Effects of Labeled Child Benefits on Family Savings

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Contrary to standard microeconomic principles, it is by now well understood that income is not fungible. For example, the label of a government transfer can induce individuals to make expenditure decisions that are skewed towards the label. In this paper, I show that child benefits are disproportionately used for savings assignable to children. I exploit a policy reform in a difference-in-differences approach to estimate the effect of child benefits on savings. The results suggest a significant positive effect on long-term savings and weak evidence for effects on child-assignable consumption. I conclude that labeling effects should be considered carefully by policy makers, if not for nudging individuals, then to avoid affecting decisions unintentionally.

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1 Introduction

A basic principle from microeconomics says: income is fungible. Fungibility means that any type of income is a perfect substitute for another and it implies that the type of income does not affect its use. Put differently, there should not be any compositional effect of income on expenditure. However, recent research shows that labeling effects often yield violations of this basic principle. Intuitively, a label attached to a transfer or income affects a consumer's perception in a way that distorts decisions towards the label. A well-known characterization of labeling effects is the mental accounting framework which suggests that individuals think of their resources as separate accounts (Thaler, 1980, 1985, 1990, 1999). Each mental account implies a different propensity to consume the respective goods. Thus, changing the relative size of mental accounts while holding the income level constant changes consumption patterns. Other explanations for violations of fungibility can be found in theories of decision framing or narrow bracketing (Tversky and Kahneman, 1981; Barberis *et al.*, 2006; Rabin and Weizsäcker, 2009). In this class of models consumers tackle small isolated decisions to solve more complex problems. Violations of fungibility can also result from reciprocity towards the bestowing party (Gouldner, 1960). Welfare recipients, then, would try to act in the interest of the institution which paid out the benefit.

Child benefits as a labeled transfer have stimulated researchers' interest in the effect on families' spending patterns.[1] The empirical literature has so far produced ambiguous results. Dutch child benefits increase expenditure on assignable children's clothes disproportionately which is clear indication of a labeling effect (Kooreman, 2000). To the contrary, in the United Kingdom no such effect is found. Instead, child benefits are spent disproportionately on adult goods like alcohol and tobacco, while the households' clothes and food expenditure is found unaffected from child benefit increases (Edmonds, 2002; Blow *et al.*, 2012). The evidence, though, is incomprehensive. Family welfare is not only determined by short-term expenditure but also by long-term savings, which have been overlooked in the literature.

In this paper, I exploit a child benefits reform to estimate the impact of a labeled transfer on child-assignable savings. Between 1978 and 1983 German child benefits were expanded for third children and to some extent for second children while they remained constant for first children. This allows me to use quasi-experimental research design in order to eliminate confounding variation that is common to all families. I identify the effect of a relative increase in labeled income from *child* benefits. If income was fungible, the income source should not affect household savings decisions. I use the German Income and Expenditure Survey (EVS) to analyze the effects on different savings measures. I distinguish between housing savings plans, securities, life insurances, and bank books. I argue that housing savings plans can be considered as child-specific savings and show suggestive evidence for the assumption. Furthermore, I analyze the effect on four child- and four adult-assignable consumption expenditures.

[1] Moreover, a larger literature investigates the effects of child benefits on other outcomes. E.g. González (2013) studies the effects of a one-time subsidy for new mothers in Spain and finds an increase in fertility and longer absence from work of mothers, but no effects on expenditure behavior.

Using difference-in-differences estimation, I find that the treatment group increases the probability to save in a housing savings plan by up to 6.6 percentage points and increases savings contributions by up to a third. The finding is consistent with a labeling effect of child benefits and hard to explain otherwise. I do not find an effect on other savings outcomes that are less connected to child welfare. Regarding consumption, I find mixed evidence. There is partly robust evidence for increases in child-assignable education and toy expenditure. I do not find evidence for effects on adult-assignable consumption. A possible explanation for the weaker evidence in the consumption domain is that families might satisfy the most basic needs out of their own incomes and allocate additional resources to more long-term investments. Eventually, the outlooks of a child's life might be more dependent on these savings than on contemporaneous consumption.

The results are robust to a number of different specifications and tests. I include comprehensive sets of control variables and find the savings results to be robust. Moreover, I apply estimations on accumulated savings, total savings and savings rates which all suggest that the baseline results are plausible. The savings results are also robust to a relative trend assumption I impose in an alternative specification of the difference-in-differences estimator. I test the plausibility of common trends in housing savings plans with two unaffected groups and find no evidence for violation of the assumption. Heterogeneity tests with respect to income yield conclusions consistent with the basic results.

For policy makers evidence on labeling effects is of particular interest as labeling cash transfers is virtually costless. My results suggest that labeling is effective to promote a desired behavior beyond consumption. Countries struggling with low private savings rates, thus, could relabel existing benefits before applying more costly measures. Furthermore, family policies in many countries involving cash transfers may shine in a new light as they are already labeled accordingly (e.g., Child Tax Credit (CTC) in the United States, Child Benefit (CB) and Children's Tax Credit (CTC) in the United Kingdom, and Child Benefits (Kindergeld) in Germany). These programs have in common the intention to mitigate financial constraints of families and to prevent child poverty which becomes ever more likely in the presence of labeling effects.

My results also contribute to a broader literature that finds support for labeling effects and mental accounting in other domains. For example, randomly allocated and non-distortionary beverage vouchers make customers of a restaurant increase their expenditure on beverage consumption which cannot be explained by standard theory (Abeler and Marklein, 2010). The perception of costs of goods can depend on the time between purchase and consumption; people perceive purchases intended to be consumed later as investments and detach the costs from consumption (Shafir and Thaler, 2006). Non-fungibility of income might also explain why increases in housing benefits are to a large extent offset by increasing rents (Cage, 1994; Susin, 2002; Fack, 2006). *Bono de Desarrollo Humano* cash transfers to women in Ecuador increase food expenditure (Schady and Rosero, 2008), which may be due to a labeling effect or to changes in the intra-household allocation in favor of women.

The remainder of the paper is structured as follows. In Section 2, I describe the empirical approach and the data. In Section 3, I report baseline results and various

robustness checks in Section 4. I conclude the analysis in Section 5.

2 Empirical Approach

Ideally, a test of labeling effects would mean that an existing transfer is relabeled to *child benefits* for randomly chosen families. Comparing the treated and untreated families would identify the causal effect of a *child* label on expenditure. Unfortunately, I have to depart from the ideal setting in two respects. First, I do not observe a relabeling but an increase in the labeled transfer. Second, the treatment does not affect a random subset of the population but families with certain characteristics. This is reflected in the empirical approach. I exploit an unanticipated German child benefits reform that affected family types differently. Child benefits are paid monthly to the parents of all under-aged children and the reforms took place from 1979 to 1982. The sample period due to data restrictions is 1978 to 1983, two cross-sections representing pre- and post-reform periods. The amount of child benefits per child is bound to the number of eligible children and increases with each additional child. This means that for the first child 50 Deutsche Mark (DEM; former German currency[2]) were paid, more for the second child and so forth. The reform between 1978 and 1983 led to the exceptional situation that the amount of child benefits for the first child remained constant whereas it increased sharply for the third child (and less so for second children). Families with three children experienced a rise in their child benefits of about 30 percent. Figure 1 illustrates the composition of child benefits by the number of children and Table 1 unravels the amounts per child. Monthly child benefits payments for first children throughout the 1970s and 1980s were 50 DEM. The government paid 80 DEM for second children in 1978 and 100 DEM in 1983. For third children parents could claim 150 DEM in 1978 and 220 DEM in 1983. In total, a family with three children would receive 370 DEM per month net transfers in 1983, which corresponds to 324 EUR in 2013 prices.[3] Child benefits were a substantial component of families' incomes; they contributed 7.4% to net income of an average family with three children in 1983.[4]

In the analysis, I compare a treatment group comprised of three-child families with a control group comprised of one-child families over time. For the treatment group child benefits increased from 280 DEM to 410 DEM in 1981 and 370 DEM afterwards, while they remained at 50 DEM in the control group.[5] In contrast to some of the earlier studies, the large reform allows the use of quasi-experimental techniques to identify

[2]Conversion rate was 1.95583 DEM per EUR.

[3]Calculated as 370 DEM multiplied with inverse value of 1983 CPI, 0.767, for base year 1995 for West Germany, which is multiplied by the inverse for 1995 CPI for Germany, 0.762, converted to EUR. Values from Statistsches Bundesamt: Preise, Verbraucherpreisindizes für Deutschland, Lange Reihen ab 1948.

[4]Calculated as fraction of average net yearly income as in the sample of three-child families, with 12 times 482 DEM, in 1995 prices, being the total yearly child benefits.

