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Labeling Effects of Child Benefits on Family Savings

Timo Hener

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Abstract

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, we show that child benefits are disproportionately used for savings assignable to children. We exploit a policy reform in a difference-in-differences approach to estimate the effect of child benefits on savings while holding total household income constant. Our results suggest a significant positive labeling effect on long-term savings, but no effect on assignable consumption. We 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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Keywords: Fungibility, labeling effects, child benefits, savings.

Timo Hener
Ifo Institute – Leibniz Institute for
Economic Research
at the University of Munich
Poschingerstr. 5
81679 Munich, Germany
Phone: +49(0)89/9224-1418
hener@ifo.de

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 effects 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 formalization of this idea can be found in mental accounting theory which suggests that individuals think of their resources as separate accounts (Thaler, 1980, 1985, 1990, 1999). These mental accounts differ in their propensities to consume specific goods that are grouped into categories. Thus, changing the relative size of mental accounts while holding the income level constant can alter consumption patterns. Imagine an unanticipated vacation allowance of $ 500 that is paid out by your employer. Who would not first think of spending it on a vacation? Besides mental accounting, 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 state who paid out the benefit.

The empirical literature on labeling effects has produced ambiguous results. In the category of family allowances, child benefits have received considerable attention and several studies investigate effects on assignable consumption expenditures. Dutch child benefits increase expenditure on children's clothes disproportionately (Kooreman, 2000). To the contrary, in the United Kingdom no such effect is found. Instead child benefits are spent disproportionately on adult assignable goods alcohol and tobacco, while Households' clothes and food expenditure is found unaffected from child benefit increases (Edmonds, 2002; Blow *et al.*, 2012). Aside from the mixed evidence for child benefits, there is strong support for labeling effects 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). It is less clear, whether fungibility of income is violated or demand elasticities are at work when housing benefits are to a large extend offset by increasing rents (Cage, 1994; Susin, 2002; Fack, 2006). The result of higher food expenditure from *Bono de Desarrollo Humano* cash transfers to women in Ecuador allows different interpretations. It may be driven by a labeling effect, but could as well be the consequence of changes in the intra-household allocation in favor of women (Schady and Rosero, 2008).

Our principal contribution to the literature on labeling effects is to present evidence for the non-fungibility of family transfer income in savings decisions. We provide evidence for child assignable housing savings plans as well as in adult assignable bank books and life insurances. With respect to child well-being, this extends the literature to potentially more important savings compared to contemporaneous consumption. Furthermore, we provide a methodological advancement compared to most existing studies by exploiting a policy reform using quasi-experimental identification strategies.

In this paper, we exploit a child benefit reform to estimate how the labeling of transfer income affects child assignable savings. Between 1978 and 1983 German child benefits were expanded for third children and to some extend for second children but remained constant for first children. This allows us to define a treatment and a control group of families with different numbers of children for use in a difference-in-differences estimation. Thus, we can eliminate confounding variation over time that is common to all families. In the estimations we hold total household income constant to identify the effect of a pure relabeling of parts of the income as *child* benefits. If income was fungible, the labeling should not affect households' savings decisions. But if labeling effects exist, there should be a reaction in expenditure patterns. We use the German Income and Expenditure Survey (EVS), a representative household survey, to identify child assignable savings and other savings. We argue that housing savings plans can be considered as child-specific savings and perform robustness checks to back up the assumption. We also present results on classic assignable consumption goods.

We find that the treatment group increases the probability of holding a housing savings plan by 6.2 percent and increases its value by 15.3 percent. We do neither find an effect on other saving schemes nor on consumption good expenditure for toys and clothes. Hence, the welfare effect for children is clearly non-negative. This result draws attention to the potential importance for policy makers. Labeling is effective to promote a desired behavior even for a long-term savings decision. Moreover, labeling cash transfers is virtually costless. Countries struggling with low private savings rates, thus, could relabel existing benefits before applying administratively 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.

Our result of a labeling effect on housing savings plans is robust to a number of different specifications and tests. A placebo difference-in-difference estimation with two unaffected groups of households confirms the suitability of our estimation strategy. The result is also robust to a relative trend assumption we impose in an alternative specification of the difference-in-differences estimator. Heterogeneity with respect to income and effects on the savings rate yield similar conclusions. Moreover, we do not find evidence for strong intra-household distribution effects (Browning *et al.*, 1994) that would confound the basic result.

The remainder of the paper is structured as follows. In Section 2, we describe the empirical approach and the data. In Section 3, we report results and various robustness checks. We conclude the analysis in Section 4.

2 Empirical Approach

Expenditure patterns are subject to independent changes over time which would bias estimations that rely only on time-dependent alterations in child benefits. Therefore, for the identification of labeling effects we exploit an unanticipated German child benefits reform that affected various family types differently. 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) were given out monthly, some 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. In our analysis, we compare a treatment group comprised of three-child families with a control group comprised of one-child families over time. In contrast to earlier studies (Kooreman, 2000; Edmonds, 2002; Blow *et al.*, 2012), the large reform allows the use of quasi-experimental techniques to identify causal effects under relatively weak assumptions.[1] 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, our 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 our estimates. Lump-sum child benefits are not means tested, paid monthly and available to all children up to the age of 16.[2]

[1]Kooreman (2000) evaluates the labeling effect by relying on a single change in child benefits for over- and under-6 year-olds to identify marginal propensities to consume child goods. His approach relies on the 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 in a difference-in-differences framework only if the mother receives the payments, which means that the effect cannot be distinguished between a labeling and a distributional preference-driven effect.

