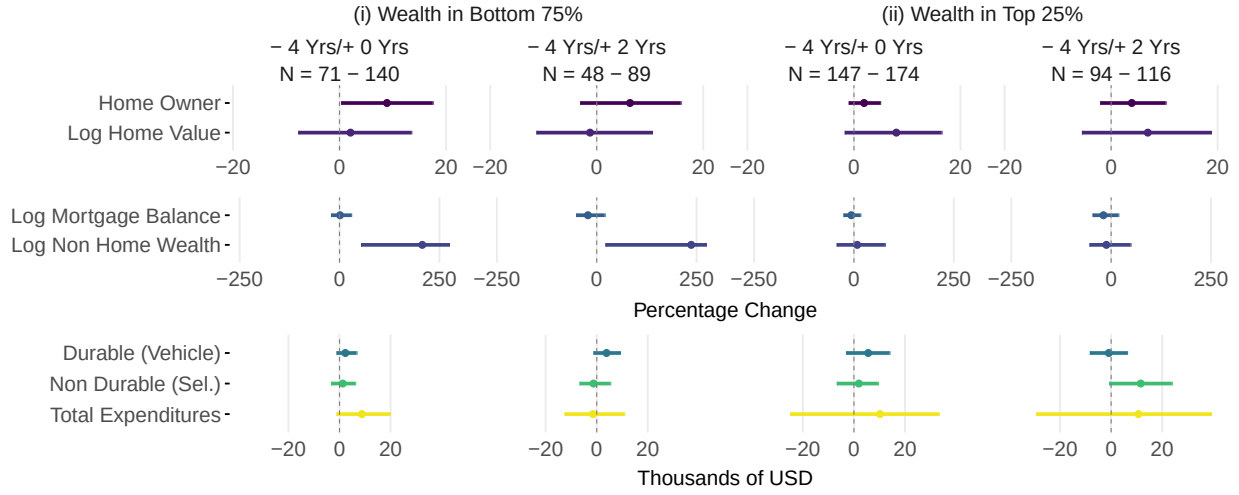
(b) 2011-2023



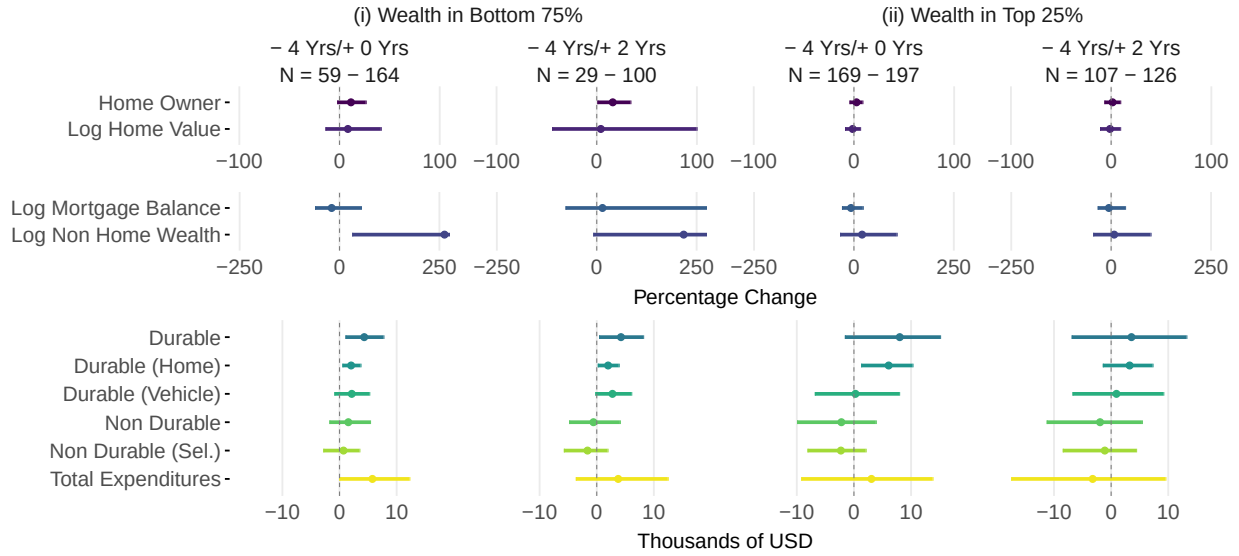
Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0 and 2 years after the transfer) with PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Households are dropped in and after any year in which a second transfer is reported. Grouped by top 25% or bottom 75% of total incomes in the year before transfer, calculated across all available PSID households. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions control for age, marital status, income, number of children, outcome variable two years prior to receipt, race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