[5]In terms of income percentages, the contribution to household net income is 7.4% in 1978, rising throughout the period and due to inflation is back at 7.4% in 1983 in the treatment group. During the same time, the fraction decreased from 1.5% to 1.2% in the control group.

causal effects under relatively weak assumptions.[6] Unobserved common changes over time that constitute confounding variation are eliminated in this approach. As evident from Figure 1, a smaller treatment for two-child families materializes during the same period, but it is only modest compared to the increases for third children. Therefore, two-child families are discarded in the analysis and only used in the robustness checks.

[Figure 1 about here.]

[Table 1 about here.]

The child benefits reform was largely unanticipated as the *Law Gazette* published the new figures on November 18, 1978, the pre-reform year. Early announcement effects, i.e., a premature change in expenditure behavior in advance of the reform, would lead to downward bias in the estimates. Lump-sum child benefits are not means tested, paid monthly and available to all children up to the age of 16.

It is important for the analysis that no other policy changes affected the groups differentially at the same time as the child benefits reform. There are other family benefits that may affect family expenditure and behavior. One of the biggest is child allowances in the income tax system. In the period in question, however, these do not affect the analysis. Child allowances were not present during the period under study, although they were reintroduced in 1983 at a very moderate level. This does not affect the estimation as allowances become effective after tax return at the end of a year. The other important family policy reform during this period is the introduction of maternity leave for working mothers in 1979. This had arguably no differential impact on treatment and control groups. Changes in the regulations of child custody in 1979 and of state benefits for missing alimony payment after divorce in 1980 do not directly affect my studied subjects and certainly not differentially across groups.

2.1 Estimation Strategy

I ought to test whether savings and consumption evolve differently over time between a treatment group and a control group. The control group depicts a counterfactual that indicates how the treatment group would have evolved in the absence of a treatment. I use two distinct definitions of the counterfactual in a difference-in-differences (DD) and a difference-in-relative-differences (DRD) model.

[6] Kooreman (2000) evaluates the labeling effect by relying on a differential change in child benefits for over- and under-6 year-olds to identify marginal propensities to consume child goods. His approach relies on the rather weak identifying assumption that consumption for younger children stays proportional to consumption for older children over time. Edmonds (2002) identifies the effect of means-tested Slovenian child benefits by income variation in the previous year. The assumption of no direct effect of previous income on contemporary consumption is crucial to this study. Blow *et al.* (2012) draw on unanticipated benefit changes by using inflation- (anticipated) and reform-driven (unanticipated) variation in child benefits over time. They interpret their finding as children being insured against income shocks so that unanticipated income gains do not need to be invested in children's welfare. Lyssiotou (2009) shows some positive effects of Cypriote child benefits on child goods consumption in a difference-in-differences framework only if the mother receives the payments.

The DD model can be described as a double difference over time between the treatment and the control group. I get the intuitive expression of the treatment effect δ as in

$$\begin{aligned}\delta = &\{E(Y_{st} \mid Treated_s = 1, Post_t = 1) - E(Y_{st} \mid Treated_s = 1, Post_t = 0)\} \\ &- \{E(Y_{st} \mid Treated_s = 0, Post_t = 1) - E(Y_{st} \mid Treated_s = 0, Post_t = 0)\},\end{aligned} \tag{1}$$

where $E()$ denotes the expected value of the outcome measure Y_{st} of family s in period t. The treatment group indicator $Treated_s$ is unity for three-child families and zero for one-child families. Constrained by data availability the pre-reform period is 1978 and the post-reform period is 1983, which is depicted in the post-reform indicator $Post_t$. In the analysis, I use the regression form

$$Y_{st} = \alpha_0 + \alpha_1 Treated_s + \alpha_2 Post_t + \delta(Treated_s \times Post_t) + \epsilon_{st}, \tag{2}$$

where ϵ_{st} is an i.i.d. error term. Besides the treatment effect, the regression yields estimates for the pre-reform baseline outcome (α_0), the baseline difference between the treatment and the control group (α_1) and the common time trend (α_2). The treatment effect δ can be interpreted as an average treatment effect on the treated (ATT). The treatment effect identifies the effect of child benefits if nothing else changes differentially between the groups conditional on control variables. I estimate the model in three steps. First, I show results of a pure DD model. Second, to interpret the compositional change of the labels in the income components only I hold total household income constant by controlling for household income including child benefits. This implicitly assumes that household income is exogenous to changes in child benefits. If child benefits increases led to a depression of labor supply due to an income effect, it could attenuate the positive effect of child benefits on disposable income. Nevertheless, total household income including child benefits should increase as an overcompensating labor supply reaction is rather implausible. The result would be that controlling for full income yields a downward bias in the treatment that works against the identification strategy. I also explicitly test the income response in the robustness section. As higher income should increase the outcome variable, by controlling for income I attribute parts of the effects to the control variable that are in fact caused by the treatment.

Third, I estimate a DD model with additional control variables. The DD approach accounts for all unobserved time-constant differences between the treatment and the control group. To allow for time-varying differences I use as a robustness check an extended version of the estimation equation that includes observable family-level control variables, namely

$$\begin{aligned}Y_{st} = &\alpha_0 + \alpha_1 Treated_s + \alpha_2 Post_t + \delta(Treated_s \times Post_t) \\ &+ \beta_1 Inc_{st} + \beta_{11}(Inc_{st} \times Treated_s) + X_{st}\beta_2 + \epsilon_{st},\end{aligned} \tag{3}$$

where X_{st} are additional control variables. The additional estimators describe the influence of income (β_1) interacted with the treatment group dummy (β_{11}) and of the control variables (β_2). All estimations are carried out using ordinary least squares.

Standard errors are obtained using Huber/White/sandwich estimates that are robust to heteroscedasticity which is likely to occur in estimations of savings and expenditure as variability of the dependent variables may easily increase with income.

I assume a common counterfactual trend in the outcome variable in the absence of a treatment between the treatment and the control group. Due to data limitations, I cannot apply a standard pre-treatment test on common trends in the outcomes.[7] In the standard DD model I make a common trend assumption, i.e. both groups would have experienced the same absolute change in the outcome variable in the absence of a treatment. The treatment and control group have different family sizes and it is conceivable that their spending and saving patterns differ. The common trend assumption can be relaxed by assuming that the groups follow a common relative trend instead of an absolute trend. By this I can account for the fact that larger families have higher expenditures and may make proportional changes over time compared to smaller families. In the difference-in-relative-differences (DRD) approach, I assume that an x-% change in the outcome of the control group corresponds to an x-% change in the outcome of the treatment group. Gregg *et al.* (2009) use a similar adjustment and refer to it as the percentage method. Estimates are obtained from transformations of the regression coefficients of the standard DD model:[8]

$$\delta_{DRD} = \frac{\alpha_2 + \delta}{\alpha_0 + \alpha_1 + \beta_1 \overline{Inc_1}} - \frac{\alpha_2}{\alpha_0 + \beta_1 \overline{Inc_0}}, \tag{4}$$

where Inc_1 denotes mean income in the treatment group and Inc_0 denotes mean income in the control group. The DRD treatment effect with the full set of control variables is defined as:

$$\delta_{DRD} = \frac{\alpha_2 + \delta}{\alpha_0 + \alpha_1 + \beta_1 \overline{Inc_1} + \beta_{11} \overline{Inc_1} + \beta_2 \overline{X_1}} - \frac{\alpha_2}{\alpha_0 + \beta_1 \overline{Inc_0} + \beta_2 \overline{X_0}}, \tag{5}$$

where X_1 denotes mean control variables in the treatment group and X_0 denotes mean control variables in the control group. Estimation of the DRD coefficients are followed by t-tests using the Delta method to obtain significance levels for the percentage estimates.

An important assumption of DD models imposes that self-selection into treatment may not occur. In this setting there are in principle two channels of self-selection: take-up and fertility. It is very unlikely that treatment group families would not claim child benefits if they are eligible, because institutional hurdles are very low. There is no explicit cost attached to the application which has to be done once in a life-time of a child. Moreover, there are no indications of a social stigma to receive child benefits. Thus, take-up of the transfer is unlikely to be a problem. More of a concern could be strategic fertility. If families with two children decide to have a third child because they want to benefit from the increase in child benefits, they might alter the composition of the treatment group and impose an estimation bias. In Figure 2 I report trends of first, second and third births as a fraction of all births over time. Birth composition

[7] See Data section for more information on the data source.

[8] See appendix for details.

was remarkably stable over the period from 1975 to the end of the treatment period in 1983. The fraction of third births was practically constant. Thus, there is no evidence of selective fertility that would bias the composition of the treatment group of families with three children after treatment.

[Figure 2 about here.]

2.2 Data

In the empirical analysis I employ two consecutive cross-sectional waves of the German Income and Expenditure Survey (EVS: Einkommens- und Verbrauchsstichprobe) from 1978 and 1983. The EVS is a representative survey of about 45,000 households that is conducted every five years, starting in 1978.[9] The data is a 98 percent sample of the full survey. The data include a complete set of expenditure and income variables at the household level. Some of the more detailed expenditure, e.g. food, are measured as a sum over four weeks. Less detailed expenditure and income information is collected as the sum over one year.