[2]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

2.1 Estimation Design

We ought to test whether savings evolve differently over time between a treatment group and a control group. The control group delivers a counterfactual that indicates how the treatment group would have evolved in the absence of a treatment. We use two distinct definitions of the counterfactual—a standard absolute counterfactual and a new relative counterfactual—in a standard difference-in-differences (DiD) and a difference-in-relative-differences (DiRD) models.

The DiD model can be described as a double difference over time between the treatment and the control group. We 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 our 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, we use the regression form

$$Y_{st} = \alpha_0 + \alpha_1 Treated_s + \alpha_2 Post_t + \delta(Treated_s \times Post_t) + \beta_1 Inc_{st} + \epsilon_{st}, \tag{2}$$

where ϵ_{st} is an i.i.d. error term. Besides the treatment effect, the regression yields estimates for the pre-reform baseline savings (α_0), the baseline difference between the treatment and the control group (α_1), the common time trend (α_2) and full income (β_1). The treatment effect δ can be interpreted as an average treatment effect on the treated (ATT). It is crucial for the interpretation of a labeling effect that the income level remains constant while changing child benefits. If full income including child benefits is the same, a change in child benefits means that the treatment is a compositional change in the income. This is how we identify labeling effects. Therefore, full household income Inc_{st} including all child benefits is included in the standard specification.

The estimation accounts for all unobserved time-constant differences between the treatment and the control group. To allow for time-varying differences we 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 interacted with the treatment group dummy (β_{11}) and of the control variables (β_2).

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 our estimation as allowances become effective after tax return at the end of a year.

In the standard DiD model we assume that both groups would have experienced the same absolute change in the outcome variable in the absence of any treatment which is often referred to as the common trend assumption. This assumption is crucial but may be too restrictive as we allow the baseline levels of the outcome Y_{st} between the groups to be different. It may be more realistic to assume a counterfactual that imposes the same percentage change of the baseline value instead. We refer to this model as a difference-in-relative-differences (DiRD) approach.[3] Estimates can easily be calculated from regression coefficients of the standard DiD model in the following way:[4]

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

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

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

where X_1 denotes mean control variables in the treatment group and X_0 denotes mean control variables in the control group.

The second important assumption of DiD models imposes that self-selection into treatment may not occur. In our setting there are basically 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 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. Bias in the estimation may anyway be small as these families should not have tremendously different expenditure and savings patterns.

All estimations are carried out using ordinary least squares techniques. Standard errors are obtained using Huber/White/sandwich estimates that are robust to heteroscedasticity which is likely to occur in estimations of expenditure and savings as variability of the dependent variables may easily increase with income. Estimation of the DiRD coefficients are followed by t-tests using the Delta method to obtain significance levels for the percentage estimates.

2.2 Data

In the empirical analysis we employ two consecutive cross-sectional waves of the German Income and Expenditure Survey (EVS: Einkommens- und Verbrauchsstichprobe) from

[3]To our knowledge, the only similar approach that has used this type of alteration can be found in Gregg *et al.* (2009). The authors refer to it as a percentage method.

[4]See appendix for details of derivation.

1978 and 1983. The EVS is a representative survey of about 45,000 households that is conducted every five years, starting in 1978.[5] Our 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. We exclude households that report negative incomes. Assumptions of linearity in expenditure are then less threatened by outliers. 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. We exclude families with more than two earners such that earning children are not included. Families with the oldest child being younger than three years are excluded to get more comparable family types. We also exclude families that 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.

Group assignment variables Assignment of households to the treatment and the control group are based on the number of children reported in the household. We 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 we would expect a downward bias in our 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 unity for the treatment group and zero for the control group. We will alter the assignment in later robustness tests.

2.3 Dependent variables

The main outcome variables are different forms of savings. The most important measure are 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

[5]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.

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).

A couple of reasons point towards the importance of HSPs for children. First, compared to usual savings HSPs are long-term activities and may not be spend on short-lived consumption goods during the savings period. Second, the savings are expected to yield a purchase or construction of a home that the child may grow up in or inherit at a later point in time. Third, although we cannot observe which individual in the family an HSP is made out to, anecdotal evidence starkly suggests that many parents save for their children in HSPs to transfer the contract to them once they leave their home. Börsch-Supan and Stahl (1991) report that the probability to hold an HSP in Germany 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 owners, with almost twofold the propensity of renters, makes more sense as savings for children than as savings for purchasing a second home.

A crucial assumption in DiD 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 income households in the magnitude of 18 percent in 1978 and 14 percent in 1983. An extra bonus per child was unaffected by the reform. The overall reduction in bonuses affected both the treatment group and the control group. The amount for tax deduction of saving deposits were constant over time.

Other dependent variables are non-assignable bank books and life insurances. We measure both a dummy for owing an account as well as the accumulated savings for the former and yearly deposits for the latter. Bank books are a general form of savings that can take on almost any duration. Life insurances usually have very long durations and partly insure the family against income default. We assume these savings forms to be either adult assignable or unassignable.

For tests on consumption expenses we use child-assignable toy expenditure and adult-assignable luxury goods expenditure in DEM per year. Luxury goods include jewelry, leather goods, and watches.[6] Our third consumption good is clothes expenditure. Unfortunately, information on clothes expenditure is only available distinguished by gender. Hence, we can only assign female clothes to the mother if all children and thus all other

[6]Variables were differently coded in 1978 and 1983 and therefore include slightly different leather products in 1983.

household members are male. Then, female clothes are adult goods. Accordingly, male clothes are assigned to the father if all children are female.

