Go Buy Yourself Something Nice! Labeling Effects on Consumption and Savings

Timo Hener *

Draft 1

March 29, 2011

Mental accounts as a psychological phenomenon can induce labeling effects in the decisions of households. In particular, child benefits have a clear "child" label that may lead to a higher marginal propensity to consume child goods than adult goods. To explore the labeling effect, we use a natural experiment of a policy reform that increased child benefits substantially for third children but leaves benefits for families with one child unchanged. Using the 1978 and 1983 income and expenditure survey (EVS) we track how adult and child expenditures respond to the benefit reform. We find no robust indication of a labeling effect for child consumption but large effects for child-specific savings and reductions in adult good consumption.

JEL classification: D12, D13, I38

Keywords:Labeling, child benefits, savings, mental accounting.

*Ifo Institute for Economic Research at the University of Munich, E-mail: hener@ifo.de.

1. Introduction

In many developed countries family support systems build heavily child benefit payments with the primary goal of increasing children's welfare. However, a transfer unconditional on working distorts private labor supply decisions of parents and, thus, might compromise the effectiveness of child benefits. If a behavioral response to child benefits existed that corrected a misallocation of family resources towards children, child benefits could rightly defend their position as one of the biggest family policy instruments. Labeling effects of child benefits as in the mental accounting framework by Thaler (1990) are capable of explaining why a higher share of labeled child benefit incomes could increase the share spent on child goods. Parents could also be morally obliged by a transfer called "child" benefit to spend more of their resources on their children, who can suffer from under-investment for a number of reasons. To answer the question whether there are labeling effects of child benefits the literature usually uses assignable child good consumption as the dependent variable. We add to that literature by exploiting a natural experiment of a child benefit reform in difference-in-differences estimations and by additionally analyzing child-specific savings as an important long-term investment in children.

Rational agents in standard microeconomic theory spend their income as to maximize utility irrespective of the sources. There are no compositional effects of income sources on demand decisions as income is assumed to be fungible, i.e., all marginal propensities to consume from different income sources are the same. This framework often fails to correctly predict actual behavior as Tversky and Kahneman (1981) and Thaler (1980) show in their seminal papers on prospect theory and bounded rationality. Allowing labeling effects Thaler (1990) introduces the concept of mental accounts that describe the composition of income. In that framework, the fraction of child benefits in total income positively influences the expenditure for child goods, total income held constant. Precisely, the marginal propensity to consume child goods out of child benefit income ($\frac{dQ}{dCB}$) is larger than the marginal propensity to consume child goods out of any other income ($\frac{dQ}{dY}$), so

$$\frac{dQ}{dCB} > \frac{dQ}{dY}, \tag{1}$$

where Q are child expenditures, CB is child benefit income, and Y denotes any other income.

Kooreman (2000) evaluates the labeling effect of Dutch child benefits by using repeated

cross-sections and variations of child benefits over time for different age groups of children to identify marginal propensities to consume child goods. He finds that marginal propensities to consume child goods are larger for child benefits than for other income. In contrast, Edmonds (2002) finds no such effects with Slovenian data, whereas Blow *et al.* (2007) even find negative effects and higher expenditure on adult goods from unanticipated benefits. In a laboratory experiment Abeler and Marklein (2008) can show that labeling effects influence consumption decisions.

We exploit a natural experiment of a child benefit reform that allows the use of a control group to exclude most of the confounding variation that can disturb identification in multivariate regressions. We use two waves of the German expenditure and income survey from 1978 and 1983, a period when child benefits for third children were expanded while child benefits for first children remained unchanged (see table 2). The treatment group in the empirical analysis consists of families that were affected by the increase in child benefits and the control group includes non-affected families. We compare expenditures on the assignable child goods toys, child-specific savings, and adult goods between the groups in standard difference-in-differences models and in modified difference-in-relative-differences estimations, and run a number of robustness checks to account for possible biases.

We find that consumption of child goods is largely unaffected by the labeling of child benefits. Expenditure for pure adult goods is significantly decreased. Child-specific savings become more likely and are higher after increasing the fraction of child benefits in total household income, so that we draw the conclusion that labeling effects of child benefits mainly materialize for savings.