Some sample selection criteria were needed to obtain a conceivable data set which features 11,754 observations. Families are only included if they have children up to the age of 16 in the household. This ensures that all children in the households are eligible for receiving child benefits. I exclude households that report negative incomes (1 instance). I exclude families with more than two earners such that earning children are not included (3,893). Families with the oldest child being younger than three years are excluded to get more comparable family types (2,742). I also exclude families who report that the second child is older than the first child and the same logic applies to third and fourth children. This is to exclude wrongly answered questionnaires (56). In total, I exclude 6,607 observations from the main sample (85 sample restriction violations occurred in families with more than one violation). The main conclusions are not sensitive to the sample restrictions.

Group assignment variables Assignment of households to the treatment and the control group are based on the number of children reported in the household. I assign households to the treatment group if three children live in the household. Families are assigned to the control group if one child lives in the household. Possible eligible children living outside the household cannot be identified in the data. The stable unit treatment value assumption (SUTVA) might be violated by cases where families are eligible to larger benefits than it is accounted for. This could mean that families with two or more eligible children are assigned to the control group and that families with four or more eligible children are assigned to the treatment group. In the former case I would expect a downward bias in the estimates. The latter case would yield an upward bias. Low prevalence of families with four or more children suggests that a downward bias is the more likely case. The treatment group indicator variable $Treated_s$ takes on a value of

[9] The first survey was undertaken in 1962/1963. The second survey from 1973 is unavailable as it has not been digitized. The regular five year interval surveys, thus, begin in 1978. Sample size increased in the meantime to about 60,000 households in recent waves.

unity for the treatment group and zero for the control group. I will alter the assignment in later robustness tests.

2.3 Dependent variables

The main outcome variables are savings. I explore a summary measure of savings contributions and the four by far most important forms of savings separately. Special attention is paid to savings that are relatively beneficial for children and thus partly assignable, namely housing savings plans (HSPs). HSPs are bundled financial products that combine savings plans and mortgage loans. So-called Bausparkassen, financial institutes that work separately from banks and other financial markets, exclusively provide HSPs. The usual mechanism of an HSP is that over at least seven years a predefined sum of money is saved by the contract-holder. When the predefined sum is accumulated the HSP entitles the contract-holder to receive a loan from the Bausparkasse to purchase a home. Both the savings and the loan are associated with interest rates typically below the market rate (Deutsch and Tomann, 1995; Scholten, 2000; Plaut and Plaut, 2004). Thus, in return of the foregone interest in the savings period the contract-holder receives preferential terms in the loan period. Despite developed financial markets and the low interest rates on savings, HSPs are widespread in Germany, Austria and European transition economies but less so in North America (Plaut and Plaut, 2004).

It can be argued that HSPs compared to other forms of savings are more likely to benefit children. First, savings in HSPs are explicitly of a long-term nature and bear low levels of risk. Second, the savings are expected to yield a purchase or construction of a house as the main benefit is given via favorable conditions of building loans. Children may benefit from real estate in two ways: higher living standards during childhood or an inheritance later in life. About half of the volume of inheritances is real estate[10] and the relevant parent cohorts in the data set will soon start to bequeath their houses. In a representative survey, 75 percent of bequeathers plan to pass their wealth on to their children and two thirds of inheritances will include real estate.[11] Third, HSPs are the most popular form of savings for children in Germany even for other means than house purchases. In a representative survey about the most suitable form to save for children HSPs ranked highest among different forms of savings,[12] which is consistent with anecdotal evidence and explicit advertising as saving devices for children. HSPs can be terminated and paid out when larger amounts of money are needed for a child, e.g. for tertiary education. A family can also have several contracts for each child at the same time. Börsch-Supan and Stahl (1991) report that the probability to save in an HSP decreases with age, increases with the number of children and is higher for home owners than for renters. This observation is consistent with the notion that young adult people have received an HSP from their parents and that parents with more children save more in HSPs for them. Moreover, saving in HSPs for home owners, with almost twofold

[10] See survey "Erben in Deutschland", Deutsches Institut für Altersvorsorge, Cologne 2011.

[11] See survey based on 1,613 participants conducted by Institut für Demoskopie Allensbach: "Postbank Erbschaftsstudie 2012", Postbank.

[12] See survey of 1,600 participants in "comdirekt Spar- und Konsumindex", comdirekt Bank AG, 2014.

the propensity of renters, makes more sense as savings for children than as savings for purchasing a second home. As HSPs are not widespread or unavailable in most countries, this raises concerns about external validity. For the purpose of this study they could be understood as an indicator of long-term savings that could take on different forms in other countries.

A crucial assumption in DD analysis is that no differential change in the outcome variable occurs at the same time as the treatment, i.e., there may not be another reform that affects one- and three-child families between 1978 and 1983 differentially. HSP legislation was not subject to such changes. The state subsidized HSPs heavily in the 1970s and 1980s by mainly two instruments: bonus payments (Wohnungsbaupraemie) and tax deductions (Sonderausgabenabschreibung). Bonuses were paid to low-to-middle income households in the magnitude of 18 percent in 1978 and 14 percent in 1983. The overall reduction in bonuses affected both the treatment group and the control group in the exact same way (Börsch-Supan, 1994). Also, an extra 2 percentage point bonus per child was paid in 1978 and in 1983 likewise. Tax deductions are independent of the number of children. HSPs are also eligible to subsidies from the wealth accumulation program (Vermoegensbildungsgesetz). The rates were constant until 1982 and were reduced in 1983 (Börsch-Supan, 1994), but equally for both the treatment and control group.

The other three significant forms of savings are bank books, securities and life insurance. Bank books are a general form of savings that can take on almost any duration and are characterized by low interest rates and low risk. Securities can yield higher interest on investment but are coupled with higher risk. Life insurances usually have very long durations and partly insure the family against income default. I assume these savings forms to be either adult-assignable or unassignable. Bank books and life insurance were eligible for subsidies from the wealth accumulation program (Vermoegensbildungsgesetz) just as the HSPs if the savings are held in the deposit for at least six years and the household is below a certain income threshold that can be considered low middle-class. Subsidy rates were constant until 1982 and were equally reduced for both the treatment and control group in 1983 (Börsch-Supan, 1994). An additional premia for long-term bank savings (Sparpraemie) was in place in 1978 and had been removed by 1983.

In a second step, I explore consumption expenditure. Apart from a summary measure of total consumption I explore each four child- and adult-assignable consumption goods separately. Child-assignable consumption includes education expenses, musical instruments, books, and toys, whereas adult-assignable consumption includes alcohol and tobacco, restaurant visits, amusement services and luxury goods. Education expenses are comprised of child care costs in public or private centres from early child care to after-school care, fees for educational institutions, costs of coaching and charges for other courses. Education expenses can therefore be regarded as child-assignable. Expenses for musical instruments and books are less clearly assignable, but their educational value could benefit children. Expenses for toys are most likely assignable to children. Alcoholic beverages and tobacco products are clear adult-assignable goods. Although restaurant expenditures also include food from canteens, a larger fraction of the expenditure is likely assigned to parents. Amusement expenditure includes the-

atre, cinemas and sport events, such that they are mostly relevant for adults. Luxury goods include jewelry, watches, leather bags and products, smoker products and funeral expenses. Thus, luxury goods are adult-assignable.[13]

2.4 Control variables

The DD framework eliminates confounding variation that is common to both one-child and three-child families. Only changes over time that affect the groups differently and occur at the same time as the benefit change will cause problems in this setting. One possible reason could be changes in the age composition of children in the two groups. Rapidly changing fertility rates in the early 1970s likely yield changes in children's age in the cross-sections of 1978 and 1983. And, possibly, families with three children experience different changes to age compositions from one-child families. Moreover, the effect on the outcome could be different between the groups. Therefore, I ought to control for age composition in the family. I use indicator variables for the age of the oldest child to account for different age compositions post reform with 16 years as the omitted category. Moreover, I allow for differential effects of child age in the treatment and the control group by interacting age indicators with the treatment group indicator. This might especially be relevant if younger children could reuse some of the goods purchased for the first child. Focusing on the oldest children is straightforward in the sense that first-time purchases have the largest impact on consumption patterns.

As the child benefits reform is likely to increase household income, I explore this channel in more detail. First, I test whether the reform significantly increases household income or only leads to compositional changes within income sources. In order to exclude a possible direct income effect on savings and expenditure I control for net household in a specification of the DD model. This is to exclude the income channel from the reform effect. I use full household income, including child benefits, as a control variable because I need the full disposable income to be held constant. Under the assumption that I can control income effects fully, the remaining treatment is a pure compositional change in income sources towards the labeled transfer. Moreover, regular income could increase differently between the two groups and induce confounding variation. The effects of income on expenditure could also be different for the treatment and the control group. Therefore, I interact income with the treatment group indicator to account for possible differential income effects between the treatment and the control group and, moreover, include a squared term.

