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Effects of Labeled Child Benefits on Family Savings

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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, 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.

Keywords Mental accounting · Labeling effects · Child benefits · Savings

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

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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 & Kahneman 1981, Barberis et al. 2006, Rabin & 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 incomprehen-

[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.

sive. 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.

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.

For policy makers evidence on labeling effects is of particular interest as labeling cash transfers is virtually costless. The 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.

The 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 & 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 & 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 & 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 re-

flected in the empirical approach. I exploit an unanticipated German child benefits reform that affected family types differently. Child benefits are not means-tests, 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. Table 1 shows the composition of child benefits by the number of children. 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. Unobserved common changes over time that constitute confounding variation are eliminated in this approach. As evident from Table 1, a smaller treatment for two-child families materializes during the same period, but it is only modest compared to

[2] Conversion rate was 1.95583 DEM per EUR.

the increases for third children. Therefore, two-child families are discarded in the analysis and only used in the robustness checks.[3]

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 in a difference-in-differences (DD) model.

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

$$\begin{aligned}Y_{st} = &\alpha_0 + \alpha_1 Treated_s + \alpha_2 Post_t + \delta(Treated_s \times Post_t) \\ &+ \beta_1 Inc_{st} + \beta_{11}(Inc_{st} \times Treated_s) + X_{st}\beta_2 + \varepsilon_{st},\end{aligned} \tag{2}$$

[3] Early announcement effects and selection into treatment are very unlikely. The reform was announced only shortly before and there was no tendency to more third births. Also, no other reform affected the groups differentially. More details in the longer working paper version.

where X_{st} are additional control variables and 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). It identifies the effect of child benefits if nothing else changes differentially between the groups conditional on 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). I show results 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 to allow for time-varying differences. All estimations are carried out using ordinary least squares. Standard errors are obtained using Huber/White/sandwich es-

timates 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.[4]

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.[5] The data is a 98 percent sample of the full survey. It includes 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

[4] I also estimate a DD model as a robustness check with a relative common trend assumption similar to Gregg et al. (2009). Details can be found in an online appendix or the long working paper version.

[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.

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. 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 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. Spe-

cial 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 & Tomann 1995, Scholten 2000, Plaut & 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 & 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[6] 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

[6] See survey ”Erben in Deutschland”, Deutsches Institut fr Altersvorsorge, Cologne 2011.

pass their wealth on to their children and two thirds of inheritances will include real estate.[7] 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,[8] 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 & 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 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.[9]

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 du-

[7] See survey based on 1,613 participants conducted by Institut fr Demoskopie Allensbach: ”Postbank Erbschaftsstudie 2012”, Postbank.

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

[9] Reforms of subsidies for HSPs were not differentially affecting the groups. For more information see Börsch-Supan (1994).

ration 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 theatre, 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.[10]

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

[10] 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.

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 income 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. Furthermore, I control for indicator variables of the fed-

eral 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

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. 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 1. Consumption expenditure for education and toys increases considerably more in the treatment group than in the control group. Other differences over time between the groups are moderate.

3 Baseline Results

3.1 Effects on savings

In Table 3 I report results from DD estimations as defined in equation 2 for the treatment effect of the child benefits increase on family savings. Panel A depicts pure DD results, Panel B shows results with income controls and Panel C shows results from

the fully specified model. In column (1) I find 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.

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.[11] 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. As before, the effects on HSPs are statistically significant at least at the five percent level. The treatment effect in the pure DD model 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. However, the point estimates for both measures are negative and the negative value for securities in (-1,386 DEM) exceeds the negative estimate for bank books (-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.[12]

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 4 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

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

[12] A table of results can be found in an online appendix.

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 find 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.

4 Robustness checks

I run a number of robustness checks to test for the validity of the estimates. When exploring the treatment effect on household income, total savings and total consumption, I find positive point estimates on household income and total savings, whereas both are statistically insignificant. Furthermore, I test for an alternative confounding explanation of the savings result. It might be that larger families just invest more into housing. However, I find no evidence for a surge for larger flats by the treated

families. In a different specification of the DD model, I relax the common trend assumption and use a relative common trend instead. This yields consistent results for savings. I also test saving rates as outcomes and find consistent results again. The validity of the estimation strategy is, furthermore, confirmed by a placebo test. Comparing families with one child and couples without children can be interpreted as a placebo treatment exercise, as both are unaffected by the reform. The placebo estimation yields no effect on HSPs, thus confirming the common trend assumption and the estimation strategy.[13]

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

[13] Result tables of all robustness checks can be found in an online appendix and the long working paper version.

welfare of the average child. However, this effect is unlikely to contribute to 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 & 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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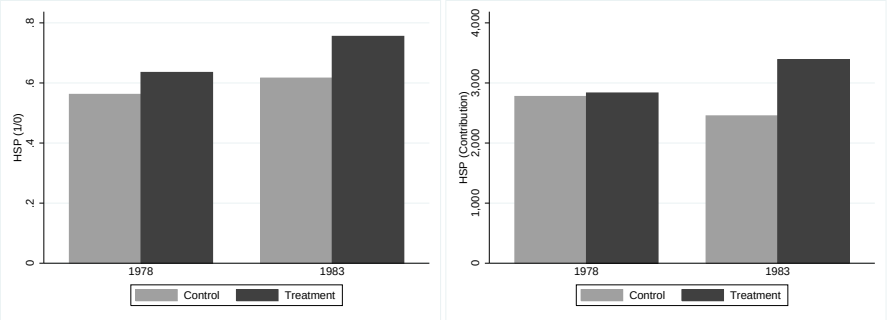
Table 1 Monthly child benefits per child in Deutsche Mark (DEM)

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.

Fig. 1 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 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

Dependent var:	(1) HSP 1/0	(2) HSP Contribution	(3) Bank book 1/0	(4) Bank book Contribution	(5) Securities 1/0	(6) Securities Contribution	(7) Life insurance 1/0	(8) Life insurance Contribution
Panel A								
Treatm. 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)
Treatm. 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 treatm.	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)
Hh income	n	n	n	n	n	n	n	n
Add. controls	n	n	n	n	n	n	n	n
Panel B								
Treatm. 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)
Treatm. 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 treatm.	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)
Hh income	y	y	y	y	y	y	y	y
Add. controls	n	n	n	n	n	n	n	n
Panel C								
Treatm. 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)
Treatm. 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 treatm.	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)
Hh income	y	y	y	y	y	y	y	y
Add. 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 equation 2. 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 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								
Treatm. 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)
Treatm. 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 treatm.	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)
Hh income	n	n	n	n	n	n	n	n
Add. 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 equation 2. 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.