2.4 Control variables

The DiD 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, we ought to control for age composition in the family. We 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, we 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.

The main reason for income control variables is that results from the difference-in-differences model cannot be readily interpreted as labeling effects. Child benefit changes induce direct income effects on expenditure, meaning that household income is not an exogenous variable. Hence, controlling for income excludes this channel from the reform effect. We use full household income, including child benefits, as a control variable because we need the full disposable income to be held constant. Only then, we can interpret the result as a pure labeling effect, as the remaining treatment is only a change in the income composition. 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, we 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, we include background characteristics of the two groups that could violate identifying assumptions. The oldest child's sex controls for differential treatment. We 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, we also include an indicator for the number of earners to account for intra-household allocations. An expenditure variable for durable goods controls for expensive purchases that could affect remaining consumption. Furthermore, we control for income squared and indicator variables for the federal state 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 DiD assumptions, we 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 are the control group and the treatment group. We 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.

From the group means over time, we can already see how the difference-in-differences results without control variables turn out to be. 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 treatment effect of about 6 percentage points. Values of HSPs decrease from 10,824 DEM to 10,742 DEM in the control group and increase from 11,055 DEM to 13,550 DEM in the treatment group, suggesting a treatment effect of more than 2,500 DEM. Prevalence of bank books is virtually constant in the control group at slightly above 95 percent and it decreases by just one percentage point in the treatment group from 95.7 to 94.7 percent. Their accumulated value decreases by about 4,000 DEM in the control group and by about 4,500 in the treatment group, suggesting a small negative treatment effect. Prevalence of life insurances is also high at 85 percent in the control group and 88 percent in the treatment group and does not change over time. The yearly deposits increase in both groups by about 360 DEM. Expenditures for toys are virtually constant in the control group from the pre- to the post-reform period, whereas they increase in the treatment group from 29.40 DEM to 34.80 DEM, suggesting a treatment effect of about 6 DEM. Expenditures for adult luxury goods increase in the control group from 207 DEM to 242 DEM, while they decrease in the treatment group from 183 DEM to 171 DEM, suggesting a negative treatment effect of about 47 DEM. Expenditures for the mother's clothes (identified only for subgroup with male children) decrease from 1,587 DEM to 1,268 DEM in the control group and from 1,526 DEM to 1,039 DEM in the treatment group, yielding a negative treatment effect of about 170 DEM. Expenditures for the father's clothes (identified only for subgroup with female children) decrease from 933 DEM to 808 DEM in the control group and from 771 DEM to 651 DEM in the treatment group, yielding a small treatment effect of 5 DEM. In the regression analysis in the following section, we also analyze the statistical significance of the treatment effects and can control for a number of additional variables, but most importantly, we identify the labeling effect only after controlling for changes in household income.

The lower panel of Table 2 shows the differences in levels and over time of control variables between the treatment and control group. Household net income increases in the control group from 65,240 DEM to 66,983 DEM, whereas it increases from 75,529 DEM to 78,391 DEM in the treatment group, pointing to the importance of income controls in the analysis. 49 percent of the oldest children are girls in both groups. Age of the fathers averages at 39 to 41 years, age of the mothers at 35 to 37 years. The number of earners increases slightly in the control group and decreases slightly in the treatment group. Expenditures for durable goods decrease from about 9,013 DEM to 7,978 DEM in the control group and from 9,191 DEM to 8,012 DEM in the treatment group. Tenant status, indicating renting of accommodation, declines from 57 to 49 percent in the control group and from 39 to 32 percent in the treatment group. 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 Results

In the following chapter, we show results of the estimations as described in section 2. We begin with baseline results for savings, followed by a placebo treatment analysis and a number of robust checks.

3.1 Baseline results

In Table 3 we report results from DiD estimations as defined in equations 2 and 3 for the treatment effect of the child benefit increase on household savings. Panel A depicts results with household income as the only control variable, whereas Panel B shows results of the full specification. In column 1 we see that the treatment effect on the probability of holding an HSP is 6.2 percentage points, which is statistically significant at the one percent level. Including all control variables yields the result that following the reform treatment group families are 4.5 percentage points more likely to hold HSPs than control group families. 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 that is already present in the descriptive analysis. Turning to the accumulated value of the HSPs, the treatment group holds additional savings worth 2,374 DEM with basic controls respectively 1,753 DEM with all control variables. Both estimates are statistically significant at conventional levels. The treatment effects are also economically significant as they play in the ballpark of up to a fifth of the average HSP value. Again, the baseline difference in the outcome is vanished when including all control variables.

[Table 3 about here.]

In contrast to savings in HSPs, we do not find any significant treatment effects on savings in bank books or life insurances. Estimates for holding bank books and for its value are negative, but standard errors are very large. All estimates for life insurances are virtually zero. This result is consistent with labeling effects of child benefits if HSPs are assignable to children. Further indication of the nature of HSPs may be found in the families' behavior in terms of accommodation conditions. In Table 4, we regress an indicator for being a renter and the flat size in square meters on the DiD model to test for changes in accommodation conditions due to the reform. All treatment effects with basic and full controls are indistinguishable from zero, indicating that the additional child benefits are not consumed and rather reflect long-term savings.