Other variables correlated with the outcomes could vary between pre- and post-reform periods. Therefore, I include background characteristics of the two groups that could violate identifying assumptions. The oldest child's sex controls for differential treatment. I include the age of both parents, as consumption patterns could vary over the life-cycle. As female labor force participation changed substantially during the study period, I also include an indicator for the number of earners to account for intra-household allocations.

[13] Unfortunately, clothes expenditure data can at best be matched by gender but not by children and adults. Clothes expenditure, although a classical example of assignable goods, is therefore excluded from the analysis.

Furthermore, I control for indicator variables of the federal states with Schleswig-Holstein as the omitted category. An indicator variable for tenant status is included to account for non-monetary wealth. Despite all careful handling of the DD assumptions, I can never fully exclude that unobserved time-variant group specific heterogeneity confounds the results.

2.5 Descriptive evidence

[Table 2 about here.]

Means of the dependent variables are reported in Table 2 for one- and three-child families, which represent the control group and the treatment group. I have 8,656 family-year observations in the control group and 3,098 family-year observations in the treatment group. The sample is representative for West Germany before reunification; all monetary values are yearly figures, denoted in then used Deutsche Mark (DEM), which had a nominal exchange rate of 1.95583 to 1 Euro, and are deflated by CPI with base year 1995. Significance levels at variable names indicate whether the means in the base year 1978 are statistically different between the treatment and the control group. Most variables are different between the groups in the base year, which highlights the importance of applying the DD strategy and adding control variables.

From the group means over time, I get a preview of the difference-in-differences results without control variables. Prevalence of HSPs increases in the control group from 56 to 62 percent, whereas it increases from 64 to 76 percent in the treatment group, suggesting a positive treatment effect. Yearly contributions to HSPs, including 0s if none are made, are decreased in the control group from 2,776 DEM to 2,453 DEM, while they increase strongly from 2,834 DEM to 3,389 DEM in the treatment group. These results are illustrated graphically in Figure 3. About 95 percent of families in the control and the treatment group own bank books. Over time there is only a small decrease in prevalence of 0.7 (control group) and 1.0 (treatment group) percentage points. Contributions to bank books decrease in both groups, from 6,741 DEM to 5,806 DEM in the control group and from 6,443 DEM to 5,698 DEM in the treatment group. I observe a parallel increase in the prevalence of securities from 32 to 36 percent (control group) resp. from 29 to 34 percent (treatment group). Over time, a sharp increase in contributions to securities is observed in both groups, from 825 DEM to 1,668 DEM (control group) resp. from 821 DEM to 1,585 DEM (treatment group). Life insurance prevalence is stable over time in both groups at high rates of 85 resp. 88 percent, with contributions increasing at comparable rates in both groups: from 1,605 DEM to 1,837 DEM (control group) resp. from 2,196 DEM to 2,375 DEM (treatment group). Overall, HSPs are the only saving devices that clearly diverge over time between the treatment and control group. When I look at accumulated wealth in these saving devices,[14] I can confirm this tendency. The accumulated value of HSPs is stable in the control group around 10.8k DEM, while it increases from 11.1k DEM to 13.6k DEM in the treatment group. Accumulated savings in bank books decrease in both groups, from 15.1k DEM to 11.2k DEM in the control

[14]No data for life insurance.

group and from 14.4k DEM to 10.0k DEM in the treatment group. The value of securities increases from 5.6k DEM to 13.4k DEM in the control group and from 8.0k DEM to 14.4k DEM in the treatment group.

[Figure 3 about here.]

Consumption expenditure for musical instruments, books, alcohol and tobacco, amusement and luxury goods developed similarly over time between the treatment and control group. In contrast, expenditure for education increased from 386 DEM to 566 DEM in the control group, but from 680 DEM to 980 DEM in the treatment group. Similarly, expenditure for toys increased in the treatment group from 353 DEM to 418 DEM while it was virtually constant in the control group around 240 DEM per year. Conversely, expenditure for restaurants decreased more in the treatment group than in the control group, from 2,404 DEM to 1,934 DEM (treatment group) and from 2,681 DEM to 2,334 DEM (control group). In the raw data I thus observe a partial shift in consumption expenditures towards child related goods and away from adult related goods in the treatment group compared to the control group.

Total savings increase from 12.1k DEM to 13.0k DEM in the control group and from 12.5k DEM to 14.7k DEM in the treatment group.[15] Total consumption expenditure per year is quite stable in both the control and the treatment group at around 53k DEM resp. around 61k DEM. Household net income increases in the control group from 65.2k DEM to 67.0k DEM and from 75.5k DEM to 78.4k DEM in the treatment group. Living space in sqm increases in both groups by 6 resp. 7 sqm, while the fraction of families renting a flat decreases in both groups. Gender of the oldest child is female in 49 percent of the families in all groups and periods. Age of the father is around 40 years in both groups, only in the post-reform period fathers in the treatment group are somewhat younger with 39 years of age. Mothers' age is equivalently three years younger than fathers' age in all cells. The number of earners in the control group is around 1.6 and around 1.4 in the treatment group, suggesting lower labor market attachment of mothers in larger families. Additional controls as categorical variables, age dummies for the children and dummies for federal states, are omitted here. Overall, the comparable figures suggest that compositional changes within and between groups are moderate.

3 Baseline Results

In what follows, I show results of the estimations as described in section 2. I begin with baseline results for savings and show effects on consumption goods before I continue with robustness checks in the next section.

3.1 Effects on savings

In Table 3 I report results from DD estimations as defined in equations 2 and 3 for the treatment effect of the child benefits increase on family savings. Panel A depicts pure

[15] Total savings contributions per year include the four savings categories mentioned before and other savings.

DD results, Panel B shows results with income controls and Panel C shows results from the fully specified model. In column (1) I see that the treatment effect on the probability to save in an HSP is 6.6 percentage points, which is statistically significant at the one percent level. Including income controls yields a treatment effect of 6.2 percentage points, also significant at the one percent level. Including all control variables reduces the effect to 4.5 percentage points. This result is significant at the five percent level. Moreover, including all control variables vanishes the baseline difference in the outcome between the treatment group and the control group, suggesting that the control variables are effective in reducing differences between the groups. Turning to the yearly contributions to HSPs in column (2) the treatment group saves an additional 878 DEM in the baseline model. The effect is highly statistically significant and economically significant as well. This translates to an increase of a third of the baseline contribution. The treatment effect in Panel B with income controls is 824 DEM, and highly statistically significant. It reduces to 708 DEM in Panel C and stays statistically significant at the five percent level.

In contrast to savings in HSPs, I do not find any significant treatment effects on savings in bank books, securities or life insurance. Estimates of treatment effects for saving in any of these devices and for the value of the yearly contributions are close to zero and standard errors are very large, irrespective of the model specification. While savings in the conservative, low-risk HSPs are increased, other forms of savings and more risky assets as securities develop equally between the treatment and control group. This result is consistent with labeling effects of child benefits as only HSPs are child-assignable.

[Table 3 about here.]

In order to verify whether the effect on the yearly contribution has a profound impact on the accumulation of savings, I explore the stock value of the savings accounts.[16] As the treatment spans over several years before the post-reform observation, this test also makes sure that the treatment effect is not simply picking up a short-term fluctuation. Table 4 reports the results on the stock values. As before, the effects on HSPs are highly statistically significant in two specifications and at least significant at the five percent level. The treatment effect in the pure DD model in Panel A is 2,579 DEM. The effect is sizable as it increases the baseline value by 23 percent. Again, the treatment effects on the stock value of bank books and securities are statistically insignificant. Note, however, that the point estimates for both measures are negative. The negative value for securities in Panel A of -1,386 DEM exceeds the negative estimate for bank books of -517 DEM. Although I cannot draw strong conclusions from insignificant results, the whole set of estimates from stock values is consistent with a shift from high-risk to low-risk savings in the treated group compared to the control group.

[Table 4 about here.]

[16] Only HSPs, bank books and securities stock values are available.

3.2 Effects on consumption

The rationale of a labeling effect—the marginal propensity to consume particular goods out of child benefits is different from the marginal propensity to consume out of regular income—also applies for assignable consumption goods. Table 5 shows the results of DD estimations on child- and adult-assignable consumption goods. I find a positive and just significant treatment effect on education expenditure with an increase of 120 DEM. With income controls the result holds in Panel B. When including all controls, the point estimate decreases to 80 DEM and loses statistical significance. A similar pattern is observed for toy expenditure. The treatment effect of 68 DEM is significant at the five percent level in Panel A. When including income controls, it stays significant at the ten percent level and is comparable in size. However, with all control variables the point estimate drops to 17 DEM and is statistically insignificant. I do not find any significant treatment effect on expenditure for musical instruments or books. In sum, there is some evidence for increases in child-assignable consumption goods, although the results are not fully robust. The effect sizes from Panel A are economically significant and correspond to 18 percent (education) and 19 percent (toys). Turning to assignable adult goods, I see no effect on alcohol and tobacco expenditure. Point estimates for restaurant expenditures are negative, but not statistically significant in any specification. Expenditure for amusement and luxury goods is not affected either. Expenditure data suggests that increases in child benefits can increase child-assignable consumption expenditure. An inverted effect on adult-assignable goods cannot be found.