2. Data and the reform background

We are restricted to two waves, 1978 and 1983, of the German income and expenditure Survey EVS for our empirical analysis due to data availability. The data source collects a complete set of expenditure variables at the household level by various groups of goods, among them toys, which can be assigned to children with high confidence. We also observe expenditures for clothes, which is reported as male and female clothes without a distinction between child and adult clothes in 1978, whereas in 1983, we have separate figures for male and female children and adults. As such, we have to modify our treatment

and control group design as discussed in the following empirical strategy section to cope with the missing distinction and assign them to the parents as adult goods. We also observe whether a household has a housing savings contract and how much it is worth. Housing savings are a common device to save for children in Germany. Virtually all major providers offer special housing savings contracts for parents, who want to save for their children. They advertise that these savings can be used not only for housing but also for education expenses at low risks. We thus assume that housing savings capture assignable savings for children. Luxury goods are considered as assignable adult goods, including jewelry, leather goods, and watches.[1]

Table 1: Means of the included variables for different family sizes

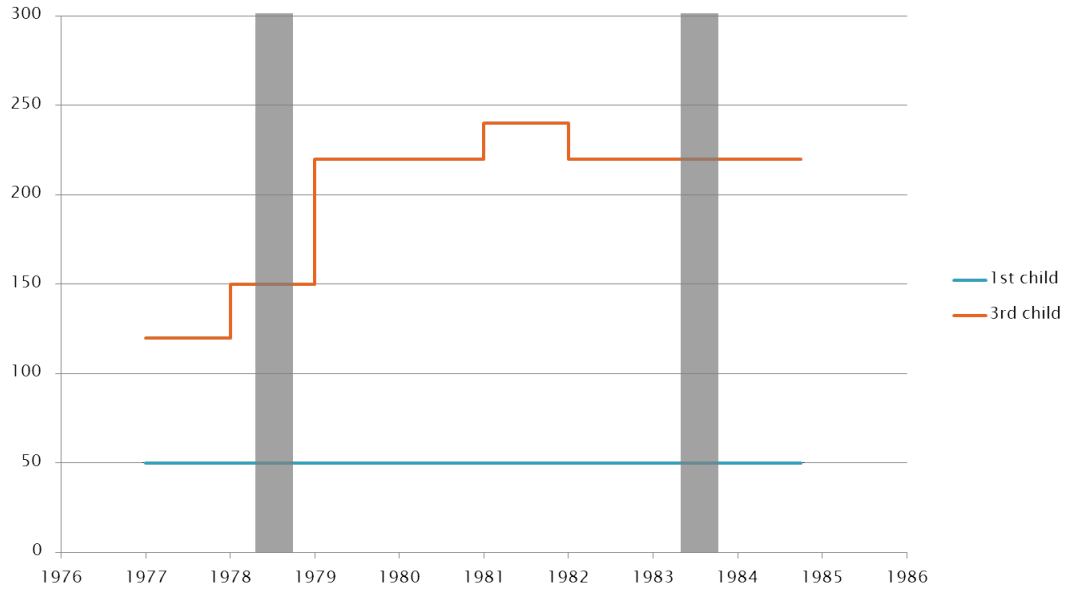
	1 Child 1978	1 child 1983	3 children 1978	3 children 1983
Toys	21.71	21.18	28.74	35.42
Luxury	196.04	227.92	181.03	168.09
Male clothes	1081.55	944.56	1196.34	1064.39
Female clothes	1793.35	1482.07	1925.49	1524.68
Housing-saver	0.59	0.65	0.63	0.75
Housing-savings	11715.72	12026.01	10982.89	13590.84
Age father	38.69	38.05	40.09	38.75
Age mother	35.51	34.97	36.90	35.50
Avrg. child age	8.17	7.61	9.69	8.20
Hh-net-income	64183.45	64452.87	71857.72	73889.47
Fed1	0.03	0.03	0.01	0.02
Fed2	0.11	0.08	0.11	0.08
Fed3	0.02	0.01	0.02	0.01
Fed4	0.26	0.23	0.25	0.22
Fed5	0.08	0.10	0.10	0.08
Fed6	0.08	0.08	0.07	0.09
Fed7	0.13	0.14	0.15	0.16
Fed8	0.16	0.19	0.19	0.21
Fed9	0.02	0.02	0.02	0.02
Fed10	0.06	0.05	0.03	0.03
N	6088	5629	1859	1455