Figure 11: Difference-in-Differences for the Effects of Transfers By Pre-Transfer Wealth and Period

(a) 1997-2009



(b) 2011-2023



Aggregated Callaway and Sant'Anna (2021) doubly-robust DiD estimates (0 and 2 years after the transfer) with PSID data. 2 years before treatment is the baseline. 95% CI plus/minus maximum pre-trend violation (deviation from zero difference in outcome between treatment and control groups in pre-treatment years). Uneven bars indicate open confidence interval (upper/lower bound could not be calculated) after allowing for pre-trend violations. Mortgage balance and non-home wealth truncated at 250%. Households are dropped in and after any year in which a second transfer is reported. Grouped by top 25% or bottom 75% of total wealth in the year before transfer, calculated across all available PSID households. N is the range of treated households; the lower number applies to mortgage balance (data for households that moved since the last survey is excluded). Regressions control for age, marital status, income, number of children, outcome variable two years prior to receipt, race (white, black) and years of education of the head of household, region of residence (South, West), homeownership status (excluded for homeownership, mortgage, and home value regressions), and size of household, as well as the lagged first difference for: head's marital status, number of children, outcome variable (excluded for non-home wealth), total income, and home value. Non-Durable (Sel.) is all non-durable goods except clothing, recreation, and vacations. Percent changes for logged variables are obtained by $(exp(\beta_{Log\ Var}) - 1) \times 100$.

References

Arias, Elizabeth, Jiaquan Xu, and Kenneth D. Kochanek. 2025. "United States Life Tables, 2023." *National Vital Statistics Reports* 74 (6). Hyattsville, MD: National Center for Health Statistics, Centers for Disease Control and Prevention: 1–63. doi:10.15620/cdc/174591.

Belloc, Ignacio, José Alberto Molina, and Jorge Velilla. 2023. "Household Wealth and Body Mass Index: Towards a Healthy Ageing?" Working Paper 1354. GLO Discussion Paper. https://www.econstor.eu/handle/10419/280282.

———. 2025. "Consumption Responses to Inheritances: The Role of Durable Goods." *Journal of Macroeconomics* 83 (March): 103661. doi:10.1016/j.jmacro.2024.103661.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2002. "How Much Should We Trust Differences-in-Differences Estimates?" w8841. Cambridge, MA: National Bureau of Economic Research. doi:10.3386/w8841.

Blickle, Kristian, and Martin Brown. 2019. "Borrowing Constraints, Home Ownership and Housing Choice: Evidence from Intra-Family Wealth Transfers." *Journal of Money, Credit and Banking* 51 (2-3): 539–80. doi:10.1111/jmcb.12566.

Board of Governors of the Federal Reserve System. 2025. "Distributional Financial Accounts (DFA): Distribution of Household Wealth in the U.S. Since 1989 — Net Worth by Age (Shares)." June 20. https://www.federalreserve.gov/releases/z1/dataviz/dfa/distribute/table/#quarter:142;series:Net/%20worth;demographic:age;population:1,3,5,7;units:shares.

Callaway, Brantly, and Pedro H. C. Sant'Anna. 2021. "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics* 225 (2): 200–230. doi:10.1016/j.jeconom.2020.12.001.

Cox, Donald. 2014. "Inheritance, Bequests, and Labor Supply." *IZA World of Labor*, September. doi:10.15185/izawol.69.

Elinder, Mikael, Oscar Erixson, and Daniel Waldenström. 2018. "Inheritance and Wealth Inequality: Evidence from Population Registers." *Journal of Public Economics* 165 (September): 17–30. doi:10.1016/j.jpubeco.2018.06.012.