[Table 5 about here.]

4 Robustness checks

In this section I test the robustness of the baseline savings results. I start with exploring effects on household income, total savings and total consumption. Then, I explore possible alternative explanations in living arrangements of families, I estimate effects in the alternative DRD model and test for effects on savings rates. Moreover, I test for income heterogeneity in the treatment effects and disentangle the HSP effects by family size and employ a simple placebo exercise.

4.1 Income and total expenditure

In order to elicit the overall effect on family finances, I explore the treatment effect on household income, total savings and total consumption. In column (1) of Table 6 I see that the treatment effect for net household income is statistically insignificant. The point estimate is positive at 1,119 DEM and somewhat larger than the treatment which corresponds to price-adjusted values between 444 DEM and 1,068 DEM yearly.[17] When including extra control variables the treatment effect is still statistically insignificant. I

[17] The difference in child benefits between treatment in control before and after the treatment in 1995 prices, with 370 DEM or 410 DEM as the treatment group value.

cannot be certain that income actually increased due to the reform, but the results seem consistent with a non-negative effect on income. Results for total savings contributions in columns (3) and (4) show a similar pattern. The treatment effect is statistically insignificant with positive point estimates between 596 DEM and 1,314 DEM. If there is no effect on total savings, the increase in HSP contribution must be compensated by a decrease in other savings forms. However, the insignificant estimates are consistent with an increase in total savings as well. Total consumption, as depicted in columns (5) and (6), is entirely unaffected by the reform. Both estimates are close to zero and are statistically insignificant. In sum, the results suggest that child benefits might have increased household income and total savings. However, none of the results is statistically significant and no robust conclusion can be drawn from the wide confidence intervals.

[Table 6 about here.]

4.2 Exploring savings results

An alternative explanation for increases in HSP savings that goes against the notion of child-related savings could be that parents try to invest in their own house. Then, HSP savings might only be an accompanying effect and I should see a parallel move to larger flats and more home owners from those families who have gathered enough capital already. In Table 7, I regress living space in square meters and an indicator for being a renter on the treatment to test for changes in accommodation conditions due to the reform. All treatment effects in any specification are indistinguishable from zero, indicating that the additional child benefits are not used for immediate housing investments. It does not seem to be the case that the reform led to a surge for larger homes that could explain the increase in HSPs. This conclusion is backed up by regressions of the savings outcomes for the subgroup of home owners. If the investment in HSPs would be intended to buy a house for the parents, I should not see this savings pattern among home owners. However, I find similar patterns for effects on savings in a sample restricted to home owners.[18] Probability to hold HSPs and contributions to HSPs increase significantly, while none of the other savings measures shows a significant effect.

[Table 7 about here.]

In Table 8 I show results from DRD estimations that relax the equal trend assumption of the regular DD to require an equal relative trend in the outcome between the control group and the treatment group in the absence of a treatment. Estimations are carried out according to equations 4 and 5, such that the marginal effect results have to be interpreted as percentage changes from the baseline. Estimates reveal that the treatment effect on saving in an HSP is 8.8 percent with income controls and 6.2 percent with full controls in terms of the baseline level. Both estimates are statistically significant, the latter one only at the ten percent level. Treatment effects on the HSP contribution are

[18] Tables are available from the author upon request.

28.9 percent with income controls and 24.6 percent with full controls. Both estimates are statistically significant at the one percent level. Treatment effect estimates on the other saving measures, bank books, securities and life insurances are altogether statistically insignificant and close to zero.

[Table 8 about here.]

In Table 9 I report results on savings rates as contributions to different savings measures as percentages of household income. The treatment effect on the savings rate of HSPs is about one percentage point. The estimate is highly significant and stable over all three model specifications. The effect is sizable compared to the baseline savings rate in HSPs of less than four percent and the total savings rate of 16 percent. Also, this result is consistent with the estimates from the DRD model. Treatment effects on savings rates in other modes of savings cannot be found. All estimates for savings rates in bank books, securities and life insurance are statistically insignificant with point estimates close to zero in all specifications.

[Table 9 about here.]

In sum, DD and DRD estimations consistently reveal that after the treatment, an increase in child benefits, parents save more in long-term HSPs instead of in regular bank books or life insurances or more risky securities. The result is consistent with a labeling effect of child benefits that favors savings which benefit children.

4.3 Income heterogeneity

One of the main objectives of child benefits is child poverty avoidance and, hence, effect heterogeneity with respect to family income is particularly relevant. I investigate income heterogeneity of the treatment effect by splitting the groups into above- and below-median income families. Estimation is carried out using a triple interaction of the treatment effect with a low-income indicator. Table 10 shows the treatment effects separately for high and low income families. The effect size on the probability to save in HSPs is 5.7 percentage points for high income households and 6.3 percentage points for low income households in estimations with income control variables. Both estimates are statistically significant at the five resp. the ten percent level. However, they are statistically indistinguishable from each other.[19] With the full set of control variables, the effect sizes are reduced to 4.7 and 4.1 percentage points, where the effect for low income families loses statistical significance. Thus, the treatment effect on saving in an HSP is not restricted to low or high income families. Estimations in Panel A for the contributions to HSPs yield effect sizes of 1,134 DEM for high income households and 668 DEM for low income households. Both estimates are statistically significant. Although the effect sizes point to a differential effect between high and low income household,

[19] None of the estimates is statistically distinguishable between high and low income household. Therefore, test statistics are not reported.

statistically the estimates are indistinguishable. When including all control variables in Panel B the pattern of results is stable and point estimates in both groups slightly decrease. There are no significant treatment effects for other forms of savings. From this exercise I can conclude that there is only weak evidence for income heterogeneity in HSP savings.

[Table 10 about here.]

4.4 Disentangling the effect

In Table 11 I disentangle the estimated effect on HSPs by family size. In columns (1) and (2) I start with a DD model where families with one child form the treatment group and childless couples are the control group.[20] This constitutes an imperfect placebo treatment exercise as none of the groups was affected by the reform, although childless couples might not produce a valid counterfactual for families. Results on the probability to save in an HSP and the effect on the contribution are statistically insignificant. This holds for both specifications and suggests that the DD model assumptions hold. However, this does not make up for a pre-treatment common trend test which is not applicable with this data as explained earlier. Moving to the disentangling between families of different size, I have seen that also the second child enjoys a moderate increase in child benefits. In other words, the treatment is lower for families with two children and, thus, I would expect a smaller effect on HSPs. In columns (3) and (4) I see the effect of a treatment group with two children compared to a control group with one child. The estimate for the probability to save in an HSP is 2.1 percentage points and statistically insignificant. This is true for the estimate with income controls in Panel A and with full controls in Panel B. Looking at the the yearly contribution to HSPs I find a statistically significant effect of 402 DEM with income controls and 363 DEM with full controls.

In columns (5) and (6), I report results for a treatment group of families with three children and a control group of families with two children. This is an artificial setting, because both groups are treated. The artificial treatment effect on saving in an HSP is 4.3 percentage points and statistically significant at the five percent level with basic controls. Including all controls yields an effect of 3.4 percentage points that is statistically significant at the ten percent level. Effects on HSP contributions are not statistically significant and range from 427 DEM to 347 DEM. If I compare these estimates with the baseline results reported again in columns (7) and (8), about two thirds of the treatment effect on the probability to save in HSPs can be attributed to the switch from two- to three-child families, only one third is attributable to two-child families. Treatment effects on HSP contributions can be divided equally.

[Table 11 about here.]

[20] The husband's age is restricted to 55 years of age to make them comparable to parents and as possible to exclude parents whose children already moved out.

5 Conclusion

The results show that an unanticipated reform of child benefits increases long-term savings in housing savings plans. The finding is consistent with labeling effects as housing savings plans are likely to be to the benefit of children. Although I cannot show a direct test of the mechanism, the literature on labeling effects seems to support that this is the most likely channel. Using difference-in-differences estimation, I find that the treatment group increases the probability to save in an HSP by up to 6.6 percentage points. Furthermore, average yearly contributions to HSPs are increased by up to 878 DEM, yielding an increase in the accumulated value by up to 2,579 DEM. The treatment effects are also economically significant and as HSPs are beneficial for children, I conclude that the effect of the labeled transfer substantially increases welfare of the average child. However, the fact that I find more robust effects for high income families casts at least doubt on the claim that child benefits are effective at reducing child poverty.