The data also contain information about household net income, expenditures on durable goods, and socio-economic characteristics as the age of all household members, the num-

[1]The variables are differently coded in 1978 and 1983 and can therefore include slightly other bag-maker products in 1983.

ber of children, and regional information that allows to control for various background variables. Means of important variables are reported in table 1 for one- and three-child-families. Fed1 to Fed10 indicate the fractions in the ten federal states (Bundeslaender), all monetary values are in DM and deflated by CPI with base year 1995. The means of the dependent variables are different between the groups, which is accounted for in the empirical analysis.

Figure 1: The natural experiment - Child benefits



Child benefits are paid monthly in Germany as a direct transfer to the households. There is differentiation according to the rank of the child, as can be seen in table 2. Within the surveyed period, 1978 to 1983, there was a huge increase in child benefits for third children by about 50 percent and a modest increase for second children. In contrast, child benefits for first children remained constant. This is our natural experiment to build the treatment and control group as depicted in figure 1.

Clearly, there are more family benefits that may affect expenditures and behavior of families. One of the biggest features is child allowances in the income tax system. In the period in question they do not disturb the analysis. Child allowances were not present during that time and were introduced again in 1983 at a very moderate level. This does not affect our results, as allowances become effective after tax return in the end of a year and our last observation appears in 1983.

Table 2: Monthly child benefits per child in Deutsche mark (DM)

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	78	150	150	150
01-01-79	30-06-79	50	78	220	220	220
01-07-79	31-01-81	50	100	220	220	220
01-02-81	31-12-81	50	120	240	240	240
01-01-82	30-06-90	50	100	220	240	240

That time there was a government program which was intended to encourage housing savings by adding a fraction to the money saved. Given eligibility 17 percent were added to the money in housing savings contracts. In 1982 the fraction was reduced to 14 percent. This is not a problem to our identification as it touches both the treatment and the control group equally. Eligibility criteria from income limits and premiums for children where not affected by the reform.

3. Empirical analysis

We seek to analyze the effect of an increase in child benefits on consumption and savings patterns of families. If we assume the increase in benefits after 1978 for third children is unanticipated, it comes as an exogenous shock to the families' income patterns. Consequently, we could run regression of expenditures on child benefits using a sample of families with three children and attribute changes in the expenditures to the exogenous increase in benefits. This approach would not be sufficient to interpret results as causal evidence. As the only variation stems from differences between two points in time, 1978 and 1983, all kinds of underlying trends and simultaneous events could be reasons for correlations with the outcome. Imagine innovations in fashion or new insights in the educational value of toys that lead to higher expenditure for these goods. If these events occur simultaneously to the benefit increase, we would not be able to distinguish the effects. Confounding variation that is irrespective of the benefit reform would yield non-causal correlations.

To account for unobserved effects over time we apply a difference-in-difference (DD) estimator. Besides the treated families with three children we use families with one child

which have not been affected by the reform as a control group. The treatment effect then is the second difference between the treatment and control group after differencing over time. By that we eliminate all sources of unobserved trends and simultaneous events, or unobserved time-variant effects on expenditures, that are common to both family types. The identifying assumption then is no group specific effects on the outcome variable occur simultaneously to the increase in benefits, i.e., all unobserved changes are valid for both family types.

We use the regression form of the DD estimator

$$Y_{st} = \alpha_0 + \alpha_1 Treated + \alpha_2 Period + \delta Treated \times Period + \beta X_{st} + \epsilon_{st}, \qquad (2)$$

on the various expenditure data Y_{st} in group s and period t, where $Treated$ is the treatment group dummy, $Period$ the common time trend, and δ the estimate of the difference-in-difference effect, which is an interaction of $Treated$ and $Period$. $\alpha_{(.)}$ and β are the different estimates.