[Table 4 about here.]

In Table 5 we show results from DiRD estimations that relax the equal trend assumption of the regular DiD 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 holding an HSP is 8.8 percent with basic controls and 6.2 percent with full controls in terms of the baseline level. Both estimates are statistically significant, the latter one only just at the ten percent level. Treatment effects on the accumulated value of HSPs are 21.1 percent with basic controls and 15.3 percent with full controls. Both estimates are statistically significant at conventional levels. Estimates of treatment effects on bank books and life insurances are small and statistically insignificant.

[Table 5 about here.]

On the whole, DiD and DiRD estimations consistently reveal that the treatment, an increase in child benefits, makes parents save more in long-term HSPs rather than regular bank books or life insurances. The result is consistent with a labeling effect of child benefits that favors savings which are assignable to children.

3.2 Placebo treatment tests

Placebo DiD estimations are carried out to test the basic assumptions underlying the regressions. Table 6 shows results for one-child-families as the treatment group and childless couples as the control group. As no change in child benefits occurred for this treatment group and the control group does not get any child benefits, estimations should not yield results if the assumption of equal trends in absence of a treatment is valid for groups of different demographic backgrounds. The results for HSP are small with changing signs and not statistically significant. The DiD assumption of equal trends for HSPs therefore seems plausible. On the remaining saving measures only the effect on the probability of holding a bank book is just significant at the ten percent level with a small negative effect of one percentage point. Results on this variable should therefore be interpreted with caution. However, the effect is small and the other three outcomes do not show a reaction to the placebo treatment.

[Table 6 about here.]

3.3 Effects on savings rate

In Table 7 we report results on savings as percentage of household income. The first three columns show effects with basic controls and the last three columns show effects with full controls. The treatment effect on HSPs can also be found as a percentage of income. The savings rate in HSP increases by 2.7 percentage points or 1.9 percentage points due to the increase in child benefits, where both estimates are statistically significant at the ten percent level or better. The same is true for results from the DiRD estimation in Panel B. Here, treatment effects are 18.4 or 12.1 percent. The other savings rates from bank books and life insurances do not show significant effects. The only just significant coefficient in column 2 and Panel B for bank books loses significance once we add all control variables. Thus, estimation results from standard DiD estimations are not subject to changes in income. Labeling effects can be found directly as a change in the propensity to save as a fraction of income, i.e., parents spend higher fractions of income on long-term savings due to the larger fraction of labeled income.

[Table 7 about here.]

3.4 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. We 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 8 shows the treatment effects separately for high and low income families. The effect size on the probability of holding HSPs is 5.7 percentage points for high income households and 6.3 percentage points for low income households in estimations with basic control variables. Both estimates are statistically significant at the five respectively the ten percent level, however, they are statistically indistinguishable from each other. With the full set of control variables, the effect size are reduced to 4.7 and 4.1 percentage points, where the effect for low income families loses statistical significance. Thus, getting an HSP due to the reform is not restricted to low or high income families. Looking at the accumulated value of HSPs, high income families show a treatment effect of 3,122 DEM, whereas low income families increase their savings by only 1,499 DEM in the estimation with basic controls. Although both effects are statistically significant at the five respectively ten percent level they are again not distinguishable from each other. Thus, although the effect sizes are somewhat larger for high income families, we cannot infer substantial income heterogeneity from this exercise. With the full set of control variables the treatment effects are reduced to 2,277 DEM and 994 DEM, where the effect for low income families loses statistical significance again, but the overall picture is unaffected. For bank books and life insurances all treatment effects are indistinguishable from zero for both high and low income families. While there is only weak evidence for income heterogeneity, the earlier result on HSPs are confirmed.

[Table 8 about here.]

3.5 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 9 shows the results of a DiD estimation on child and adult assignable consumption goods. The treatment effect on toy expenditure is 5.45 DEM without controls and statistically significant at the ten percent level. The effect size is rather large at just below 20 percent of the baseline level. When including the full set of control variables the effect becomes small and insignificant. Thus, there is only weak evidence on labeling effects for consumption goods. The treatment effect on luxury goods has a negative sign and is large as well at about 25 percent. However, the estimate is statistically insignificant in both specifications with basic and full controls. A labeling effect that works through changes in the propensity to consume out of different income sources cannot explain reductions of consumption goods when child benefits increase. Such a result would be indication of spillover effects into other categories of consumption that go beyond a linear association of income shares and propensities to consume.

Estimations with parents' clothes in columns (3) and (4) use fewer observations because we can only distinguish male and female clothing expenditures. Thus, the father's clothes are measured only for households in which all other members are female and vice versa for mother's clothes. For the father's clothes, we find small and insignificant treatment effects. Expenditure for the mother's clothes is reduced by the reform, but only just statistically significant when including basic controls. The effect sizes are below ten percent of baseline expenditure.

[Table 9 about here.]