Partly in line with earlier work, I find some evidence for positive effects of child benefits on child-assignable consumption expenditure. Education expenses, including child care, and toy expenditure are increased. In contrast to evidence from Blow *et al.* (2012) I find no evidence for disproportionately high spending on adult-assignable goods like alcohol and tobacco.

The increasingly popular idea of libertarian paternalism or "nudging" as framed in the famous book by Thaler and Sunstein (2008) receives additional support for long-term savings. Policy makers willing to make use of it may see new routes to affect behavior in a gentle way. Surely, nudging is not without controversy and opponents of the idea have strong arguments. But even if policy makers want to exclude affecting the free will of individuals by policies, one should be aware of labeling effects that can have unintended consequences.

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A DRD modification

I want to impose that the time trend between the treatment and the control group are equal relative to the baseline level of the respective group instead of equal in absolute terms. This is equivalent to dividing the first difference by the pre-reform value. The standard difference-in-differences (DD) formulation

$$\begin{aligned}\delta = &\{E(Y_{st} \mid Treated_s = 1, Post_t = 1) - E(Y_{st} \mid Treated_s = 1, Post_t = 0)\} \\ &- \{E(Y_{st} \mid Treated_s = 0, Post_t = 1) - E(Y_{st} \mid Treated_s = 0, Post_t = 0)\},\end{aligned} \tag{6}$$

then becomes difference-in-relative-differences (DRD) case

$$\begin{aligned}\delta = &\frac{\{E(Y_{st} \mid Treated_s = 1, Post_t = 1) - E(Y_{st} \mid Treated_s = 1, Post_t = 0)\}}{E(Y_{st} \mid Treated_s = 1, Post_t = 0)} \\ &- \frac{\{E(Y_{st} \mid Treated_s = 0, Post_t = 1) - E(Y_{st} \mid Treated_s = 0, Post_t = 0)\}}{E(Y_{st} \mid Treated_s = 0, Post_t = 0)}.\end{aligned} \tag{7}$$

In the fully specified model including control variables we estimate the regression form of the standard DD as in

$$\begin{aligned}Y_{st} = &\alpha_0 + \alpha_1 Treated_s + \alpha_2 Post_t + \delta(Treated_s \times Post_t) \\ &+ \beta_1 Inc_{st} + \beta_{11}(Inc_{st} \times Treated_s) + X_{st}\beta_2 + \epsilon_{st}.\end{aligned} \tag{8}$$

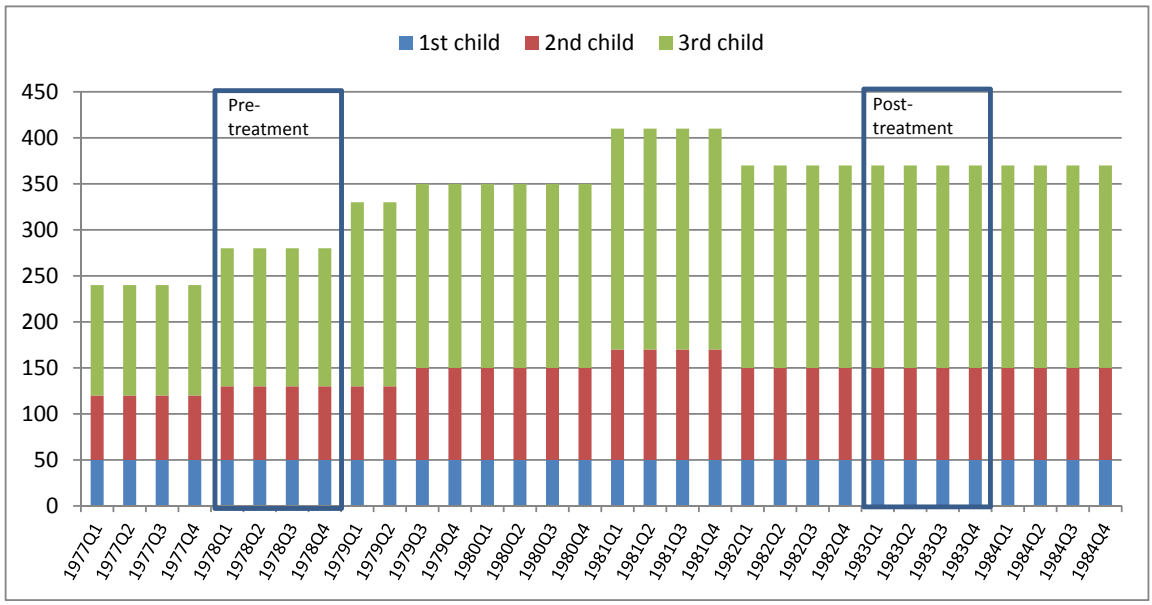
Now I can identify the expected outcomes from the regression coefficients and rewrite the DRD treatment effect as

$$\begin{aligned}&\delta_{DRD} = \\ &\frac{\alpha_0 + \alpha_1 + \alpha_2 + \delta + \beta_1\overline{Inc_1} + \beta_{11}\overline{Inc_1} + \beta_2\overline{X_1} - (\alpha_0 + \alpha_1 + \beta_1\overline{Inc_1} + \beta_{11}\overline{Inc_1} + \beta_2\overline{X_1})}{\alpha_0 + \alpha_1 + \beta_1\overline{Inc_1} + \beta_{11}\overline{Inc_1} + \beta_2\overline{X_1}} - \\ &\frac{\alpha_0 + \alpha_2 + \beta_1\overline{Inc_0} + \beta_2\overline{X_0} - (\alpha_0 + \beta_1\overline{Inc_0} + \beta_2\overline{X_0})}{\alpha_0 + \beta_1\overline{Inc_0} + \beta_2\overline{X_0}},\end{aligned} \tag{9}$$

which simplifies to

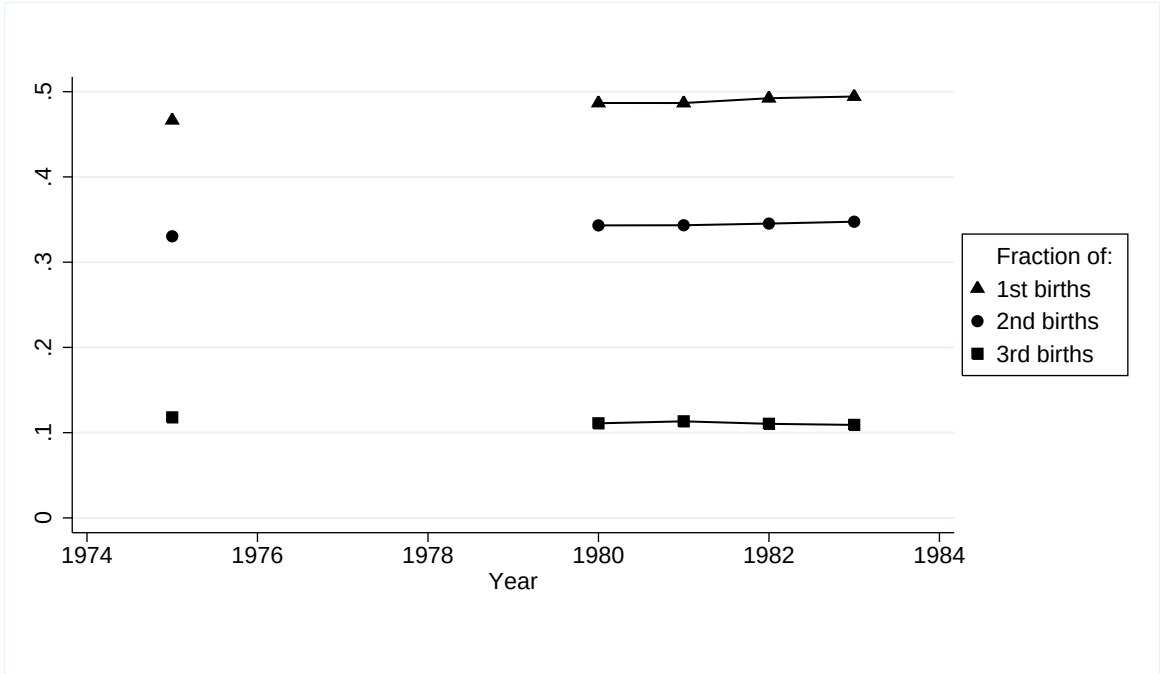
$$\delta_{DRD} = \frac{\alpha_2 + \delta}{\alpha_0 + \alpha_1 + \beta_1\overline{Inc_1} + \beta_{11}\overline{Inc_1} + \beta_2\overline{X_1}} - \frac{\alpha_2}{\alpha_0 + \beta_1\overline{Inc_0} + \beta_2\overline{X_0}}. \tag{10}$$