There is some uncertainty to the assigning of the treatment group, though. We observe whether a household has one, two, three and so forth children, which means that as children get older there might be less children left in the household than are eligible. Children have to be in education to be eligible and are thus less likely to live outside the house. We reduce this friction by excluding households with children over 16 years of age and concentrating on household with one or three children. There could still be false group assignments if one child has moved out and we observe just a one child family or a three child family. The former case makes the control group more similar in terms of child benefits and thus would downsize our estimates. The latter case would induce an upward bias to our estimate as the child benefits for second, third and fourth children sum up to more than with one order less. If one keeps in mind that the estimates could partly reflect even higher benefits than for third children, the conclusions are still valid with the limitation that we cannot make a statement about the relative effectiveness of any DM spent for benefits.

In later chapters we deal with compositional changes of the groups by adding socio-economic covariates, with the age composition of children by adding different dummy constructs, with heterogenous effects of income by adding interactions and regressions of expenditure shares of total income, with self-selection into treatment by keeping only

non-selectors in the sample, and with relative price changes by applying a difference-in-relative-difference estimator. Particular assumptions are discussed in detail at place.

The benefit reform increases income to some extend and thus can have pure income effects on expenditures. As the goods considered are unlikely to be inferior we would expect positive income effects only. Labeling effects that attach higher marginal propensities to consume child goods would increase expenditure on child goods. They could even induce negative effects on adult goods if parents are motivated to change their overall consumption pattern towards child goods and cutting back on their own consumption.

Table 3 shows the results of a simple DD estimation for different consumption and savings expenditures. The DD coefficient for the $Treated \times Period$ term is the marginal effect of the increase in expenditures under the identifying assumption that no group specific change to the outcome variable occurred. The treatment and control groups for parent's clothes are different, because we can only observe male and female clothes expenditures. To interpret them as adult goods, we use only households without girl children for father's clothes and without boy children for mother's clothes. Male and female clothes expenditure then is not partly child expenditure but assigned to one of the parents.

Table 3: Plain DD regression

	Toys	Housing-saver	Housing-savings	Luxury goods	Father's clothes	Mother's clothes
Treated×	7.213***	0.066***	2,297.650***	-44.822*	26.755	-129.744
Period	(2.326)	(0.019)	(836.904)	(26.420)	(72.638)	(109.135)
Treated	7.031***	0.043***	-732.822	-15.008	-134.035***	-37.973
	(1.557)	(0.013)	(560.272)	(17.685)	(47.996)	(72.047)
Period	-0.537	0.058***	310.295	31.878***	-154.142***	-339.366***
	(1.086)	(0.009)	(390.877)	(12.341)	(18.982)	(29.925)
Constant	21.714***	0.588***	11,715.717***	196.043***	914.193***	1,539.288***
	(0.753)	(0.006)	(270.924)	(8.554)	(13.180)	(20.706)
Age composition	No	No	No	No	No	No
Income, interacted	No	No	No	No	No	No
Add. controls	No	No	No	No	No	No
Observations	15031	15031	15030	15031	6156	6483
R-squared	0.006	0.010	0.001	0.001	0.013	0.023

Notes: Standard errors in parentheses,*** p<0.01,** p<0.05,* p<0.1.

Expenditure on toys of families with three children is increased by 7.21 DM compared to the control group constituted of families with one child. This is a substantial effect given the constant of 21.71 DM and the 7.03 DM higher average of the treatment group. An interpretation of the constant is the average expenditure of the control group before reform. Expenditures are not increased over time by pure time trends.

In the savings equations we find positive and significant effects of the child benefit increase. 6.6 percent more families have a housing savings contract due to the reform and their value increases by 2297 DM. A back-of-the-envelope calculation also underlines the economic significance of the result. Summing up the additional child benefits compared to the base year 1978 for the consecutive four years, disregarding variation in interviewing months, yields 1144 DM for the second child and 4420 DM for the third child. About forty percent of the total additional benefit is added to housing savings contracts on average. The baseline differences for families with three children are a 4 percentage points higher probability to own a housing savings account prior to the reform, but no statistically significant difference in the value of the contract. Both the probability and the value show a substantial increase over time irrespective of the benefit reform.

Assignable adult goods in our estimation are luxury goods, father's clothes and mother's clothes. Expenditures on luxury goods are reduced by 44.82 DM due to the reform. A possible interpretation is that positive income effects are overcompensated by a shift away from adult consumption towards child goods in response to the increase in child related transfers. The treatment group has somewhat lower average expenditures on these items and there is a sizable, significantly positive time trend. Clothes expenditures of father and mother are not significantly affected by the reform, the coefficient for mother's clothes is large and negative.