3.6 Distributional confounders and disentangling the effect

A frequent concern in studies about welfare payments and expenditure are distributional confounders (see Blow *et al.*, 2012). If child benefits alter the intra-household allocation of resources, shifts in consumption patterns could reflect gender-specific preferences which has been documented extensively in the literature (see Thomas, 1994; Lundberg *et al.*, 1997; Duflo, 2003; Ward-Batts, 2008). Our interpretation of a labeling effect could, thus, be confounded by intra-household allocation. On the one hand, this concern is only partly applicable in our setting, because child benefits are not paid to a particular parent. As Jacoby (2002) shows, such a flypaper effect[7] could make the transfer stick to particular individuals and affect consumption. Instead, child benefits are transferred to whoever the parents choose themselves, which can in itself be a result of the intra-household allocation decision. On the other hand, we do not want to rule out that the

[7] See Hines and Thaler (1995) for seminal literature.

mere fact that child benefits are paid may affect intra-household distributions in a systematic way. In our data, we cannot identify who is the recipient of child benefits. To test the robustness of our results to intra-household allocations, we exploit a peculiarity in the payment of child benefits. Civil servants file their application for child benefits at their employer and receive the payment via payroll, whereas everybody else has to file at the employment agency's family office (Familienkasse) and receive the payment separately. The convenience of application and payment decreases the cost of choosing the civil servant within a household as recipient of child benefits. Therefore, we assume that if the husband is a civil servant, the probability that he is the recipient is higher than in the rest of the population. If the labeling effect was confounded by intra-household allocation, we would find heterogeneity in the treatment effect along these lines. Table 10 depicts separate treatment effects for households where the husband is a civil servant and for all other households. We find an increase in the probability of holding an HSP by 10.2 percentage points in civil servant households and by 5.2 percentage points in other households when including only basic controls. Both effects are statistically significant at conventional levels. However, the effects are indistinguishable from each other. When including all controls the effect sizes decrease somewhat, but stay statistically significant at the ten percent level. The accumulated value of HSPs is increased by 3,286 DEM for civil servant households with basic controls, but the estimate is statistically insignificant. For other households the increase is smaller at 2,104 DEM but reaches statistical significance. Again, the two estimates cannot be distinguished in a statistical sense. When including all controls, the same pattern emerges. For other savings in bank books and life insurances we do not find significant effects for neither civil servant households nor for the others. Evidence for heterogeneity in the treatment effect with respect to civil servant status is rather weak. Thus, intra-household allocation may play a role for the savings decision, but it could also be a particular preference of civil servant households for long-term savings. Due to the nature of child benefits payments that are not tied to a particular person, the results do not seem to suggest that the only channel is intra-household allocation.

[Table 10 about here.]

In Table 11 we disentangle the estimated effect on HSPs by family size. We have seen that not only the third child enjoyed an increase in child benefits but also the second child, although at a much lower rate. In other words, the treatment effect is much lower for families with two children and, thus, we would expect a smaller effect on HSPs. In columns (1) and (2) of Table 11 we see the effect of a treatment group with two children compared to a control group with one child. The estimate for the probability of holding an HSP is positive but small and statistically insignificant. This is true for the estimate with basic controls in Panel A and with full controls in Panel B. Looking at the accumulated savings we find a statistically significant effect of 1,604 DEM with basic controls and 1,231 DEM with full controls. Compared to the baseline estimates from Table 3 that are repeated in columns (5) and (6), the effects are smaller although not in a statistical sense. This result is consistent with the expectation from a smaller treatment. In columns (3) and (4), we 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 and is only supposed to illustrate what the treatment effect is composed of. The artificial treatment effect on holding 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 accumulated savings are not statistically significant and range from 578 DEM to 770 DEM. Hence, the treatment effects are in fact smaller for the treatment group with two children. Looking at the composition of the effect reveals that the largest increase in the incidence of HSPs comes from families with three children compared to families with two children. A large part of the effect on the accumulated savings in HSPs can already be found for families who received the smaller treatment, pointing to some non-linearity in the effect. Nevertheless, the conclusion of treatment effect on HSPs is consistent when considering the comparatively small treatment for families with two children.

[Table 11 about here.]

4 Conclusion

Our results suggest that an unanticipated reform of child benefits works through a labeling effect to increase long-term savings in housing savings plans. Using difference-in-differences estimation, we find that the treatment group increases the probability of holding an HSP by 4.5 percentage points or 6.2 percent. Accumulated value of the HSPs increases by 1,735 DEM or 15.3 percent. As these figures are economically significant and as HSPs are beneficial for children, we conclude that the labeling effect substantially increases child welfare. However, in contrast to earlier work, evidence for labeling effects is very weak when looking at consumption goods. Taking into account that the treatment effect on HSPs is small and sometimes statistically insignificant for low income families,[8] it is at least doubtful that the labeling effect is helpful at reducing child poverty.

The increasingly popular idea of libertarian paternalism or "nudging" as framed in the famous book by Thaler and Sunstein (2008) receives additional support from this new evidence for labeling effects on 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 one wants to exclude affecting the free will of individuals by policy measures, one should be aware of the power of labeling effects that can have unintended consequences.

Despite the strong results, this work has certain limitations. First, the results may not be fully transferable to more recent periods. Future research may want to find a way how to identify assignable long-term savings in recent years. Second, the results may not be persistent over longer time spans. Future research may look into the life-cycle perspective of labeling effects from child benefits.

[8]There is no income heterogeneity in the effect on consumption either. Results are not reported.