Federal Reserve. 2023. "The Fed - Chart: Survey of Consumer Finances, 1989 - 2022." https://www.federalreserve.gov/econres/scf/dataviz/scf/chart/#series:Before_Tax_Income;demographic:nwcat;population:all;units:median.

Freyaldenhoven, Simon, Christian Hansen, and Jesse M. Shapiro. 2019. "Pre-Event Trends in the Panel Event-Study Design." *American Economic Review* 109 (9): 3307–38. doi:10.1257/aer.20180609.

Institute for Social Research, University of Michigan. 2023. "Panel Study of Income Dynamics: 1999 Public Release Family File Codebook." https://psidonline.isr.umich.edu/documents/psid/codebook/FAM1999ER_codebook.pdf.

Joint Center For Housing Studies of Harvard University. 2022. "The State of the Nation's Housing 2022."

https://www.jchs.harvard.edu/state-nations-housing-2022.

Joulfaian, David, and Mark O. Wilhelm. 1994. "Inheritance and Labor Supply." *The Journal of Human Resources* 29 (4): 1205. doi:10.2307/146138.

Lee, Hyojung, Dowell Myers, Gary Painter, Johanna Thunell, and Julie Zissimopoulos. 2020. "The Role of Parental Financial Assistance in the Transition to Homeownership by Young Adults." *Journal of Housing Economics* 47 (March). Academic Press. doi:10.1016/J.JHE.2018.08.002.

Luea, Heather M. 2008. "The Impact of Financial Help and Gifts on Housing Demand and Cost Burdens." *Contemporary Economic Policy* 26 (July): 420–32. doi:10.1111/J.1465-7287.2008.00106.X.

Nekoei, Arash, and David Seim. 2023. "How Do Inheritances Shape Wealth Inequality? Theory and Evidence from Sweden." *The Review of Economic Studies* 90 (1): 463–98. doi:10.1093/restud/rdac016.

Panel Study of Income Dynamics. 2025. "Panel Study of Income Dynamics: 2021 Family File Codebook." Produced and distributed by the Survey Research Center, Institute for Social Research, University of Michigan, Ann Arbor, MI. https://psidonline.isr.umich.edu/documents/psid/codebook/FAM2021ER_codebook.pdf.

Penn Wharton Budget Model. 2021. "Inheritances by Age and Income Group." *Penn Wharton Budget Model*. https://budgetmodel.wharton.upenn.edu/issues/2021/7/16/inheritances-by-age-and-income-group.

Rambachan, Ashesh, and Jonathan Roth. 2023. "A More Credible Approach to Parallel Trends." *Review of Economic Studies* 90 (5): 2555–91. doi:10.1093/restud/rdad018.

Roth, Jonathan, Pedro H. C. Sant'Anna, Alyssa Bilinski, and John Poe. 2023. "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature." *Journal of Econometrics* 235 (2): 2218–44. doi:10.1016/j.jeconom.2023.03.008.

Social Research Center. 2025. "Panel Study of Income Dynamics, Public Use Dataset." Ann Arbor, MI: Institute for Social Research, University of Michigan. Accessed July 4.

Suari-Andreu, Eduard. 2021. "Housing and Household Consumption: An Investigation of the Wealth and Collateral Effects." *Journal of Housing Economics* 54 (December): 101786. doi:10.1016/j.jhe.2021.101786.

U.S. Census Bureau. 2025. "Total Households." Washington, DC: U.S. Census Bureau. https://fred.stlouisfed.org/series/TTLHH.

U.S. Federal Housing Finance Agency. 2025. "All-Transactions House Price Index for the United States [USSTHPI]." Retrieved from FRED, Federal Reserve Bank of St. Louis. https://fred.stlouisfed.org/series/USSTHPI.

Wang, Xueqi, and Graham Squires. 2024. "Intergenerational Transfer, Parental Support and Housing: A Literature Review." *Property Management* 42 (March). Emerald Publishing: 192–234. doi:10.1108/PM-04-2023-0037/FULL/XML.

World Inequality Database. n.d. "Codes Dictionary - WID - World Inequality Database." https://wid.world/codes-dictionary.

Link to Online Appendix