3.1. Robustness analysis

In the DD framework we have eliminated confounding variation that is common to both family types with three children and one child. Only changes that affect the groups differently and at the same time as the benefit change would cause problems to that identification. One such variation that could induce this identification problem is the age composition of the children in the two groups. Unstable fertility patterns in the early 1970s likely yield changes in the children's age in the cross sections of 1978 and 1983. And possibly families with three children respond differently to changes in their children's age than one child families. Therefore, we construct dummies for the child age in the family. As the econometric equation is unidentifiable if an exclusive variable of the treatment group enters, what would be the case with age dummies for second and third children, we have to use dummy sets that identify the equation under different assumptions. Expenditures can be affected by the child's age and obviously conditional

on having three children compared to one. We use dummies for the age of the oldest child, 16 years as the omitted category, to account for different age composition post reform and interact age dummies with the treatment group dummy. Thus, we assume that the effect of child age on consumption patterns, which differs for the treatment group, is reflected in differential effects of the oldest child's age. For consumption of child goods the two younger children could reuse some of the goods and we assume that the age of the oldest child interacted with treatment reflects the differential effect. Using the older children is straightforward in the sense that first time purchases have the largest impact on consumption patterns.

Table 4 reports DD estimates with controls for age dummies and its interactions. The results from the previous section largely hold. Marginal effects are reduced to some extent, especially estimates for the effects on the child good toys are down by one third. Different age compositions can thus explain some of the variation in expenditures, while the benefit reform still seems to be responsible for most of the effect. With interactions of the treatment with other variables the main effect of the treatment dummy is no longer easily interpretable as the initial difference between the treatment and control group.

Table 4: DD regression with child age controls

	Toys	Housing-saver	Housing-savings	Luxury goods	Father's clothes	Mother's clothes
Treated×	4.538*	0.061***	1,985.599**	-48.155*	35.284	-122.441
Period	(2.343)	(0.019)	(847.272)	(26.851)	(74.500)	(110.668)
Treated	7.509**	0.011	-2,352.977**	-43.328	-173.598*	-169.888
	(3.281)	(0.027)	(1,186.194)	(37.595)	(103.447)	(152.644)
Period	-0.887	0.052***	86.970	37.814***	-141.254***	-314.791***
	(1.088)	(0.009)	(393.464)	(12.470)	(19.147)	(30.135)
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Age composition	Yes	Yes	Yes	Yes	Yes	Yes
Income, interacted	No	No	No	No	No	No
Add. controls	No	No	No	No	No	No
Observations	15031	15031	15030	15031	6156	6483
R-squared	0.029	0.027	0.014	0.007	0.025	0.040

An alternative dummy construction involves the age of all children in the household. To achieve that, the dummies of children of different order are stacked in one dummy variable. A family with a one- and a four-year-old then carries the age dummy for one and four years equal to unity and the remaining equal to zero. The assumption then is that expenditures are only dependent on the age of a child but not on the birth order. Results are very similar to the ones reported in table 4. Tables are available on request.

Another source of confounding variation could stem from household income changes. Income could increase differently in the two groups. Furthermore, the effects of income on expenditure could be different for the treatment group. Moreover, child benefit changes induce effects also through income changes. In that sense household income is not an exogenous variable. Nevertheless, controlling for income excludes this channel from the reform effect. We use full household income including child benefits to interpret results, because the effect of child labeling as additional variation induced by the child benefit increase then is only due to higher shares of benefits in income. Only different marginal propensities to consume can account for differences in expenditure.

We report results with added controls of income and its interaction in table 5. The estimate for toys becomes insignificantly different from zero. Group specific income changes seem to be responsible for the higher consumption of the treatment group post reform. The savings equations still report significant positive effects of the reform and luxury goods consumption is reduced. In contrast to earlier estimates, the negative effect on mother's clothes expenditure becomes more negative and is, almost statistically significantly by normal standards, reduced due to the reform. The larger negative effect is plausible if we assume that the pure income effect of higher child benefits points in a positive direction. Excluding that channel reveals the negative labeling effect that the reform has on adult clothes consumption.