Figure 1: The policy reform – Child benefits



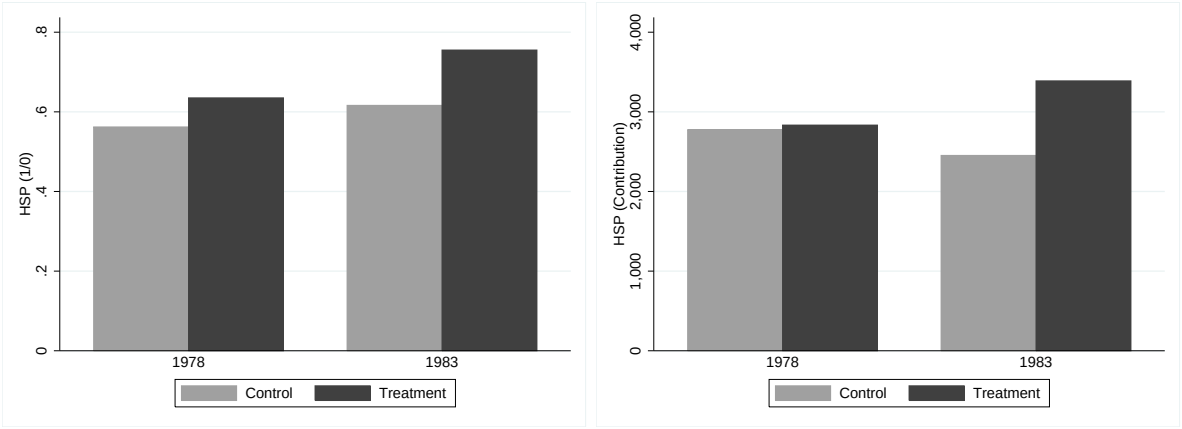
Notes: The figure depicts monthly child benefits for the first, second and third child. The full bar denotes child benefits of a family with three children. The marked bars denote the pre- and post-reform periods.

Figure 2: Fertility patterns by birth order



Notes: The figure shows the fraction of first, second and third births over all births for West German married couples. Data source: own calculations based on birth register information from Statistisches Bundesamt, Fachserie 1, Reihe 1.1, 2010, Table 2.9. Data is available for 1975, 1980, 1981, 1982 and 1983.

Figure 3: Graphical illustration of DD result for HSP



Notes: Bars represent means of the outcome variables for the treatment and control group in pre- and post-reform periods. The left figure shows means of the probability of savings in HSPs. The right figure shows means of HSP contributions.

Table 1: Monthly child benefits per child in Deutsche Mark (DEM)

In effect from...	...to	1st child	2nd child	3rd child	4th child	5th child
01-01-75	31-12-77	50	70	120	120	120
01-01-78	31-12-78	50	80	150	150	150
01-01-79	30-06-79	50	80	200	200	200
01-07-79	31-01-81	50	100	200	200	200
01-02-81	31-12-81	50	120	240	240	240
01-01-82	30-06-90	50	100	220	240	240

Notes: Child benefits per month per child in DEM in Germany in the respective period. Child benefits are paid in cash for all children until the age of 16 and for older children if they are still in school.

Table 2: Descriptive statistics – Means by family size and period

Family size:	1 child	1 child	3 children	3 children
	Pre-reform (1978)	Post-reform (1983)	Pre-reform (1978)	Post-reform (1983)
Savings				
HSP (1/0)***	0.562	0.616	0.635	0.755
HSP (Contribution)	2,776	2,453	2,834	3,389
Bank book (1/0)	0.959	0.952	0.957	0.947
Bank book (Contribution)	6,741	5,806	6,443	5,698
Securities (1/0)**	0.316	0.356	0.287	0.339
Securities (Contribution)	825	1,668	821	1,585
Life insurance (1/0)***	0.851	0.848	0.877	0.877
Life insurance (Con.)***	1,605	1,837	2,196	2,375
HSP (accum.)	10,825	10,742	11,055	13,550
Bank book (accum.)	15,102	11,175	14,425	9,980
Securities (accum.)***	5,577	13,364	7,996	14,397
Expenditure				
Education***	386	566	680	980
Music. instruments***	105	94	220	202
Books***	343	324	474	436
Toys***	240	237	353	418
Alc. & Tobacco***	2,282	2,140	2,154	2,004
Restaurants***	2,681	2,334	2,404	1,934
Amusement	122	195	129	211
Luxuries***	2,186	2,061	1,965	1,834
Other				
Total savings***	12,084	12,980	12,452	14,662
Consumption expenditure***	53,657	53,164	60,807	60,461
Hh net income***	65,240	66,983	75,529	78,391
Living space (sqm)***	94	100	116	123
Renter***	0.566	0.489	0.394	0.320
Female child	0.486	0.491	0.491	0.487
Father age**	40.6	40.5	40.0	38.7
Mother age***	37.4	37.5	36.8	35.4
Earners***	1.563	1.608	1.426	1.399
N	4,700	3,956	1,713	1,385

Notes: Figures are sample means within the treatment and the control group in each period. All monetary variables are adjusted by the consumer price index with base year 1995. Indicator variables are denoted by (1/0). Excluded are categorial state dummies. Asterisks at variable names indicate the significance of differences in means in the pre-reform period between one- and three-child families. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 3: Effect of child benefits on savings in DD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	HSP		Bank book		Securities		Life insurance	
	1/0	Contribution	1/0	Contribution	1/0	Contribution	1/0	Contribution
Panel A								
Treatment effect	0.066***	878.310***	-0.004	190.482	0.013	-79.140	0.002	-52.533
	(0.020)	(283.326)	(0.009)	(449.787)	(0.020)	(307.166)	(0.014)	(114.535)
Treatment group	0.073***	57.863	-0.002	-298.349	-0.029**	-4.257	0.026***	591.124***
	(0.014)	(179.363)	(0.006)	(350.618)	(0.013)	(149.546)	(0.009)	(81.945)
Post treatment	0.054***	-322.817***	-0.006	-934.675***	0.039***	842.954***	-0.003	231.896***
	(0.011)	(107.426)	(0.004)	(207.339)	(0.010)	(146.951)	(0.008)	(49.647)
Household income	n	n	n	n	n	n	n	n
Additional controls	n	n	n	n	n	n	n	n
Panel B								
Treatment effect	0.062***	824.175***	-0.004	88.592	0.009	-117.992	0.001	-96.480
	(0.019)	(277.439)	(0.009)	(437.704)	(0.019)	(304.809)	(0.014)	(103.274)
Treatment group	0.040***	-439.949**	-0.003	-1,235.307***	-0.068***	-361.531**	0.019*	186.997**
	(0.014)	(178.844)	(0.006)	(350.662)	(0.013)	(147.977)	(0.010)	(75.223)
Post treatment	0.049***	-407.170***	-0.007	-1,093.440***	0.033***	782.416***	-0.004	163.419***
	(0.010)	(106.021)	(0.004)	(202.270)	(0.010)	(142.948)	(0.008)	(46.108)
Household income	y	y	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n	n	n
Panel C								
Treatment effect	0.045**	707.555**	-0.009	-56.773	0.006	-147.067	0.001	-105.406
	(0.019)	(274.689)	(0.009)	(433.767)	(0.019)	(303.892)	(0.014)	(109.285)
Treatment group	-0.002	-539.368	-0.065***	-1,480.532	-0.026	-335.767	-0.004	14.922
	(0.043)	(659.523)	(0.021)	(1,331.813)	(0.040)	(634.242)	(0.030)	(378.037)
Post treatment	0.032***	-435.503***	-0.004	-1,082.414***	0.037***	758.983***	-0.002	173.274***
	(0.010)	(104.287)	(0.005)	(205.529)	(0.010)	(139.718)	(0.008)	(45.597)
Household income	y	y	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y	y	y
Observations	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The results represent coefficients from difference-in-differences estimations as described in equations 2 and 3. The treatment group dummy equals one if the family has three children and zero if it has one child. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The household income control variable includes child benefits. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 4: Effect of child benefits on accumulated savings in DD

Dependent variable:	(1) HSP Value	(2) Bank book Value	(3) Securities Value
Panel A			
Treatment effect	2,578.640***	-517.190	-1,386.296
	(844.501)	(707.611)	(1,793.337)
Household income	n	n	n
Additional controls	n	n	n
Panel B			
Treatment effect	2,373.621***	-653.666	-1,737.399
	(816.807)	(693.013)	(1,746.194)
Household income	y	y	y
Additional controls	n	n	n
Panel C			
Treatment effect	1,762.320**	-582.987	-2,396.796
	(812.439)	(702.074)	(1,694.619)
Household income	y	y	y
Additional controls	y	y	y
Observations	11,753	11,754	11,754