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A Derivation of DiRD Estimator

We 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 (DiD) 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 (DiRD) 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 DiD as in

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

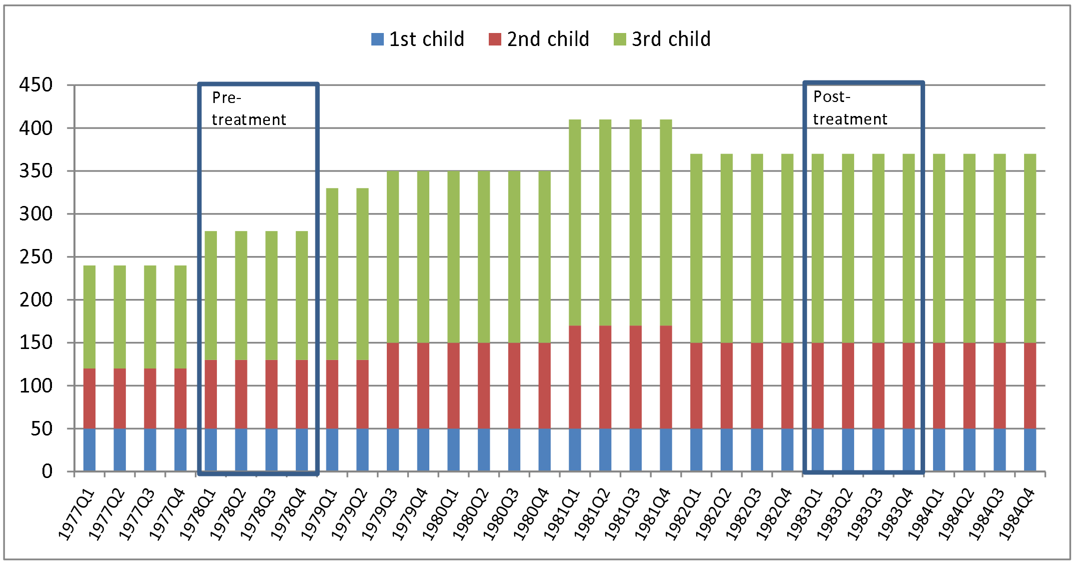
Now we can identify the expected outcomes from the regression coefficients and rewrite the DiRD treatment effect as

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

what simplifies to

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

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.

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 Pre-reform (1978)	1 child Post-reform (1983)	3 children Pre-reform (1978)	3 children Post-reform (1983)
Dependent variables				
Housing savings plan (1/0)	0.562	0.616	0.635	0.755
Housing savings plan (worth)	10824.770	10742.010	11054.610	13550.490
Bank book (1/0)	0.959	0.952	0.957	0.947
Bank book (worth)	15102.050	11174.950	14424.650	9980.365
Life insurance (1/0)	0.851	0.848	0.877	0.877
Life insurance (deposits)	1389.646	1747.252	1885.330	2248.224
Toys	20.023	19.740	29.403	34.800
Luxury goods	206.614	241.843	183.142	171.269
Mother's clothes	1587.092	1268.407	1525.797	1038.974
Father's clothes	932.740	808.187	771.473	650.732
Flat size (sqm)	93.757	99.918	115.563	123.092
Control variables				
Household net income	65239.510	66983.010	75528.900	78391.330
Oldest child's sex (2=fem)	1.486	1.491	1.491	1.487
Age father	40.554	40.517	40.005	38.690
Age mother	37.390	37.466	36.811	35.421
Number of earners	1.563	1.608	1.426	1.399
Durable goods	9013.303	7977.767	9190.789	8012.263
Tenant status (1/0)	0.566	0.489	0.394	0.320
N	4700	3956	1713	1385

Notes: Figures are sample means within the treatment and the control group in each period. All monetary variables are corrected for consumer price index with base year 1995. Indicator variables are denoted by (1/0). Included are all non-categorial control variables, thus, age and state dummies are left out.

Table 3: Effect of child benefits on savings in DiD

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Housing savings plan (1/0)	Housing savings plan (worth)	Bank book (1/0)	Bank book (worth)	Life insurance (1/0)	Life insurance (deposits)
Panel A						
Treatment effect	0.062***	2,373.621***	-0.004	-653.666	0.001	-33.335
	(0.019)	(816.807)	(0.009)	(693.013)	(0.014)	(109.186)
Treatment group	0.040***	-1,658.306***	-0.003	-1,932.386***	0.019*	140.518*
	(0.014)	(576.055)	(0.006)	(559.293)	(0.010)	(74.287)
Post treatment	0.049***	-402.645	-0.007	-4,139.754***	-0.004	297.425***
	(0.010)	(416.200)	(0.004)	(366.280)	(0.008)	(46.604)
Household income	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n
Panel B						
Treatment effect	0.045**	1,752.817**	-0.009	-581.073	0.001	-59.735
	(0.019)	(811.948)	(0.009)	(701.787)	(0.014)	(117.625)
Treatment group	-0.002	-504.810	-0.065***	-3,562.982*	-0.004	-250.753
	(0.043)	(2,052.363)	(0.021)	(1,957.663)	(0.030)	(357.194)
Post treatment	0.031***	-443.831	-0.004	-4,032.027***	-0.002	303.757***
	(0.010)	(404.888)	(0.005)	(362.750)	(0.008)	(46.381)
Household income	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y
Observations	*11,754*	*11,753*	*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 in order to infer labeling effects. 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, amount spent on durable goods, 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 housing in DiD

	(1)	(2)	(3)	(4)
Dependent variable:	Renter (1/0)	Flat size (sqm)	Renter (1/0)	Flat size (sqm)
Panel A				
Treatment effect	0.009	0.750	-0.010	0.445
	(0.019)	(1.358)	(0.019)	(1.341)
Treatment group	-0.116***	16.112***	-0.150***	3.207
	(0.014)	(0.961)	(0.040)	(3.424)
Post treatment	-0.068***	5.196***	-0.059***	3.597***
	(0.010)	(0.603)	(0.010)	(0.547)
Household income	y	y	y	y
Additional controls	n	n	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 in order to infer labeling effects. 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, amount spent on durable goods, and, in regressions, of flat size 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 savings in DiRD