Table 5: DD regression additionally controlling for income

	Toys	Housing-saver	Housing-savings	Luxury goods	Father's clothes	Mother's clothes
Treated×	3.569	0.056***	1,621.750**	-59.963**	39.494	-156.373
Period	(2.332)	(0.019)	(818.476)	(26.436)	(70.090)	(102.557)
Treated	-6.589	0.050	-432.138	-109.591**	127.306	0.000
	(4.437)	(0.036)	(1,557.135)	(50.298)	(131.788)	(0.000)
Period	-1.056	0.049***	-114.003	33.748***	-150.220***	-329.090***
	(1.082)	(0.009)	(379.704)	(12.265)	(18.014)	(27.906)
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Age composition	Yes	Yes	Yes	Yes	Yes	Yes
Income, interacted	Yes	Yes	Yes	Yes	Yes	Yes
Add. controls	No	No	No	No	No	No
Observations	15031	15031	15030	15031	6156	6483
R-squared	0.041	0.059	0.083	0.040	0.137	0.177

Other variables correlated with the outcomes could be changed differently between the groups from prior to post reform observations. Such compositional effects are not controlled for in the main effect of the treatment dummy, which indicates the initial difference between the control and the treatment group. Therefore, we include some more

background characteristics of the two groups that could violate identifying assumptions. We include age of the parents as they could have different consumption pattern over the life cycle if children are not born at the same age of the parents in all cases. An earning mother could have higher bargaining power and shift consumption patterns due to different preferences of the wife. As female participation changed substantially that time, we include a dummy for female earners in case they affect the treatment and control group differently. The amount spent on durable goods in the particular year controls for seldom large investments that could deter usual consumption. Further control variables are income squared, and dummies for the federal state as well for the urbanization level.

Table 6: DD regression additionally controlling for background characteristics

	Toys	Housing-saver	Housing-savings	Luxury goods	Father's clothes	Mother's clothes
Treated×	3.619	0.041**	1,444.826*	-60.513**	39.654	-136.444
Period	(2.339)	(0.018)	(814.457)	(26.426)	(69.619)	(102.094)
Treated	0.000	0.000	0.000	0.000	117.006	-522.749***
	(0.000)	(0.000)	(0.000)	(0.000)	(131.075)	(194.869)
Period	-1.961	0.083***	-428.544	43.359***	-147.561***	-312.572***
	(1.194)	(0.009)	(415.734)	(13.490)	(19.990)	(31.004)
Constant	Yes	Yes	Yes	Yes	Yes	Yes
Age composition	Yes	Yes	Yes	Yes	Yes	Yes
Income, interacted	Yes	Yes	Yes	Yes	Yes	Yes
Add. controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15031	15031	15030	15031	6156	6483
R-squared	0.046	0.130	0.102	0.052	0.160	0.196

Notes: Standard errors in parentheses,*** p<0.01,** p<0.05,* p<0.1. The set of additional control variables includes age of the parents, household net income squared, the number of earners, dummy variables for the regional urbanization level, the amount spent on durable goods in the particular year, and dummy variables for the federal states.

Results with all additional controls are reported in table 6. Earlier results for savings and adult goods are robust to adding more control variables that could have changed the groups' compositions. We still find positive effects for child savings and negative effects for adult consumption.

The results hold also when excluding all families with children under the age of five that could be self-selected into the treatment group as a response to the higher child benefits for higher order children. The selection then is mirrored if we only consider families that did not react by fertility responses to the reform. Tables are omitted and available on request.

3.2. Placebo tests

We cannot make usual placebo tests of our treatment by pushing the treatment period back in time. Data is only available in a 5 year sequence and child benefits changed before 1978, too. However, we can estimate our model for consumption and purchases, which would rather be seen as public household goods without knowing individual preferences of children or adults. Although, some of them are capable of increasing the family's welfare and are of interest to policy makers. As for the savings we look at savings books and life insurance which have no special purpose and if so, in the case of life insurance more likely as an insurance for the partner against income default. Additional consumption considered here is food. First, there is overall food consumption which could have positive health effects for all family members including kids if larger purchases were about to increase quality. Second, we observe luxury foodstuffs, which is rather not health enhancing. Third, we observe food consumption out of the house which is rather an adult good, but children can also have a share in it as e.g. in school lunch. Moreover, we consider housing arrangements. First, we regress on the probability to rent a flat or a house in contrast to owning it. If there is an effect on the probability to own our housing savings equations could be biased as the contracts could have been eliminated to purchase houses. Second, we use the size of the flat in square meters as a dependent variable. Flat size could be a welfare indication for all family members.