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The results represent coefficients from difference-in-differences estimations as described in equations 2 and 3. The treatment group dummy equals one if the family has three children and zero if it has one child. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The household income control variable includes child benefits. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 5: Effect of child benefits on consumption in DD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Education	Musical instr.	Books	Toys	Alc. & Tobacco	Restaurants	Amusement	Luxury goods
Panel A								
Treatment effect	120.258*	-5.968	-20.200	68.163**	-8.096	-123.367	8.926	-4.754
	(63.212)	(40.463)	(30.087)	(34.651)	(77.429)	(126.948)	(17.908)	(110.880)
Treatment group	293.641***	114.629***	131.752***	112.559***	-127.735**	-276.431***	6.945	-221.828***
	(40.023)	(30.943)	(24.885)	(22.646)	(52.713)	(103.144)	(9.769)	(76.723)
Post treatment	180.037***	-11.387	-18.444	-3.395	-141.945***	-347.207***	72.243***	-125.663**
	(29.002)	(14.211)	(13.269)	(13.717)	(39.312)	(70.381)	(8.969)	(55.602)
Household income	n	n	n	n	n	n	n	n
Additional controls	n	n	n	n	n	n	n	n
Panel B								
Treatment effect	111.252*	-8.330	-26.300	65.430*	-13.833	-150.709	6.641	-42.921
	(61.901)	(40.357)	(29.305)	(34.272)	(77.153)	(124.756)	(17.702)	(103.545)
Treatment group	210.826***	92.905***	75.662***	87.426***	-180.492***	-527.862***	-14.071	-572.809***
	(39.048)	(29.654)	(24.647)	(22.913)	(53.139)	(100.525)	(10.011)	(69.991)
Post treatment	166.004***	-15.068	-27.949**	-7.654	-150.884***	-389.812***	68.682***	-185.135***
	(28.862)	(14.250)	(13.107)	(13.738)	(39.146)	(69.071)	(8.909)	(52.281)
Household income	y	y	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n	n	n
Panel C								
Treatment effect	80.229	-5.958	-27.809	17.454	-4.350	-148.735	8.814	16.362
	(60.843)	(40.471)	(28.942)	(32.517)	(78.420)	(127.767)	(18.393)	(107.376)
Treatment group	-125.873	-191.408**	115.366*	-57.307	-282.503*	-54.509	15.328	-224.778
	(152.583)	(86.579)	(69.262)	(82.849)	(156.842)	(289.631)	(43.054)	(280.043)
Post treatment	158.880***	-6.960	-14.497	-0.387	-112.168***	-354.876***	71.191***	-145.655***
	(29.137)	(14.097)	(13.144)	(14.142)	(39.950)	(69.110)	(8.943)	(51.997)
Household income	y	y	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y	y	y
Observations	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The results represent coefficients from difference-in-differences estimations as described in equations 2 and 3. The treatment group dummy equals one if the family has three children and zero if it has one child. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The household income control variable includes child benefits. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 6: Exploration of income and total expenditure

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Hh net income		Savings		Consumption	
Treatment effect	1,118.928	1,824.995	1,313.881	595.512	146.067	-165.622
	(1,219.259)	(1,172.402)	(808.337)	(732.351)	(916.316)	(702.864)
Household income	n	n	n	y	n	y
Additional controls	n	y	n	y	n	y
Observations	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The results represent coefficients from difference-in-differences estimations as described in equations 2 and 3. The treatment group dummy equals one if the family has three children and zero if it has one child. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The household income control variable includes child benefits. Additional control variables include age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 7: Effect of child benefits on housing in DD

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Living space			Tenant		
Treatment effect	1.369	0.750	0.673	0.003	0.009	-0.010
	(1.525)	(1.358)	(1.388)	(0.020)	(0.019)	(0.019)
Household income	n	y	y	n	y	y
Additional controls	n	n	y	n	n	y
Observations	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The results represent coefficients from difference-in-differences estimations as described in equations 2 and 3. The treatment group dummy equals one if the family has three children and zero if it has one child. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The household income control variable includes child benefits. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 8: Effect of child benefits on savings in DRD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	HSP		Bank book		Securities		Life insurance	
	1/0	Contribution	1/0	Contribution	1/0	Contribution	1/0	Contribution
Panel A								
Treatment effect	0.088***	0.289***	-0.004	0.007	0.039	-0.150	0.002	-0.070
	[0.009]	[0.004]	[0.668]	[0.903]	[0.565]	[0.732]	[0.922]	[0.169]
Household income	y	y	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n	n	n
Panel B								
Treatment effect	0.062*	0.246***	-0.010	-0.013	0.032	-0.190	0.001	-0.076
	[0.050]	[0.010]	[0.313]	[0.822]	[0.637]	[0.655]	[0.936]	[0.159]
Household income	y	y	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y	y	y
Observations	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The figures represent estimates of treatment effects from difference-in-relative-differences estimations as described in equations 4 and 5 evaluated at the treatment group specific mean of all included control variables. The treatment group dummy equals one if the family has three children and zero if it has one child. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The household income control variable includes child benefits. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, a dummy for the tenant status.
P-values based on delta method in squared brackets. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 9: Effect of child benefits on savings as percentage of income in DD

	(1)	(2)	(3)	(4)
Dependent variable:	HSP	Bank book	Securities	Life insurance
		Income shares		
Panel A				
Treatment effect	1.095***	0.045	-0.154	-0.172
	(0.320)	(0.562)	(0.319)	(0.148)
Household income	n	n	n	n
Additional controls	n	n	n	n
Panel B				
Treatment effect	1.077***	0.029	-0.181	-0.178
	(0.319)	(0.562)	(0.318)	(0.148)
Household income	y	y	y	y
Additional controls	n	n	n	n
Panel C				
Treatment effect	0.947***	-0.169	-0.215	-0.182
	(0.318)	(0.561)	(0.322)	(0.137)
Household income	y	y	y	y
Additional controls	y	y	y	y
Observations	*11,754*	*11,754*	*11,754*	*11,754*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The results represent coefficients from difference-in-differences estimations as described in equations 2 and 3. The treatment group dummy equals one if the family has three children and zero if it has one child. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The household income control variable includes child benefits. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 10: Income heterogeneity of the effect of child benefits on savings in DD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	HSP		Bank book		Securities		Life insurance	
	1/0	Contribution	1/0	Contribution	1/0	Contribution	1/0	Contribution
Panel A								
Treatment effect high income	0.057**	1,134.412**	0.004	406.315	0.005	-396.525	0.015	-104.641
	(0.024)	(443.966)	(0.011)	(710.576)	(0.027)	(503.713)	(0.018)	(166.516)
Treatment effect low income	0.063*	667.5***	-0.018	-23.330	0.006	-59.190	-0.013	-11.630
	[0.050]	[0.008]	[0.257]	[0.951]	[0.831]	[0.694]	[0.566]	[0.901]
Household income	y	y	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n	n	n
Panel B								
Treatment effect high income	0.047*	959.745**	-0.000	203.682	0.004	-447.678	0.016	-131.308
	(0.024)	(437.685)	(0.011)	(695.634)	(0.027)	(503.622)	(0.018)	(168.733)
Treatment effect low income	0.041	582.1**	-0.022	-163.200	0.006	-44.550	-0.015	-19.670
	[0.191]	[0.024]	[0.161]	[0.681]	[0.804]	[0.790]	[0.521]	[0.834]
Household income	y	y	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y	y	y
Observations	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*	*11,754*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The figures represent treatment effects for high and low income household from a triple interacted difference-in-differences estimations. Reported coefficients are marginal treatment effects for each of the income groups, i.e., the baseline DD treatment effect for high income households and the baseline DD treatment plus the triple interaction for low income household. The treatment group dummy equals one if the family has three children and zero if it has one child. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The low income household dummy equals zero for households at or above the median income in the sample and one for households with lower incomes. The household income control variable includes child benefits. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 11: Disentangling the effect on HSPs by different family sizes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	0 vs. 1 child		1 vs. 2 children		2 vs. 3 children		1 vs. 3 children	
Dependent variable:	Housing savings plan							
	1/0	Contribution	1/0	Contribution	1/0	Contribution	1/0	Contribution
Panel A								
Treatment effect	0.002	-192.852	0.021	402.016**	0.043**	427.422	0.062***	824.175***
	(0.015)	(167.845)	(0.013)	(157.894)	(0.018)	(283.563)	(0.019)	(277.439)
Household income	y	y	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n	n	n
Panel B								
Treatment effect	0.005	-164.843	0.012	362.907**	0.034*	347.601	0.045**	707.555**
	(0.014)	(165.289)	(0.013)	(156.786)	(0.018)	(278.332)	(0.019)	(274.689)
Household income	y	y	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y	y	y
Observations	*16,969*	*16,969*	*20,745*	*20,745*	*15,187*	*15,187*	*11,754*	*11,754*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The results represent coefficients from difference-in-differences estimations as described in equations 2 and 3. The treatment group dummy equals one for the larger family and is zero for the smaller family. The control group without children in columns (1) and (2) is restricted to couples with male ages until 55 years to make them more comparable to parents. The post treatment dummy equals zero if the year is 1978 and one if the year is 1983. The household income control variable includes child benefits. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, age dummies of the oldest child's age (16 years excluded category) and its interactions with the treatment group dummy, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.