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Housing savings plan (1/0)	Housing savings plan (worth)	Bank book (1/0)	Bank book (worth)	Life insurance (1/0)	Life insurance (deposits)
Panel A						
Treatment effect	0.088***	0.211***	-0.004	-0.056	0.002	-0.073
	[0.009]	[0.006]	[0.668]	[0.143]	[0.922]	[0.265]
Household income	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n
Panel B						
Treatment effect	0.062*	0.153**	-0.010	-0.052	0.001	-0.089
	[0.050]	[0.037]	[0.315]	[0.184]	[0.935]	[0.204]
Household income	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y
Observations	*11,754*	*11,753*	*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 in order to infer labeling effects. 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, amount spent on durable goods, 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 6: Placebo DiD estimations on savings

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Housing savings plan (1/0)	Housing savings plan (worth)	Bank book (1/0)	Bank book (worth)	Life insurance (1/0)	Life insurance (deposits)
Panel A						
Treatment effect	0.008	-385.705	-0.011*	-194.693	-0.009	73.388
	(0.012)	(466.755)	(0.006)	(506.948)	(0.010)	(53.123)
Treatment group	0.200***	3,312.736***	0.026***	-6,858.844***	0.147***	356.700***
	(0.008)	(331.750)	(0.004)	(385.945)	(0.007)	(33.501)
Post treatment	0.038***	-5.215	0.004	-4,008.002***	0.004	237.778***
	(0.006)	(209.680)	(0.003)	(354.764)	(0.006)	(25.882)
Household income	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n
Panel B						
Treatment effect	0.014	-305.907	-0.010*	-517.621	-0.003	71.467
	(0.011)	(455.934)	(0.006)	(502.553)	(0.010)	(53.094)
Treatment group	-0.011	-5,608.488***	0.061***	4,921.298***	0.080***	-566.993***
	(0.018)	(1,175.248)	(0.010)	(1,101.727)	(0.014)	(141.672)
Post treatment	0.017***	-276.400	0.004	-3,613.275***	0.002	231.869***
	(0.005)	(204.559)	(0.003)	(356.182)	(0.006)	(25.508)
Household income	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y
Observations	*32,166*	*32,163*	*32,166*	*32,166*	*32,166*	*32,166*

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 one child and zero if the couple is childless. 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 in order to infer labeling effects. Additional control variables include an interaction of household income with the treatment group dummy, household income squared, the oldest child's gender, federal state dummies (Schleswig-Holstein excluded category), age of each of the parents, the number of earners, amount spent on durable goods, 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 savings as percentage of income in DiD

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: (as fraction of income)	Housing savings plan (worth)	Bank book (worth)	Life insurance (deposits)	Housing savings plan (worth)	Bank book (worth)	Life insurance (deposits)
Panel A: DiD						
Treatment effect	2.734***	-0.075	-0.051	1.859*	0.167	-0.103
	(1.045)	(0.911)	(0.126)	(1.029)	(0.902)	(0.133)
Treatment group	-2.036***	-3.520***	0.251***	-1.330	-6.582***	0.073
	(0.753)	(0.750)	(0.079)	(1.830)	(2.121)	(0.262)
Post treatment	-0.627	-6.352***	0.519***	-0.776	-6.136***	0.520***
	(0.589)	(0.517)	(0.061)	(0.571)	(0.515)	(0.061)
Household income	y	y	y	y	y	y
Additional controls	n	n	n	y	y	y
Panel B: DiRD						
Treatment effect	0.184**	-0.061*	-0.054	0.121*	-0.049	-0.078
	[0.012]	[0.087]	[0.361]	[0.079]	[0.177]	[0.210]
Household income	y	y	y	y	y	y
Additional controls	n	n	n	y	y	y
Observations	*11,754*	*11,753*	*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 in Panel A represent coefficients from difference-in-differences estimations as described in equations 2 and 3. The figures in Panel B 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 in order to infer labeling effects. 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, amount spent on durable goods, a dummy for the tenant status.
Robust standard errors in parenthesis and p-values based on delta method in squared brackets. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 8: Income heterogeneity of the effect of child benefits on savings in DiD

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Housing savings plan (1/0)	Housing savings plan (worth)	Bank book (1/0)	Bank book (worth)	Life insurance (1/0)	Life insurance (deposits)
Panel A						
Treatment effect high income	0.057** (0.024)	3,122.396** (1,312.433)	0.004 (0.011)	-436.935 (1,108.800)	0.015 (0.018)	-60.214 (175.493)
Treatment effect low income	0.063* [0.050]	1498.994* [0.064]	-0.017 [0.257]	161.509 [0.830]	-0.013 [0.566]	34.751 [0.734]
Equality of coefficients (p-value)[1)]	0.874	0.293	0.266	0.655	0.337	0.640
Household income	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n
Panel B						
Treatment effect high income	0.046* (0.024)	2,277.310* (1,285.255)	0.000 (0.011)	-218.087 (1,093.043)	0.016 (0.018)	-97.866 (180.993)
Treatment effect low income	0.041 [0.188]	994.248 [0.224]	-0.023 [0.159]	198.702 [0.792]	-0.015 [0.521]	19.181 [0.854]
Equality of coefficients (p-value)[1)]	0.893	0.397	0.244	0.753	0.291	0.571
Household income	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y
Observations	*11,754*	*11,753*	*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 DiD treatment effect for high income households and the baseline DiD 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 in order to infer labeling effects. 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, amount spent on durable goods, a dummy for the tenant status. Robust standard errors in parenthesis. P-values from F test of combined effect of basic treatment effect and interaction with low household income dummy in squared brackets. * significant at 10%; ** significant at 5%; *** significant at 1% level.
[1)] Test for equality of coefficients of the treatment effect for low income households against high income households based on triple interaction term.