Table 7 reports the results for alternative dependent variables. Savings that are not attributable to a particular child purpose as in savings books and life insurances are not affected by the reform. Neither the probability to hold an account nor the value or deposits undertaken shows a significant effect. Overall food consumption is reported to be significantly negatively affected by the treatment. However, there are large differences in the baseline food consumption between the groups, much larger differences than for any other good considered. Moreover, food expenditure does not necessarily tell anything about the quality of food and who in the household profits. Luxury foodstuffs and meals out of the house are not significantly affected. The probability of renting a house or flat is not affected either, which is encouraging to the housing savings interpretation as an investment for children. The size of the home does not robustly show a significant effect, although there seems to be a small positive change without any controls.

Table 7: More outcome variables and placebos

	Savings book	value	Life insurance	deposit	Food	Luxury foodstuffs	Restaurant meals	Renting	Square meters
no controls									
DD effect	-0.003	-524.569	0.009	30.231	-42.940***	5.751	-1.592	-0.010	2.854**
	(0.008)	(680.051)	(0.014)	(107.614)	(9.563)	(6.019)	(10.489)	(0.019)	(1.289)
Constant	No	No	No	No	No	No	No	No	No
Age composition	No	No	No	No	No	No	No	No	No
Income, interacted	No	No	No	No	No	No	No	No	No
Add. controls	No	No	No	No	No	No	No	No	No
full model									
DD effect	-0.009	-677.031	0.005	-102.208	-31.761***	4.856	-6.128	0.009	0.120
	(0.008)	(677.878)	(0.014)	(102.031)	(9.238)	(6.090)	(10.440)	(0.017)	(1.110)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age composition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income, interacted	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Add. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Figure 2: Counterfactuals of DD and DRD models

(a) DD

(b) DRD

3.3. Difference-in-relative-differences estimation approach

A different kind of concern involves the general assumption of the DD estimation. The DD approach tests deviations from the control-group trend in absolute terms. This approach eliminates two sources of confounding variation—a common trend that occurs in the absence of a treatment and a common shift in the outcome variable that occurs simultaneously to the treatment. However, one might be interested in the effect on relative deviations instead if there is reason to believe that a proportional common change for both groups coincides. This can e.g. be the case for relative price changes of the dependent variable different from overall inflation. If the treatment and control group have different levels of consumption, a proportional measure like the price would have a proportional effect on the outcome instead of a constant markup. More generally, imagine a control group person buys one suit and a treatment group person buys two suits prior to the treatment, what should be the correct counterfactual if the control group person buys two suits post treatment? The unobserved shift we want to eliminate could be that people just buy one more suit that season, which makes the counterfactual three suits, or that people double their suit collection, which makes the counterfactual four. A DD model in relative terms, hereafter named difference-in-relative-differences (DRD) model, which captures the relative changes in the outcome variable, captures the latter kind of change.

In the DRD model the common trend assumption is altered. The counterfactual case—the dependent variable's change of treatment group without the treatment—is no longer an equal level change compared to the control group but a relatively equal change in the dependent variable. This means that common trends are weighted by the observed

level. In the standard DD we assume that for the counterfactual a one unit change in the dependent variable of the control group must equal a one unit change in that variable of the treatment group. In contrast, in the DRD a one percentage point change in the control group's dependent variable must equal a one percentage point change in the treatment group's dependent variable for the counterfactual case. In extreme cases this altered assumption can even switch the sign of the treatment effect as depicted in figure 2.

With simple modifications to the original DD model we can estimate deviations in relative terms from the control group with

$$\frac{\alpha_2 + \delta}{\alpha_0 + \alpha_1} - \frac{\alpha_2}{\alpha_0}. \tag{3}$$

See the Appendix for a prove.

The results from the plain DD estimation also hold as estimates from a DRD estimation. The effects show the same sign and are significant for toys, savings, and luxury goods. Unobserved relative price changes thus do not seem to cause threats to identification.