Table 9: Effect of child benefits on consumption in DiD

	(1)	(2)	(3)	(4)
Dependent variable:	Toys (expenditure)	Luxury goods (expenditure)	Father's clothes (expenditure)	Mother's clothes (expenditure)
Panel A				
Treatment effect	5.453*	-52.471	13.125	-187.841*
	(2.856)	(32.551)	(62.667)	(107.560)
Treatment group	7.285***	-72.840***	-262.028***	-238.190***
	(1.909)	(24.142)	(46.008)	(90.573)
Post treatment	-0.638	26.863*	-148.356***	-340.605***
	(1.145)	(13.925)	(21.540)	(32.753)
Household income	y	y	y	y
Additional controls	n	n	n	n
Panel B				
Treatment effect	1.471	-53.543	36.299	-159.019
	(2.708)	(36.725)	(59.892)	(112.110)
Treatment group	-4.806	-135.302	71.225	-590.726**
	(6.902)	(136.181)	(150.824)	(277.634)
Post treatment	0.138	39.604***	-137.961***	-320.699***
	(1.182)	(14.725)	(21.453)	(32.891)
Household income	y	y	y	y
Additional controls	y	y	y	y
Observations	*11,754*	*11,753*	*4,632*	*4,894*

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 in order to infer labeling effects. 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, amount spent on durable goods, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

Table 10: Distributional confounders from civil servant heterogeneity in DiD

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Housing savings plan (1/0)	Housing savings plan (worth)	Bank book (1/0)	Bank book (worth)	Life insurance (1/0)	Life insurance (deposits)
Panel A						
Treatment effect no civil servant	0.052** (0.022)	2,104.149** (851.467)	0.001 (0.011)	-936.101 (811.362)	0.010 (0.015)	6.963 (129.364)
Treatment effect civil servant	0.102*** [0.005]	3,286.154 [0.134]	-0.022 [0.196]	164.559 [0.895]	-0.017 [0.594]	-43.763 [0.798]
Equality of coefficients (p-value)[1)]	0.239	0.615	0.245	0.460	0.457	0.813
Household income	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n
Panel B						
Treatment effect no civil servant	0.039* (0.022)	1,550.728* (847.395)	-0.004 (0.011)	-941.281 (817.181)	0.008 (0.015)	-35.278 (134.761)
Treatment effect civil servant	0.067* [0.060]	2,498.263 [0.246]	-0.026 [0.129]	664.749 [0.594]	-0.018 [0.593]	-85.1 [0.635]
Equality of coefficients (p-value)[1)]	0.497	0.682	0.271	0.281	0.488	0.817
Household income	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y
Observations	*11,754*	*11,753*	*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 households in which the father is a civil servant and for remaining households from a triple interacted 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 low income household dummy equals zero for households at or above the median income in the sample and and one for households with lower incomes. The household income control variable includes child benefits in order to infer labeling effects. 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, amount spent on durable goods, a dummy for the tenant status. Robust standard errors in parenthesis. P-values from F test of combined effect of basic treatment effect and interaction with low household income dummy in squared brackets. * significant at 10%; ** significant at 5%; *** significant at 1% level.
[1)] Test for equality of coefficients of the treatment effect for civil servant households against others based on triple interaction term.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	1 vs. 2 children		2 vs. 3 children		1 vs. 3 children	
Dependent variable:	Housing savings plan (1/0)	Housing savings plan (worth)	Housing savings plan (1/0)	Housing savings plan (worth)	Housing savings plan (1/0)	Housing savings plan (worth)
Panel A						
Treatment effect	0.021	1,604.422***	0.043**	770.464	0.062***	2,373.621***
	(0.013)	(587.816)	(0.018)	(815.942)	(0.019)	(816.807)
Household income	y	y	y	y	y	y
Additional controls	n	n	n	n	n	n
Panel B						
Treatment effect	0.012	1,230.560**	0.034*	578.226	0.045**	1,752.817**
	(0.013)	(588.416)	(0.018)	(821.969)	(0.019)	(811.948)
Household income	y	y	y	y	y	y
Additional controls	y	y	y	y	y	y
Observations	*20,745*	*20,745*	*15,187*	*15,186*	*11,754*	*11,753*

Notes: Each column in each panel reports the results of a regression for the outcome listed at the top. The first two columns report results for a treatment group of families with two children and a control group of families with one child. The columns (3) and (4) report results for a treatment group of families with three children and a control group of families with two children. The columns (5) and (6) report results repeated from Table 3 for a treatment group of families with three children and a control group of families with one child. The results represent coefficients from difference-in-differences estimations. The household income control variable includes child benefits in order to infer labeling effects. 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, amount spent on durable goods, a dummy for the tenant status.
Robust standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1% level.

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