4. Conclusion

The labeling effect of child benefits does not materialize in child good consumption but in savings for children and we find indication of abdication of adult consumption. Instead of short-lived goods, which may not be in insufficient supply in the families, parents rather invest in their children through long-term savings in response to the labeling of child benefits. For family policy an important lesson can be learned from considering savings as an objective that may be equally beneficial to children as the consumption researchers usually have in mind.

References

Abeler, J. and Marklein, F. (2008). *Fungibility, Labels, and Consumption*. IZA Discussion Papers 3500, Institute for the Study of Labor (IZA).

Angrist, J. and Pischke, J. (2009). *Mostly harmless econometrics: an empiricist's companion*. Princeton Univ Press.

Blow, L., Walker, I. and Zhu, Y. (2007). *Who Benefits from Child Benefit?* Working Papers 200716, Geary Institute, University College Dublin.

Edmonds, E. (2002). Reconsidering the labeling effect for child benefits: evidence from a transition economy. *Economics Letters*, **76** (3), 303–309.

Kooreman, P. (2000). The labeling effect of a child benefit system. *American Economic Review*, **90** (3), 571–583.

Lyssiotou, P. (2009). Are child benefits fungible?:evidence from a natural policy. *EALE conference paper*.

Thaler, R. (1980). Toward a positive theory of consumer choice. *Journal of Economic Behavior & Organization*, **1** (1), 39–60.

— (1990). Anomalies: Saving, fungibility, and mental accounts. *Journal of Economic Perspectives*, pp. 193–205.

Tversky, A. and Kahneman, D. (1981). The framing of decisions and the psychology of choice. *Science*, **211** (4481), 453.

A. Prove of DRD estimator

The DRD is derived as follows, notation as in Angrist and Pischke (2009).

The control group's relative trend is described by

$$\frac{E[Y_{ist}|s=C,t=1983]-E[Y_{ist}|s=C,t=1978]}{E[Y_{ist}|s=C,t=1978]}, \tag{4}$$

which can be written in terms of standard regressors, such that

$$\frac{(\gamma_C+\lambda_{1983}+\delta(0))-(\gamma_C+\lambda_{1978})}{\gamma_C+\lambda_{1978}}. \tag{5}$$

This reduces to

$$\frac{\lambda_{1983}-\lambda_{1978}}{\gamma_C+\lambda_{1978}}. \tag{6}$$

The same exercise for the treatment group yields the following three equations.

$$\frac{E[Y_{ist}|s=T,t=1983]-E[Y_{ist}|s=T,t=1978]}{E[Y_{ist}|s=T,t=1978]}, \tag{7}$$

and

$$\frac{(\gamma_T+\lambda_{1983}+\delta(1))-(\gamma_T+\lambda_{1978})}{\gamma_T+\lambda_{1978}}, \tag{8}$$

and

$$\frac{\lambda_{1983}-\lambda_{1978}+\delta}{\gamma_T+\lambda_{1978}}. \tag{9}$$

The population DRD then is

$$\frac{\lambda_{1983}-\lambda_{1978}+\delta}{\gamma_T+\lambda_{1978}}-\frac{\lambda_{1983}-\lambda_{1978}}{\gamma_C+\lambda_{1978}}. \tag{10}$$

When we use the standard population diff-in-diff regression

$$Y_{ist}=\alpha+\gamma_s+\lambda_t+\delta D_{st}+\epsilon st \tag{11}$$

the regressors express

$$\begin{aligned}\alpha =&\gamma_C + \lambda_{1978}\\ \gamma =&\gamma_T - \gamma_C\\ \lambda =&\lambda_{1983} - \lambda_{1978}.\end{aligned} \tag{12}$$

Using the regressors' notations we can simplify the population DRD equation as

$$\frac{\lambda + \delta}{\gamma_T + \alpha - \gamma_C} - \frac{\lambda}{\alpha} \tag{13}$$

and equally so as

$$\frac{\lambda + \delta}{\alpha + \gamma} - \frac{\lambda}{\alpha}. \tag{14}$$

In the notation of our DD model in equation 2 the DRD estimator is represented by

$$\frac{\alpha_2 + \delta}{\alpha_0 + \alpha_1} - \frac{\alpha_2}{\alpha_0}. \tag{15}$$